Essays in Banking and Crime:

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Abstract/Foreword

This dissertation consists of two essays which explore the interface between retail banks and organized criminality. In the first of these, "Dark Banking? Banks and Illicit Financial Flows from the Mexican Drug Trade," I explore why banks provide financial services to organized-crime syndicates. I also ask whether there is a role for regulation in insulating finance from criminal activity. I address these questions using evidence from the drug trade in Mexico, finding that local drug cartel activity causes an increase in bank deposits, and branch networks grow in affected areas. After the election of a "law-and-order" government, these effects dissipate, with liquidity flowing into branches of U.S. banks along the border.

In the second essay, "Bank Branch Networks, Banking Relationships, and Organized Crime," I explore if banks develop relationships with criminal organizations, exploiting spatial variation in cartel activity, again using Mexico as an empirical laboratory. I test whether banks with prior exposure to criminal activity are more likely to enter areas where cartels operate, as well as whether previous exposure to specific cartels predicts entry into banking markets where said cartels have entered. Results suggest that certain banks do establish these relationships. Bank characteristics that have significant effects on differential behavior regarding collusion with organized criminal organizations are domestic majority equity ownership and bank size.

JEL Codes: G210, K42

Keywords: Banks, Deposit, Crime, Financial Crime, Liquidity, Money Laundering, Organized Crime

A Personal Note

This dissertation is dedicated to my wife Abril. She has seen me through these years by being a ready support both financial and emotional, and truly this work belongs as much to her effort as it does to mine –although she would've undoubtedly done a better job had she written it. I am excited to be able to finally be able to put to work the human capital I have gained through my doctoral studies and give back to her –and our baby daughter, about to be born!

Also worthy of great thanks are my parents, Álvaro Alberto, and Bertha Elizabeth. It has not been easy to see you ail through these past two years, but hopefully I can ease your burdens through my efforts, as you have eased mine through yours. I love you.

My brother and best friend, Abraham, also deserves a special mention. The discussions and conversations we have had over the years have made me not only a better scholar, but a better man.

Last, but not least, my committee deserves all my gratitude. Phil Strahan's reputation precedes him but does not do justice to his talent and dedication both as a researcher and a mentor. Phil, I hope to embarrass you as little as possible in my career; I will strive to improve always as a researcher. Nadya Malenko is not only a brilliant mind, but a brilliant human being. Nadya, thanks for always being available, and for being a friend. Edie Hotchkiss has a penetrating intellect, and every time she speaks –not often—her words are full of wit, pith, and good advice. Thanks, Edie, for your time and patience.

May Providence ever shine on all of you. Thank you again, from the bottom of my heart.

SDG/AMDG/JJ

Dark Banking?

Banks and Illicit Financial Flows from the Mexican Drug Trade

Abstract

Do banks enable crime? Does regulation insulate finance from criminal activity? I address these questions using evidence from the drug trade in Mexico, finding that local drug cartel activity causes an increase in bank deposits. Accordingly, branch networks grow in affected areas; this growth is not driven by increased lending opportunities. After the election of a "law-and-order" government, these effects dissipate, with liquidity flowing into branches of U.S. banks along the border. I interpret this as evidence that "finance follows crime" in weak institutional environments, and that, absent transnational policy coordination, regulatory arbitrage via cross-border liquidity flows undermines banking regulation.

1 Introduction

Mexican drug cartels are the largest foreign suppliers of heroin, methamphetamines, and cocaine to the U.S. market. In consequence, these cartels hold a significant market share of the worldwide illicit drug trade, which has been estimated at 1.5% of global GDP (UNODC, 2011), and has the U.S. as its primary demand market. Apart from welfare considerations, these figures are enough to render the U.S.-Mexico cross-border drug trade of significant economic importance. However, we know little about the financial operations of this illicit industry. In truth, the finance literature is generally all but silent on phenomena like money laundering and the transmission of illicit financial flows.

Do banks enable the transmission of illegal liquidity, or serve as an essential tool in the financial management of illicit "firms?" Alternatively, do they passively profit from illicit cash flows? Do regulation and regulatory enforcement succeed in insulating financial intermediation from criminal activity? Answering these questions is of key importance from the point of view of financial stability.

In this paper, I shed light on these issues using the illegal drug trade in Mexico as an empirical laboratory. I find that –prior to 2006—cartel entry into Mexican municipalities causes a steep increase in bank deposits held locally. Accordingly, bank branch networks grow in areas with organized-crime presence, consistent with banks possessing private information regarding the financial dealings of criminal enterprises. However, importantly, lending drops in these localities, implying these liquidity windfalls were *not* used to expand local credit supply. After the election of a Federal administration in Mexico that cracks down on organized crime in 2006, these effects dissipate, and liquidity flows into branches of U.S. banks located along the Mexican border.

These deposits are not only concentrated in the border region, but also appear to "mirror" cartel activity in Mexico: both distance to the border and distance to the geographic center of activity of drug cartels in Mexico predict deposits volume after 2006. Further, I find evidence that is consistent with banks trading off benefits of taking on this surge in business volume with its risks: deposits volume in border counties predicts enforcement actions at the bank level, but only after 2006. Lastly, I find that government actions against drug cartels engender political backlash in localities that benefit from liquidity windfalls: the 2006 level of deposits predicts the likelihood of incumbent loss in subsequent local elections in Mexican municipalities with organized-crime presence.

These results suggest that heightened law-enforcement and anti-money laundering (AML) actions succeeded in driving away illicit cash flows from the Mexican financial system. However, they also suggest that these efforts failed in financially handicapping crime syndicates, who were able to shift their mode of financial operation to avoid these controls, and thus potential prosecution. These findings also have implications for policy: evidence suggests that increased regulatory stringency against criminal finance fails on two fronts. First, it is rendered moot by regulatory arbitrage (in the form of liquidity exports) given an absence of transnational coordination in banking regulation. Second, the political response it provokes may render it untenable as long-run policy.

Mexico is an ideal laboratory to study interactions between banks and organized crime: it is an economy with a large and developed banking system, and it has seen a steep rise in drug trafficking activity in the past decades. Moreover, government crackdown on this activity has led to temporal variation in the ease of doing business for drug cartels operating in this country. A complementary empirical setting in this investigation is the American side of the U.S.-Mexico border. The border region is economically integrated, with large volumes of goods and capital flowing in both directions; further, economic conditions in Mexico have been shown to affect the regional economies of U.S. border cities (Hanson, 1996).

There is significant circumstantial evidence regarding the dealings of Mexican drug cartels with banks. For instance, in 2012 the U.S. Department of Justice (DOJ) levied a fine of $1.9 billion from HSBC in the wake of a highly public money laundering scandal (Protess & Silver-Greenberg, 2012). This fine came in response to the alleged laundering by HSBC Mexico of around $880M for both the *Sinaloa* (Mexico) and *Norte Del Valle* (Colombia) drug cartels.[1] Although at the time the fine was a record sum for monetary damages garnished from a bank, it corresponded to only slightly over a month's profits for HSBC Global Holdings. (*BBC*, 2012). This penalty came only months after the Senate Permanent Subcommittee on Investigations issued a report documenting the systematic breach of AML policies by HSBC's U.S. subsidiary, which at the time processed around $7 billion of transfers from its Mexican counterpart annually (U.S. Senate, 2012).[2]

Are the above anecdotes surprising? Perhaps, as great efforts have been made since the 1980s to place safeguards on the financial systems of both the U.S. and Mexico to foil money laundering and illicit financial flows. In the U.S., ten different federal agencies are charged with the enforcement of eight federal statutes governing know-your-customer requirements and AML.[3] The cost of AML compliance has been estimated at $25.3 billion annual for US banks (*LexisNexis* Risk Solutions, 2018). Even in Mexico, an environment of relative institutional weakness, AML requirements cost financial firms 3-8% of net income (Quinn, 2013). Despite these exacting rules, money laundering remains massive in scale, in both

[1] United States Department of Justice Case 1:12-cr-00763-ILG Document 3-3, Filed 12/11/12. Attachment A: Statement of Facts.

[2] Further details of this case may be found in Appendix D.

[3] Bank Secrecy Act (1970), Money Laundering Control Act (1986), Anti-Drug Abuse Act of 1988, Annunzio-Wylie Anti-Money Laundering Act (1992), Money Laundering Suppression Act (1994), Money Laundering and Financial Crimes Strategy Act (1998), Uniting and Strengthening America by Providing Appropriate Tools Required to Intercept and Obstruct Terrorism Act of 2001 (USA PATRIOT Act), and Intelligence Reform & Terrorism Prevention Act of 2004.

Mexico and the United States. In the former country, estimates place laundering above $10 billion annual, over 1% of GDP (Angel, 2016). In the U.S., laundering volume surpasses $300 billion per annum (U.S. Dept. of the Treasury, 2015).

Beyond money laundering, illicit cash flows must be rendered mobile to access their full liquidity potential. This cash must be infused into the financial system before, for instance, paying suppliers in remote regions or being wired to a tax haven for safekeeping. As the international payments system is largely built on bank infrastructure, banks are almost by necessity involved in the transmission of illicit financial flows, even unbeknownst to them. However, as a result of the inner workings of money laundering—which I explain at some length in section 2(c) of this paper—it is likely that banks facilitate an important fraction of laundering transactions per se. However, there is little understanding, as before mentioned, of bank behavior in the presence of organized crime. How do banks behave in the face of risks that criminal activity entails? Similarly, we lack credible estimates of the impact that AML regulations have on within-country bank outcomes, as well as on cross-border liquidity flows. In this paper, I exploit variation in the areas of operation of drug cartels and the stringency of law enforcement in Mexico to identify these questions.

Main themes presented in subsequent sections of this paper are (i) testing if criminal activity produced "excess" liquidity at a local scale in Mexico; (ii) testing the impact of increased regulatory activity in Mexico on both local banking activity and liquidity flows into the U.S. and; (iii) testing the timing of shifts in this activity.

This paper contributes to several research literatures. Like Peek & Rosengren (2000), Schnabl (2012), Cetorelli & Goldberg (2012), and Bruno & Shin (2014), it documents cross-border liquidity transmission. However, in this paper, the shock experienced at the origin of the financial flows is not itself a liquidity event, but rather a shift in regulatory regime. In this sense, this paper provides evidence in favor of the hypothesis of "regulatory arbitrage through international bank flows" of Houston, Lin, & Ma (2012). This paper kicked off a rich literature on cross-border regulatory arbitrage in banking, but the papers that have followed it (Reinhardt & Sowerbutts, 2015; Berrospide *et al.*, 2016; Boyer & Kempf, 2016; Frost, de Haan, & van Horen, 2017; Temesvary, 2018) focus almost exclusively on the avoidance of macroprudential regulation. This paper, in contrast, focuses on regulatory arbitrage meant to avoid operational regulation. To the best of my knowledge, this is the first time this phenomenon has been addressed in the literature.

This paper is also part of a growing "forensic economics" literature; Zitzewitz (2012) surveys this literature at length. Related strands of this wider literature are that which addresses the economic analysis of money laundering, the growing literature on the economic impact of the drug trade –particularly in the context of the Mexican economy—and research investigating the spatial propagation of criminality. [4] This paper contributes to these literatures by documenting the effects of the illicit drug trade on the nominal side of the economy. Lastly, this paper contributes to the literature on the enduring economic importance of retail banking. While Gilje (2019), Nguyen (2019), and others explore the credit effects of local bank presence, less has been written on the importance of branch networks from the point of view of banking institutions –as a channel for the receipt of deposits, a cheap source of funding. [5] In fact, most papers dealing with noninterest sources of bank income (Demirgüç-Kunt & Huizinga, 2010; DeYoung & Rice, 2004) stress their negative risk impacts, as the primary concern in the literature has been regarding income streams accrued through trading activities. [6]

The work closest to this present paper is perhaps Slutzky, Villamizar-Villegas, & Williams (2019), which deals with the consequences of AML regulation in Colombia. However, the questions this paper addresses, and the conclusions it reaches are quite different. First, Slutzky et al. center on the credit effects caused by the introduction of AML regulation,

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[4] Key papers in this literature include Masciandaro (1999) and Masciandaro *et al.* (2007), who provide theoretical contributions to the study of money laundering. Walker & Unger (2009) and Schneider (2013) take an empirical approach to studying laundering in Australia and a panel of OECD economies, respectively. Argentiero et al. (2008) straddle these approaches, calibrating a structural model to estimate the volume of money laundering in the Italian economy.

Robles, Calderón, & Magaloni (2013) and Ríos (2008, 2016) explore the effects of the drug trade on the real economy in Mexico. Ben-Yishay & Pearlman (2013) investigate the drop in labor-market participation induced by drug-related violence in this same country. Dell (2015) provides a thorough study of drug-trafficking networks in Mexico; she has also researched the consequences on cartel-related violence caused by labor-market dislocations (Dell et al., 2018). A recent paper by Sobrino (2019) documents the impacts of external demand shocks in the guise of opioid-painkiller reformulations on patterns of drug-related violence in Mexico.

[5] Gilje, Loutskina, and Strahan (2016) do stress the role of branch networks in integrating lending markets through within-bank liquidity transmission, but their focus is on credit outcomes, not on bank profitability.

[6] Köhler (2014) and Drechsler, Savov, & Schnabl (2018) are exceptions, in that they pose channels through which the deposit-taking function of banks might improve their risk-reward trade-off.

through a liquidity-shock channel; on the other hand, I study (i) bank response to organized crime activity and; (ii) liquidity transmission brought about by the regulatory response to this activity. Moreover, Slutzky et al. contend that AML mechanisms in fact work, albeit producing financial dislocations. I, contrastingly, document that absent international policy coordination, the effects of AML constraints are ultimately undone through regulatory arbitrage, as agents seeking to infuse illegal proceeds into the financial system can perform needed transactions in demarcations with less onerous controls. What allows me to gain greater clarity regarding this point is my unique empirical setting(s), in which I observe variation in outcomes across an economically important international border –open to capital flows, but relatively closed to other factors of production.

2 Institutional Context

A. A brief timeline of the Mexican drug war

i. Origins

In 2000, Congress approved a massive aid package for Colombia; this *Plan Colombia* provided for annual expenditures of around $1.2 billion on military materiel for the South American nation (Mejía, 2015).[7] The intent of this initiative was to shore up the efforts of the Colombian government against drug cartels and allied leftist guerrillas. The cartels had grown to be a threat to the Colombian state, and with guerrilla groups such as the FARC, had waged a campaign of terror against the government lasting over two decades.[8] As the *Plan* tipped the scales in favor of the Colombian government, the nation's drug economy shrank. From 2000 to 2013, coca plantations decreased in area from 160,000 ha. to 48,000 ha., and the business volume of drug cultivation and trafficking activities in Colombia dropped from around $7.5

[7] The *Plan* also contemplated the deployment of U.S. Marines to Colombia, and earmarked funds for humanitarian aid and local infrastructure. Further detail is provided in Appendix D.

[8] The Revolutionary Armed Forces of Colombia—People's Army (Spanish: *Fuerzas Armadas Revolucionarias de Colombia—Ejército del Pueblo*, or FARC–EP) was the main Marxist guerrilla group involved in the Colombian armed conflict, which spanned 1964 to 2017, when a peace accord was signed under the auspices of President Juan Manuel Santos.

billion in 2008 to $4.5 billion in 2013 in nominal terms (Mejía, *ibid.*). As the Colombian drug cartels waned, Mexican cartels thrived.[9]

ii. The 2006 election and the Calderón administration

The 2006 presidential election in Mexico was highly competitive. Two front-runners emerged leading up to the ballot: Felipe Calderón, the candidate of the right-wing PAN party, and Andrés Manuel López Obrador[10], of the left-wing PRD. Running head-to-head in the polls, the candidates sought to gain an advantage; Calderón did so by emphasizing the law-and-order angle of the PAN platform. Calling organized crime "a disease, a cancer that burrowed under our skin little by little," (Pérez Silva & LeDuc, 2015) he vowed to "take back the streets, especially along the [U.S.] border."[11]

The Calderón administration (2006-2012) delivered on this promise once in office. Ten days after inauguration, the new government announced the deployment of ground troops, with strong police backing, into the state of Michoacán. (Ellingwood & Wilkinson, 2009). This "joint operation" was followed by similar efforts in other states. This sea change in the anti-narcotics policy of the Mexican government ultimately failed in its objective of increasing public safety in Mexico. It did, however, result in the detention of over 120,000 individuals charged with involvement in the drug trade (*El Universal*, 2010), and the capture or killing of many drug bosses, including Osiel Cárdenas, leader of the Gulf Cartel (*AP,* 2010).

At first, the sole focus of the Mexican government in this new "war on drugs" was on the seizure and destruction of cartel assets such as drug plantations and weaponry, as well as the capture of kingpins. Calderón et al. (2015) document the strategy of "beheading" cartels pursued by the PAN administration, which led to widespread splintering of crime syndicates. This led to geographic spillovers in organized crime activity, evident in Figure 2, which plots the number of municipalities in Mexico with drug cartel activity as identified by Coscia &

[9] Slutzky, Villamizar-Villegas, & Williams (2019) claim the *Plan* was ultimately unsuccessful and did not contribute to large-scale variation in the activity of Colombian drug cartels. This, however, is not the consensus view.

[10] López Obrador, also known by his initials AMLO, is the incumbent President of Mexico, having been elected in 2018 after two previously unsuccessful bids for office.

[11] Op. Cit.

Ríos (2012). As the fight against the cartels wore on, however, the government began to focus also on undermining the financial capability of organized crime syndicates. The Calderón administration introduced several measures aimed at curbing money laundering and cash deposits –a critical component of the money laundering cycle. Namely, in 2008, two related restrictions were placed on peso-denominated deposits at retail banks. First, banks were required to disclose all deposit operations over MXN $25,000 to Mexican tax authorities. [12] Second, cash deposits were subject to a levy of 2% of the amount in excess of this reporting threshold. These figures were later set, in 2009, at MXN $15,000[13] and 3% respectively. In 2010, another round of restrictions was imposed on banks, this time on dollar-denominated transactions. Deposits or foreign-exchange transactions totaling over USD $1,500 by a same party within a calendar month were forbidden, unless the depositor was an accountholder, in which case the limit was set at USD $4,000. Corporate bank customers were forbidden from holding deposit accounts, unless they were registered in a port of entry or along the United States border (*Notimex*, 2010).

Mexico held presidential elections again in 2012; a major campaign issue was the violence that had plagued much of the Mexican territory for the prior six years, throughout the Calderón administration. The PRI party, which had previously governed Mexico for over seven decades as a de-facto state party, carried the election running on the platform of being "a party that knows how to govern and cares about security."[14] Indeed, PRI officials disclosed to U.S. diplomatic officers in confidential settings that although they acknowledged "previous PRI governments [as] corrupt, at least they governed strongly and securely." In 2014, the PRI government of President Peña Nieto repealed both peso- and dollar-deposit restrictions put in place by the Calderón administration.

B. The Mexican banking system

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[12] Around USD $2,200 at the mean MXN/USD exchange rate for 2008.

[13] Around USD $1,300 at the mean contemporaneous exchange rate.

[14] Leaked diplomatic cable communication ("Impact of a PRI Congressional Majority: The Math, the Substance, the Symbolism"), U.S. Embassy in Mexico to Department of Commerce, DHS, and diverse Federal agencies (March 3, 2009). Canonical ID: 09MEXICO604_a. Available at https://wikileaks.org/plusd/cables/09MEXICO604_a.html.

Mexico has a bank-dominated financial system. In 2015, for reference, financial assets were around 90% of GDP, and over half of these were held by banks. Concentration is high in the Mexican banking market, with the seven largest market players accounting for almost two thirds of all bank assets.

After the 1994-1995 Tequila Crisis, Mexican banks faced significant distress, with over 50% of total bank loans non-performing by 1996 (Haber & Musacchio, 2013). Faced with an immediate need for fresh capital, Mexican bank authorities sold off several bailed-out banks to international banking groups. This led to a significant portion of the Mexican banking system coming under the control of foreign banks: of the seven largest banks in Mexico, five are subsidiaries of multinational banking corporations. Banks in Mexico operate almost entirely at national scale.

C. Money laundering and illicit financial flows

Money laundering is an essential component of illicit economic activity. By "money laundering," in this paper I mean the set of procedures by which funds derived from illegal activities are hidden from authorities and placed into the financial system. There are three stages in money laundering: placement, layering, and integration. In the *placement* phase, cash is incorporated into the financial system, typically either through the purchase of financial assets, or through deposit at retail banks. The *layering* phase is that in which the origin of the funds is obscured, to avoid detection by tax and law-enforcement authorities. Finally, in the *integration* phase, the newly "clean" funds are transferred back to the criminal organization, or to suppliers in payment for goods bought and services rendered.

Although a variety of mechanisms underpin illicit financial flows (trade mis-invoicing, wire transfers, etc.), illicit activity that produces large volumes of cash profits must rely on methods allowing for the transfer of liquidity at scale.[15] The preferred method of Mexican

[15] Why not simply wire money across the border? In the United States, outgoing wire transfers are monitored by the Office of Foreign Assets Control (OFAC) for possible transfer of funds to parties on the U.S. Treasury's sanctions list. It is thus less risky for criminal groups to wire funds *into* the U.S. than out of the country.

drug cartels for this purpose has been, historically, bulk cash smuggling.[16] In bulk money smuggling as practiced by Mexican cartels, large volumes of cash or other monetary instruments –such as pre-paid debit cards—are first transported inside the U.S. from drug-sale locations throughout the country to "consolidation cities," metropolitan areas mainly in the Southwestern United States. Once concentrated and accounted for, proceeds are then moved to "deconsolidation cities" along the Mexican border such as Tucson, El Paso, and San Diego. In these cities, large cash shipments are divided into smaller volumes, and prepared for shipment across the border. Finally, cash is passed through into Mexico either by "mules" on private motor vehicles or stashed into tractor trailers transporting consumer goods across the border (United States Office of National Drug Control Policy, 2011).

Once in Mexico, this cash must make its way into the financial system in order to be of use.[17] This entails two separate yet related transactions: first, money –in physical USD—must be exchanged for Mexican pesos (MXN). This operation may be performed through either a retail bank or a currency exchange bureau. Second, cash in MXN must be placed in a store of value. At times, this might be affected through assets such as precious metals or real estate, which typically have a purchase process involving relatively minor due diligence. Alternatively, funds may be directly deposited in a retail bank; this minimizes transaction costs, as both currency exchange and store-of-value transactions can be jointly performed. Given the lax client-facing controls extant in the Mexican banking system, this avenue for cash placement appears particularly attractive.[18] Further, once funds have been placed within the Mexican banking system, they can rapidly be relocated to a fiscal haven, voiding the need for layering altogether.

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[16] Evidence of this, although circumstantial, was the seizure of close to $206 million in cash, allegedly connected with methamphetamine trafficking, by Mexican authorities in 2007. At the time, this was considered the largest drug cash seizure in global history (International Monetary Fund, 2008).

[17] This is mainly because Mexican cartels purchase a significant portion of their productive inputs in the United States. Thus, it becomes necessary for electronic payments to be made to suppliers abroad, which is only possible if cash from operations has been placed into the financial system. This is evident for the case of firearms: in 2016, the United States Government Accountability Office reported that around 70% of weapons seized in Mexico from 2009 to 2014 and traced by the United States Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) were identified as purchased in the U.S. (GAO, 2016).

[18] In FY2018, the Mexican ministry of Finance investigated around 124,000 individuals for suspicious financial activity. Of these, 14 were sanctioned, for a likelihood of sanction of about 0.01% (Ureste, 2018).

D. Effects of increased AML stringency on illicit profit repatriation

In the wake of the efforts of the Calderón administration to curtail the drug trade, large volumes of cash ostensibly related to money laundering began to flow into the financial system of the United States.[19] As the restrictions on cash deposits took effect in Mexico, both the Office of the Comptroller of the Currency (OCC) and the Financial Crimes Enforcement Network (FINCEN) issued bulletin alerts to U.S. financial institutions, anticipating that Mexican customers might "seek new relationships with U.S. financial institutions."[20] Banking authorities cautioned that although "the transactional activity that U.S. financial institutions may experience as a result of the new Mexican restrictions may not be indicative of criminal activity," banks should strengthen AML mechanisms to stay in compliance of the Bank Secrecy Act (BSA).[21] In sum, U.S. banking authorities expected money laundering and profit-repatriation mechanisms to shift away from bulk-cash smuggling, to rely on direct infusion into the financial system of the United States.

3 Data

I use a variety of data sources in this paper. Data on financial and operational variables for Mexican banks comes from CNBV, the Mexican bank regulator. This data includes number of credit card contracts outstanding, deposits volume, and number of branches by bank and municipality, and is available at monthly frequency from 1995 to the present. I collect bank-specific year-end reports containing this data and merge them to form an (unbalanced) panel spanning 1995 to 2010. I merge this data with data obtained from Coscia & Rios (2012), who

[19] Nevaer (2012) reports upwards of $1 trillion but provides no data in support of this claim.

[20] FINCEN Advisory Information Bulletin (FIN-2010-a007), Issued Date: June 21, 2010. Subject: Newly Released Mexican Regulations Imposing Restrictions on Mexican Banks for Transactions in U.S. Currency.

[21] OCC Bulletin 2010-28. (July 22, 2010). Bank Secrecy Act/Anti-money Laundering: Regulations Imposing Restrictions on Mexican Banks for Transactions in U.S. Currency. Retrieved from https://www.occ.treas.gov/news-issuances/bulletins/2010/bulletin-2010-28.html.

construct a novel database on areas of drug-cartel activity in Mexico for a sample period spanning 1990-2010.[22]

Further detail on the measure constructed by Coscia and Ríos can be found in Appendix A.[23] In short, however, this data is obtained from news-media coverage of organized-crime activity: Coscia and Ríos use a web crawler to query the Google News archive, searching for the co-occurrence of municipality names and words within a corpus of terms known ex-post to be associated with criminal organizations. This allows them to track the local presence of seven "cartel families" for a period spanning 1990-2010.

Although this media coverage undoubtedly reflects violence exerted by cartels, it also manifests overt communications on behalf of organized crime syndicates. Phillips & Ríos (2019) document how cartels often –surprisingly—make their presence known in areas in which they operate, through "billboards, graffiti, banners, […] statements to the news media and [web content]" (Phillips & Ríos, op. cit.). This is especially prevalent in areas where incumbent crime syndicates face competitive entry.

Data on U.S. bank deposits, as well as geolocation data for bank branches, comes from Summary of Deposits reports provided by the Federal Deposit Insurance Corporation (FDIC), while data on U.S. bank financials is from the Reports of Condition and Income (Call Reports) provided by the Federal Financial Institutions Examination Council (FFIEC). Data on regulatory enforcement actions is obtained from the websites of the Board of Governors of the Federal Reserve System, the Federal Deposit Insurance Corporation (FDIC), and the Office of the Comptroller of the Currency (OCC). Data on drug seizures is from U.S. Customs and Border Protection (USCBP), which reports the annual volume of cannabis and cocaine seized at each of the land points of entry into the United States, including the 48 along the Mexican border, as well as seizures performed at checkpoints in the interior of the U.S. Data

[22] Downloadable from Michele Coscia's academic webpage: http://www.michelecoscia.com/?page_id=1032

[23] Mexico is a federal union of 31 states and a capital region, divided into 2,456 municipios, or municipalities. *Municipios* are roughly comparable to counties in the U.S. context, both in scale and in the scope of local governance. Regulatory bank reports are disaggregated at the *municipio* level, and the data of Coscia & Ríos has the municipality as the level of observation. All empirical analyses for Mexico in this paper are thus specified at the municipality level.

on illicit drug prices is retrieved from a variety of sources, including the UN Office on Drugs and Crime (UNODC).

Summary statistics for key variables are presented in Table 1. In Panel A, statistics on outcome variables and controls are presented for sampled municipalities in Mexico. Panel B contains summary statistics for the U.S. sample, at the bank branch level. Although the treatment sample (municipalities eventually treated) is quite different across several dimensions from the "never treated" sample for the case of Mexico, the identification strategy I follow ensures that many localities are used as both treatment units and controls. I expound on this point in the Empirical Strategy section of this paper. I also report results for treatment-on-the-treated (ToT) estimations of tests performed in this empirical setting in the upcoming Results section. Lastly, to further address concerns that selection might be driving the findings of this paper, I have run (untabulated) matched-sample tests for core results, with little qualitative impact.

Figure 1 plots the spatiotemporal variation in drug-cartel presence in Mexican municipalities. As can be seen, a significant expansion of cartel presence took place from 1995 to 2010. According to the measure of Coscia & Ríos, only around 1% of municipalities were treated in 1995, in stark contrast with the treatment rate of almost 29% calculated for 2010.[24] As the variation exploited is at the level of the municipality-year, a potential concern is that treatment be clustered in a few periods.[25] To address this concern, I plot the net number of municipalities entering the treatment group in each year from 1990 to 2010 in Figure 2. Although there are spikes in entry around the years 2005 and 2008, it is apparent that there is a steady accrual of municipalities into the treatment group, particularly from 1995 onward, the relevant time sample for this paper.[26]

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[24] This figure is at odds with those calculated by the Mexican government: official data reported cartel activity in up to two-thirds of municipalities by 2008 (Dell, 2015). However, government data on local cartel presence is classified; also, it is likely to capture variation in drug-related *violence*, not the "productive" activity of criminal organizations. Phillips & Ríos (2019) document productive activities of drug cartels as a predictor of overt cartel communication.

[25] A municipality is coded as "treated" for year *t* iff the Coscia/Ríos data flags it as having cartel presence in that year, not so if was flagged in previous periods but not this one. In other words, municipalities can "drop out" of the treatment group.

[26] As before stated, the post-2006 spike is attributable to the "splintering" of already active crime syndicates. Cartel lieutenants not only vied for leadership of acephalous organizations, but also often carved out new areas of influence for successor cartels.

4 Empirical strategy

Several empirical challenges complicate testing the effects of organized crime on local banking outcomes. First, both affected and unaffected regions are exposed to common shocks such as business-cycle fluctuations. Hence, a naïve panel-regression analysis would not identify the causal impact of cartel activity on the variables of interest. Further, cartel presence might be endogenous to local characteristics, invariant or time varying. For instance, cartels might flock to "boomtowns," i.e., locales with high economic growth or above-average expectations of economic performance. Conversely, cartels might prey on poorer localities to take advantage of institutional weaknesses or attempt to corrupt local officials through bribes. Geographic conditions could also influence the attractiveness of a locality to criminal organizations. Mexican drug cartels are in essence logistics firms, and transportation costs affect their bottom line significantly.

Lastly, "treatment" in this setting affects localities in a time-staggered manner. Hence, treatment periods will be location specific. A simple differences-in-differences estimation would hence not be appropriate to identify the causal impact of cartel activity on outcomes of interest. To deal with these issues, I pursue a generalized differences-in-differences (DiD) empirical strategy in the first part of this paper, which deals with the effects of drug-cartel activity on banking outcomes at a local scale. This method allows the treatment and control groups to be defined dynamically. In each period t, the treatment group consists of municipalities with cartel presence, and the control group of those in which no cartels are active.

Likewise, testing the effectiveness of AML policy is not straightforward. Regulation —and its enforcement—are neither randomly assigned (save for special cases) nor arise in a vacuum. Rather, these tend to originate as a response to extant social phenomena as well as affect these, and thus their study is subject to the "reflection" problem identified by Manski (1993). To get around this problem, in the second part of this paper I run a series of canonical differences-in-differences tests to determine both the impact that a shift in the *Mexican* regulatory regime had on U.S. banking outcomes, such as deposits volume, and the endogenous response of banks to this shock. Since variation in drug-cartel activity and in

Mexican regulation are arguably exogenous to banking activity in the U.S., I can isolate the causal effect of changes in the Mexican drug trade on banks in U.S. Border States. In this manner, I indirectly estimate the effects of heightened AML stringency on cross-border financial flows.

5 Results

A. Mexico

I first present results for a set of panel regressions of bank activity in Mexico on measures of organized-crime presence. These regressions are all specified at the municipality-year level. I start with the most parsimonious set of models, with the following general specification:

$$ln(y_{it}) = \alpha_i + \gamma_t + \beta Treated_{it} + \varepsilon_{it} \tag{1}$$

In equation (1), y_{it} is a (logged) outcome vector, with entries corresponding to $Credit_{it}$, the number of credit-card contracts active in municipality i in year t, $Deposits_{it}$, total deposit volume in a given municipality-year, and $Branches_{it}$, the number of bank branches active in this municipality in year t. The model is fully saturated, with α_i being a municipality-specific fixed effect, and γ_t a year fixed effect. $Treated_{it}$ is a binary variable which "turns on" if there are any cartels active in municipality i at time t[27]. The above specification is like the main ones in Jayaratne & Strahan (1996) or Bertrand and Mullainathan (2003).

It is worth noting that this model may be estimated even if all localities are eventually treated during the sample period, as for every period t the control group will contain localities so far untreated**.** Results for these baseline regressions are presented in Table 2. For the preferred specifications (Column 1) of Panels A-C, cartel presence is found to have an impact of around

[27] The treatment indicator used is "contemporaneous" to the outcome variable. However, the data used in the construction of outcomes is obtained from end-of-year reporting, and treatment data contains a "natural" lag, due to publication delay. Hence, the lag structure of the expected treatment effect is replicated by the data.

29% on deposits, 12% on bank branches, and -17% on credit card lending. These results are robust to the inclusion of controls, which include (log) municipal population, unemployment rate, (log) number of homicides, and (log) annual municipal government expenditures, as is apparent in Column 2 of Panels A-C.[28]

Results for deposits and branches are robust to the inclusion of (log) active credit card contracts as an additional control.[29] This strengthens the contention that a treatment effect exists for these outcomes, independent of either endogenous lending growth, or a credit expansion driven by a prior growth in real economic activity.

In Table 3, the results of ToT tests analogous to the ones mentioned above are reported. Directionally, the effect estimations obtained line up well with ATE estimates, although the magnitudes are somewhat muted. To account for possible nonlinear treatment effects, I also specify regressions with the following specification:

$$ln(y_{it}) = \alpha_i + \gamma_t + \beta_1 Treated_{it} + \beta_2 Cartels_{it} + \varepsilon_{it} \tag{2}$$

In equation (2), $Cartels_{it}$ is a count variable tallying the number of drug cartels active in municipality i at time t. Results for this set of regressions are presented in Column (3) of Panels A-C of Table 2. As can be seen, previously estimated coefficients retain their sign, although their magnitudes change somewhat. The functional relationship between the different outcomes and criminality also seems to be idiosyncratic: while the effect on deposits is fully realized upon *any* level of cartel activity, the contraction of credit seems to accrue more slowly, and be virtually zero if only one cartel is active locally.

A speculative explanation for this is that the real (licit) economy contracts upon the local entry of crime syndicates but is slow to do so. Although potential extorsion and the erosion of the rule of law seem prima facie sufficient to scare away investment, capital is not always

[28] Quantitatively, the result for branches is quite diminished upon the inclusion of controls. This might be due to sample attrition: one or more control variables is missing for around 2/3 of the sample, which biases the sample toward larger municipalities in these specifications.

[29] Results untabulated.

mobile. In fact, Calderón et al (2015) find evidence consistent with this contraction only happening when competitive entry of new criminal groups produces violence. The relation between branches and cartel presence, lastly, appears to be approximately log linear.

To address concerns that the above results might be driven by pre-trends, I present an analysis of coefficient dynamics in Figure 3. In these panels, the X-axis represents time around first treatment at the municipality level. As can be seen in these graphs, the estimated coefficients remain relatively flat prior to the onset of cartel activity, and present sharp increments around $t = 0$, the period of initial treatment. For both graphs, the omitted category is the pre-treatment period in excess of four years. These tests are performed on the treatment sample; to test more generally for the absence of pre-trends, I estimate models of the form

$$ln(y_{it}) = \alpha_i + \gamma_t + \beta_1 Treated_{it} + \beta_2 \mathbb{1}\{\mathcal{T} - k\}_i + \varepsilon_{it} \tag{3}$$

In equation (3), $t = \mathcal{T}$ is the period of first treatment for municipality i, while $k \epsilon \{1\} \cup \{2\}$. Results for these estimations are presented in Table 4: pre-period dummies fail to load significantly in all specifications.[30]

To address concerns regarding the potential endogeneity of treatment assignment, I run regression models with the following general specification:

$$Treated_{it} = \alpha_i + \gamma_t + \boldsymbol{X_t}\beta + \varepsilon_{it} \tag{4}$$

where i indexes municipalities and t indexes years, and the contemporaneous municipal unemployment rate, (log) population, (log) municipal government expenditures, and the

[30] These graphs can also be thought of as representing “treatment on the treated” (ToT) treatment effects, due to the estimations that produce them having been run only on the treatment sample. This should further allay concerns regarding imbalance between treatment and control groups.

"marginalization index" are included as explanatory variables, as proxies of local economic conditions.[31]

Results for these models are presented in Table 5. As can be seen, local economic conditions do not seem to explain the contemporaneous local presence of drug cartels.

It is of interest to determine whether increased law enforcement actions, or changes in regulatory stringency, could shift the estimated quasi-elasticities of these outcomes to criminal activity. Testing whether the law enforcement and AML actions of the Calderón administration had an effect is key to the goals of this paper. Particularly, it is relevant to determine whether the 2006 shock had by itself an effect on outcomes of interest. Further, it is pertinent to check for effects caused by policies enacted in 2008 and 2010. To test for these potential shifts, I run the following differences-in-differences specification:

$$ln(Deposits_{it}) = \alpha_i + \gamma_t + \beta(Treated_{it} \times \mathbb{1}_\tau) + \varepsilon_{it} \tag{5}$$

for $\mathcal{T} = \{2006, \dots, 2010\}$. Results are presented in Table 6; I report estimations both on the entire sample (ATE), and on the treatment sample (ToT). For both specifications, a steep decline in the rate of deposits accumulation is discernible for 2006 in treated municipalities. In the ToT estimation, the 2006 decline all but erases the treatment effect, and subsequent years continue the decline. Results are less straightforward for the whole sample, yet it is quite clear that a significant dampening of the treatment effect occurs, irrespective of sample.

B. United States

The previous results indicate that the regulatory tightening brought about by the 2006 election was a negative liquidity event for Mexican banks in treated localities. However, was

[31] The "marginalization index" (*índice de marginación* in the original Spanish) is a needs-based measure of poverty calculated annually by the Mexican government at the municipality level, using data from INEGI. In short, this index captures the first principal component in the variation of unmet necessities among municipal populations, in areas such as health, basic education, and housing (Ávila, 2001).

this reversal caused by decreased business volume in the drug trade and associated laundering activity?

In Figure 4, I plot supply- and demand-side factors related to the trans-border cannabis market, a microcosm of the larger illicit drug trade. As can be seen, seizures of shipments remained largely flat over the 2005-2013 period, with only a slight dip occurring around 2008. Although drug seizures at the border are a coarse measure of the extant business volume in the U.S.-Mexico drug market, this data confirms what is also apparent in Figure 2: there is no discernible drop in cartel activity around the 2006 election, nor immediately after.

If production volume did not decline, and assuming no major price shocks, which is reasonable given the demand dynamics discernible from Figure 4, we are left with somewhat of a puzzle: where did this "missing" liquidity go?[32]

Due to how illicit liquidity was infused into the financial system before this regulatory shock (i.e., through bulk dollar smuggling), it is a reasonable hypothesis that a portion of extant liquidity flows stopped making their way into Mexico and were instead deposited in U.S. banks. This hypothesis is also supported by anecdotal data. In the United States, law enforcement agencies, as well as financial regulators, issued memoranda warning about heightened money-laundering risks under the Calderón administration. Further, in Congressional Testimony, Immigration and Customs Enforcement (ICE) personnel declared, after the last round of deposit restrictions had been imposed in Mexico:

> *"[A] significant development […] has been a change in Mexican banking regulations that severely limits the amount of U.S. dollars that can be deposited [...] This […] has change[d] how drug proceeds are laundered. [There is] a desire to place these*

[32] If anything, the proportion of the U.S. population reporting cannabis use during the previous month grew over this period. The same temporal pattern is observed during this period for other illicit drugs.

> *funds into U.S. financial institutions and then wire the proceeds to Mexico…"* [33]

I hypothesize, however, that the sequence of regulatory shocks *starting in 2006* is the relevant "event" for liquidity transmission, and not the regulations or regulatory actions executed *in any one year* of the Calderón administration. The reason for this is dual: first, military actions against cartels ratcheted upward quickly starting in 2006. This means that the shift in the cost structure of cartels stemming from these actions was an abrupt *level change* around this year. Second, as counter-narcotics efforts were highly trumpeted by the government, it is likely that a change in expectations occurred with the election. Hence, I expect most of the treatment effect reversal to be realized immediately after the 2006 election, and smaller effects to occur with the imposition of AML measures in 2008 and 2010. I further expect an effect reversal to occur around 2012, when the PAN party was voted out in favor of the PRI, perceived as more lenient toward organized crime.

To test for these hypothesized regulatory effects on cross-border liquidity flows, I estimate the following equation:

$$ln(Deposits_{it}) = \alpha_i + \gamma_t + \sum_{\tau \epsilon \mathcal{T}} \beta_\tau Post_\tau \times Distance_{it} + \varepsilon_{it} \tag{6}$$

In the equation above, i indexes bank branches, while t indexes years. $Distance_{it}$ is the shortest path between a branch i, represented as a latitude-longitude vector, and the set of points lying on the U.S.-Mexico border, while $\mathcal{T} = \{2006, 2012\}$.[34] The inclusion of 2012 is to

[33] Testimony of U.S. ICE/Homeland Security Investigations Special Agent Matthew Allen to the House Committee on Homeland Security, Subcommittee on Border and Maritime Security (May 21, 2012)

[34] More formally, the calculated distance is the minimal geodesic using the formula of Vincenty (1975), on an ellipsoidal model of the Earth.

test whether a reversal in the 2006 shock was experienced once the law-and-order focused PAN government lost the Federal administration. The sample considered in this estimation is the set of all FDIC-insured bank branches located in one of the 24 counties in the United States that straddle the Mexican border.[35] The sample period is 1995-2018. Results for this estimation are presented in Table 7.

As can be seen in column (1) of this table, $Distance_{it}$ loads negatively on (logged) deposits when interacted with a post-2006 dummy.[36] More precisely, each one-hundred mile increase in branch distance to the border predicts around a 7% decrease in deposits after 2006. The interaction with a post-2012 dummy, however, yields a positive sign: a one-hundred-mile increase in branch distance to the border predicts around a 2% *increase* in deposits in column (3); this estimate is however not statistically significant. The pattern of temporal variation is quite clearly seen in Panel A of Figure 5: there is a sharp drop in the estimated coefficient around 2005-2006, with a further drop in 2007. After this latter year, there is a reversal in the estimated effect.

It is unclear that the regression equation above captures the correct specification to gauge the effect of branch-level exposure to cartel activity on deposits. Considering this, for the sake of robustness, I specify two other sets of models using alternative exposure measures. For the first set, I proxy branch-level exposure with the least geodesic distance from bank branches to the geographic center of drug-cartel activity in Mexico. Details of the construction of this measure may be found in Appendix B.[37] A graph of coefficient dynamics for regressions estimated using this measure is found in Figure 5 (Panel B). For the second set of regressions, I take a similar approach, using drug-seizure data to create a measure of branch exposure. Details of the construction of this measure are also included in the appendix. A plot of coefficient dynamics for regressions estimated using this measure is found in Figure 5 (Panel C). In sum, this metric is the distance of a branch to the nearest USCBP checkpoint, weighted

[35] These counties are San Diego and Imperial counties in CA; Yuma, Pima, Santa Cruz, and Cochise counties in AZ; Hidalgo, Luna, and Doña Ana counties in NM; El Paso, Hudspeth, Jeff Davis, Presidio, Brewster, Terrell, Val Verde, Kinney, Maverick, Webb, Zapata, Starr, Hidalgo, and Cameron counties in TX.

[36] "Post" dummies in all specifications "turn on" in the year of their index and continue taking a value of one for all years thereafter in the sample period.

[37] Henceforth, I refer to this measure as "distance-to-treatment."

at yearly frequency by the dollar value of seizures at this point. A graph illustrating this measure may be found in Appendix B.

It is apparent from all three sets of regression estimations that the 2006 shock was a positive liquidity event for bank branches in counties contiguous to the Mexican border. However, is there evidence that this had any positive impact on bank financials? To test for this, I estimate equations with the following general form:

$$ln(y_{jt}) = \alpha_j + \beta_1\left(\frac{\sum_i Branches_{ijt} \times \mathbb{1}\{Border_i\}}{\sum_i Branches_{ijt}}\right) + \beta_2\left(Exposure_{jt} \times Treatment\ period\right) + \varepsilon_{ijt} \quad (7)$$

In these regressions, i indexes counties, j indexes banks, and t sample-period years.[38] The variable $Exposure_{jt}$ in the third term of the expression above is shorthand for the ratio appearing inside parentheses in the second term of the equation. $Exposure_{jt}$ is equal to the proportion of branches belonging to bank j in counties contiguous to the Mexico border, out of all branches in the United States. This measure is meant to proxy for the degree to which a bank's business is dependent on Mexican customers –including, potentially, cartel operatives. Standard errors in these regressions are clustered at the bank level. $Treatment\ period$ is a dummy variable that turns on in the period of interest (2006-2012) and zero otherwise.

Table 8 reports the result of this set of tests. Although there is a small effect on profitability for the wider sample, effects are strongest for the interaction term $Exposure_{jt} \times Treatment\ period \times Small_{jt}$, where this last variable is a dummy that turns on for banks in the first two deciles of the per-year distribution of bank assets.

These results make sense if one considers what a positive liquidity shock means for the financial condition of banks. Increased deposits –a cheap source of funding—lessen the necessity for interbank borrowing, depressing interest expense. This effect is only relevant for small banks, which may face financial constraints –not so for large banks, who many

[38] Interactions with size dummies are omitted from equation (7).

times operate as dealers in the interbank market for funds. Another possibility is that smaller banks are more pliable to pressure from criminal syndicates, either because of the high fixed costs that AML mechanisms carry, or because of lower latent reputational risk.

Because of the way these deposits are brought into banks and flow through them, it is also likely that an increased volume of transactions accompanied this positive liquidity shock. Indeed, the cycle of money laundering requires a multiplicity of transactions in both the placement and layering of illicit monies. This explains the positive effects on noninterest income reported in Table 8.

Having obtained these results, the question becomes whether there is an endogenous bank-branch network response to these liquidity shocks. To test for this, I run canonical differences-in-differences specifications of the following type:

$$ln(Branches_{it}) = \alpha_i + \gamma_t + \beta_1 Distance_{it} + \beta_2 (Distance_{it} \times Post_{\mathcal{T}}) + \varepsilon_{it} \tag{8}$$

In the above equation, $Distance_{it}$ is any of the distance measures introduced before: least geodesic distance to the Mexico border, "distance to treatment", or "distance to seizure." $Branches_{it}$ is the number of active bank branches for locality i in year t, and $\mathcal{T} \epsilon \{2006, 2012\}$. The sample considered in this regression is of all ZIP codes in counties on the southern border. As can be seen in Table 9, a 100-mile increase in distance from the border predicts a decline of around 3-5% in active bank branches after 2006.

Untabulated tests of this effect at the county level yield a better interpretation of the economic significance this result: a one standard deviation increase in distance to the border would produce a net increase of around 11 branches in a county after 2006. Although this effect is not large in absolute terms, it must be considered within the wider trend of branch closures occurring in the U.S. during this period, shown in Figure 6.

6 Further results

A. Bank enforcement in the United States

So far, I have shown that (i) there are detectable liquidity flows associated with the illicit drug trade; (ii) these flows are sensitive to regulatory stringency; and (iii) there are endogenous bank responses to these shifting capital flows. However, several potential concerns remain. For one, it may be that the cross-border flows extant after 2006 correspond to the flight of *licit* capital from Mexico, as violence ratcheted upward due to the onset of the Drug War. To rule this out, I test whether deposit receipts in border counties predict enforcement actions by bank regulators after 2006. More precisely, I estimate the equation

$$\mathbb{1}\{Enforcement_{it}\} = \alpha_i + \gamma_t + \beta\left[\ln\left(Deposits_{it}^{Border}\right) \times Post_{2006}\right] + Decile_{it} + \{RegDummies_{it}\} + \varepsilon_{it} \tag{9}$$

where factorial terms for $ln(Deposits_{it}^{Border}) \times Post_{2006}$ have been omitted in Equation (9).[39] In the above equation, i indexes banks, and t indexes years. $Deposits_{it}^{Border}$ is the quantity of deposits a bank holds in the 24 counties straddling the Mexican border, while α_i is a bank fixed effect, and γ_t a set of year fixed effects. $Decile_{it}$ is the within-sample asset decile bank i belongs to in year t, whereas $RegDummies_{it}$ is a set of indicator variables for the principal regulator of bank i in this year: OCC, Federal Reserve, OTS, or FDIC.

The sample considered for this test is of all banks with branches in AZ, CA, NM, and TX for the period spanning 1995-2017. Results for this estimation are presented in Table 10. Roughly, these results might be interpreted as follows: for every additional \$50M of deposits in border counties, a bank was 1% more likely to receive an enforcement action (of any kind) from its primary regulator after 2006. Although this result in no way conclusively proves that these post-2006 liquidity flows were "dirty money," it does strengthen the contention that banks were engaging in riskier transactional activity in the border region during this period.

B. Electoral outcomes in Mexico

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[39] Loadings for these factorials are, however, included in the table reporting results for this estimation.

Another potential objection that could be raised regarding the results presented so far is that showing the presence of regulatory arbitrage is not tantamount to showing the failure of heightened AML enforcement. In a strict sense, we would need to observe a clear-cut counterfactual to gauge the impact of the observed policy shifts. To address this, I test whether the imposition of AML controls, together with heightened law enforcement, had any electoral impact in Mexico. More precisely, I estimate the regression equation

$$\mathbb{1}\{Incumbent\ loss\}_{it} = \alpha_i + \gamma_t + \beta\left(Treated_{it} \times Deposits_i^{2006} \times Post_{2006}\right) + \varepsilon_{it} \tag{10}$$

where factorials for $Treated_{it} \times Deposits_i^{2006} \times Post_{2006}$ have been omitted in Equation (10).[40] In this equation, i indexes municipalities and t indexes years, for a repeated cross-section of municipal elections spanning 2000-2017. Table 11 reports the results of linear probability and panel logistic-regression estimations of this model. Both models are saturated with municipality- and year fixed effects and contain party dummies for the three major political parties in Mexico during the sample period (PAN, PRI, PRD). I find that, among treated municipalities, the 2006 level of deposits positively predicts the probability of incumbent loss in post-2006 municipal elections. I interpret this finding as evidence of political backlash against government actions that compromised local liquidity windfalls.

This finding is consistent with the view that post-2006 electoral politics in Mexico were increasingly centered around a referendum on the government's anti-cartel policies, which may have led to the defeat of the ruling PAN party at the Federal level in 2012, and the subsequent reversal of many of these.

[40] For notational clarity. Coefficients for these terms are reported in Table 11.

7 Discussion

Banking is a heavily regulated economic activity the world over, as banks are critical to welfare through the allocation of capital, the support of a payments system, and the escrow of savings. Hence, legal norms disallow banks to engage in certain activities, to preserve financial stability and preempt the channeling of capital to antisocial activity. Despite these norms, however, banking institutions have a rich history of malfeasance. From the South Sea Bubble, to the excesses of the "Wildcat banking" era (Jackson & Kotlikoff, 2018) to the billions in fines and settlements paid for illegal lending practices by Citigroup, Deutsche Bank, and others in connection to the 2008 Financial Crisis (Kraakman, Soltes, & Hofstetter, 2018), there are numerous examples of intermediaries breaking laws and regulations meant to constrain them. Is there reason to think that this pattern of behavior is systemic?

There is an ample literature that portrays banks as information producers. Starting with Fama (1985), a great deal of papers have documented the screening (Stiglitz & Weiss, 1988), monitoring (Houston & James, 1996), processing of "soft" information (Petersen & Rajan, 1994), and efficient capital allocation (Beck & Levine, 2004) functions of banks. In short, the common conceptual thread in these works is this: the advantage that banks hold is their relative advantage in generating –and ultimately diffusing—information on the credit risk of borrowers. Hence, the key source of bank value is information revelation to the wider economy.

A more recent strand of literature portrays banks, contrastingly, as "information garbling" mechanisms. Dang, Gorton, Holmström & Ordoñez (2017), for instance, paint banks as *secret keepers*. These authors claim that banks muddle information on the credit risk of debt claims they hold as assets. This allows for these claims to trade as "moneylike" securities (*i.e.*, at face value), as it is costly for agents to produce information on their "true" discount. Jackson & Kotlikoff (2018) draw a link between this "secret keeping" function of banks and malfeasance in banking. In sum, these authors link historical banking crises to bank malfeasance: if banks are skilled at distorting information about the debt claims they hold, this generates the moral hazard of using this capacity for hiding outright graft, or other privately optimal illicit activity.

This all pertains to the asset side of banks' balance sheets, however. What about liabilities? The anecdotes on banks obscuring the origin of deposits they hold or helping structure mechanisms for the store of value that muddle the identity of depositors are numerous. Banks have been documented receiving deposits from illicit sources, channeling deposits to fiscal havens, and helping clients avoid taxation through obscuring their liquid wealth. However, we lack a rigorous understanding of whether banks systematically engage in these activities, and if so, why.

Results obtained in this paper are consistent with banks acting as "potentially crooked secret keepers" (Jackson & Kotlikoff, op. cit.) In other words, the evidence presented may point to banks utilizing installed "opacity technology" gained through their "secret-keeping" function to optimize operations in a manner inaccessible to other economic agents. Banks can expose themselves to the risk of potential relationships with organized crime in order to secure cheap sources of liquidity. Anecdotes point to banks anticipating lenient penalties should this risk realize, and authorities crack down. Further research is needed, however, to disentangle the potential links between banking activity and the illicit sectors of the economy.

However, if banks do in fact "enable" criminality, is this unambiguously welfare decreasing? It is far from clear that this is the case: there is no consensus view in the literature regarding the net effect that organized crime has on economic outcomes.

A reasonable prior is that criminal activities have net negative economic effects, as they generate distortions that decrease the productivity of the licit economy and undermine government institutions through corruption. Money laundering, and the associated transmission of illicit financial flows, are no exception concerning potential negative impacts. Indeed, the infusion of criminal profits into the financial system might undermine the solidity of the financial sector through, for instance, reputational erosion. Money laundering might also cause negative impacts in the international trade and finance sectors, via distortions in the balance of payments. These impacts have been documented in previous research, like that of Kumar (2012) and Bartlett & Ballantine (2002).

A case may be made that illicit activities can have a positive impact on economic growth, however –especially in developing economies—through liquidity spillovers that ultimately make their way into productive investment (Villa, Loayza, & Misas, 2017). However, money laundering *per se* quite likely leads to some measure of capital misallocation[41], and once laundering is extant, there will be considerable costs associated with ending it. Geiger and Wuensch (2006), for instance, argue that a *laissez-faire* approach to money laundering might be optimal, since anti-money laundering regulations may be welfare-reducing, entailing stark costs of compliance. In fact, these authors argue, these costs risk becoming so onerous that the policy intent of anti-money laundering regulation backfires, leading to an equilibrium increase in money-laundering volume.

Are there normative implications to be drawn from this paper? Surely so. However, its intent is not to allocate blame for criminal activity to financial intermediaries. Rather, the goal is to bring attention to the stark trade-offs implied by regulation and legal enforcement aimed at "securing" the financial sector from interactions with organized crime. These regulations are seldom questioned, yet carry significant costs, both direct –through compliance expense— and indirect –through the liquidity channel established by Slutzky et al. (2018). Careful consideration should be given to the imposition of operational controls on financial intermediaries, weighing costs and benefits: this is especially true in the context of open financial systems, where the costs faced by agents seeking to circumvent these controls via liquidity exports are relatively low.

8 Conclusions

In this paper, I have presented evidence that i. local drug-cartel activity leads to positive local liquidity shocks for banks, which respond endogenously by increasing their local footprint; ii. that regulatory tightening in law-enforcement and AML policy aimed at handicapping the activity of organized crime leads to cross-border liquidity flows; iii. that increased legal enforcement leads to political backlash in locales that have been benefited by liquidity windfalls generated by illicit economic activity.

[41] Bribes, kickbacks, and other "investments" needed to set up a money laundering mechanism might be thought of as purely transfers. However, they are also transactions costs from the viewpoint of a Walrasian planner: as endogenous entry into regulation is impossible, these costs will not be competed away, and remain in equilibrium.

I document that local criminal presence leads to an expansion of the deposit base by around a third in Mexican municipalities. After 2006, when a law-and-order administration takes power, deposits flow into banks located along the Mexico border in the United States. These results are robust to a variety of empirical specifications. Importantly, I document that banks respond to these positive liquidity shocks by expanding their branch networks into regions exposed to criminal activity.

In sum, "finance follows crime," i.e. there is growth in both the extensive (branch network expansion) and intensive (deposit capture) margins of banking activity when criminal activity produces large liquidity windfalls. These findings have several implications. For one, banks –wittingly or unwittingly—enable the operation of organized crime. Second, deposits captured through retail branch networks matter. Banks are willing to enter areas rife with crime, and expose themselves to reputational hazards, to secure relatively cheap sources of funding. This suggests that banks actively trade off reputational and operational risks with funding needs.[42]

Increased regulatory stringency, on the other hand, produces liquidity flows. Evidence presented herein shows that once the relative cost of depositing illicit cash flows was tilted in favor of the United States, deposits indeed flowed from Mexico, which had become relatively unattractive because of new deposit controls. This supports the hypothesis of cross-border regulatory arbitrage of Houston, Lin, & Ma (2012).[43]

Lastly, AML regulation likely fails in financially handicapping organized crime syndicates. Although the locus of liquidity spillovers from criminal activity shifts upon increased regulatory stringency in Mexico, there is no evidence that drug-cartel activity was reduced as a result. Increased controls on financial intermediation, do, however, produce political backlash. This is a clear example of a "race-to-the-bottom" regulatory dialectic, such as was

[42] Whether banks possess intent to collaborate with criminal activity in entering these areas is moot. It is highly unlikely that banks are oblivious to the presence of criminal activity at a local scale, especially when this activity has been documented in the media, which is a necessary condition for a locality to be coded as treated in the first part of this paper. That banks willingly face risks in entering areas of criminal activity is *fait accompli*.

[43] Although Houston et al. document a slightly different channel, in which banks themselves redirect capital to lower-regulation polities, the *economic* effect of depositors or banks driving these liquidity flows is similar.

hypothesized by Kane (1981), and brings into question the wider welfare effects of (perhaps well-meaning) regulatory efforts aimed at insulating the financial sector from organized crime.

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Figure 1. Geographic expansion of drug-cartel activity in Mexico

This figure shows the spatiotemporal variation in drug-cartel activity in Mexico from 1995 to 2010. Areas in which at least one drug cartel is active are darkly shaded; light shading signifies no data available; no shading indicates no cartels active. Units of observation are *municipios*. Data for the elaboration of this figure is obtained from Coscia & Ríos (2012).

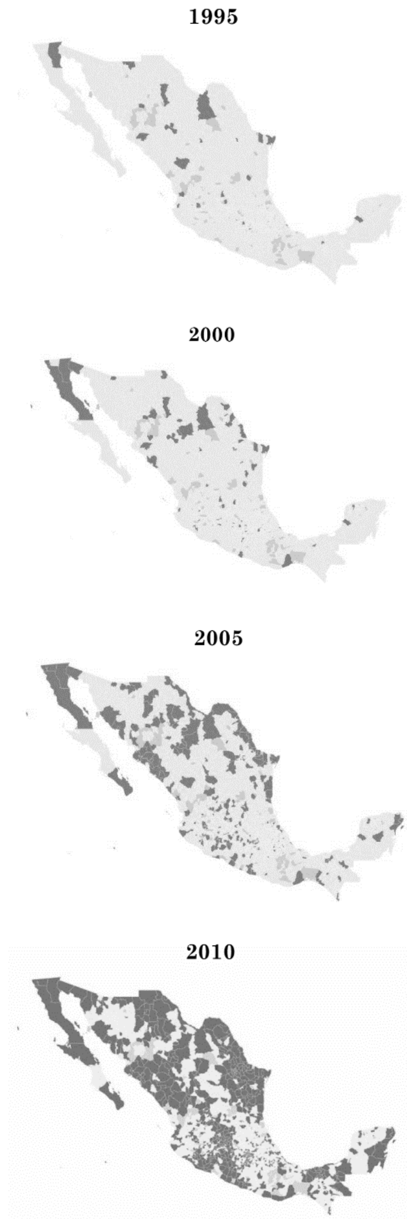


Figure 2. Net entry of municipalities into treatment group, 1990-2010

This figure plots the net number of municipalities entering the treatment group for years 1990 to 2010, according to the data presented in Coscia & Ríos (2012). Sample periods for analyses presented in this paper begin in 1995.

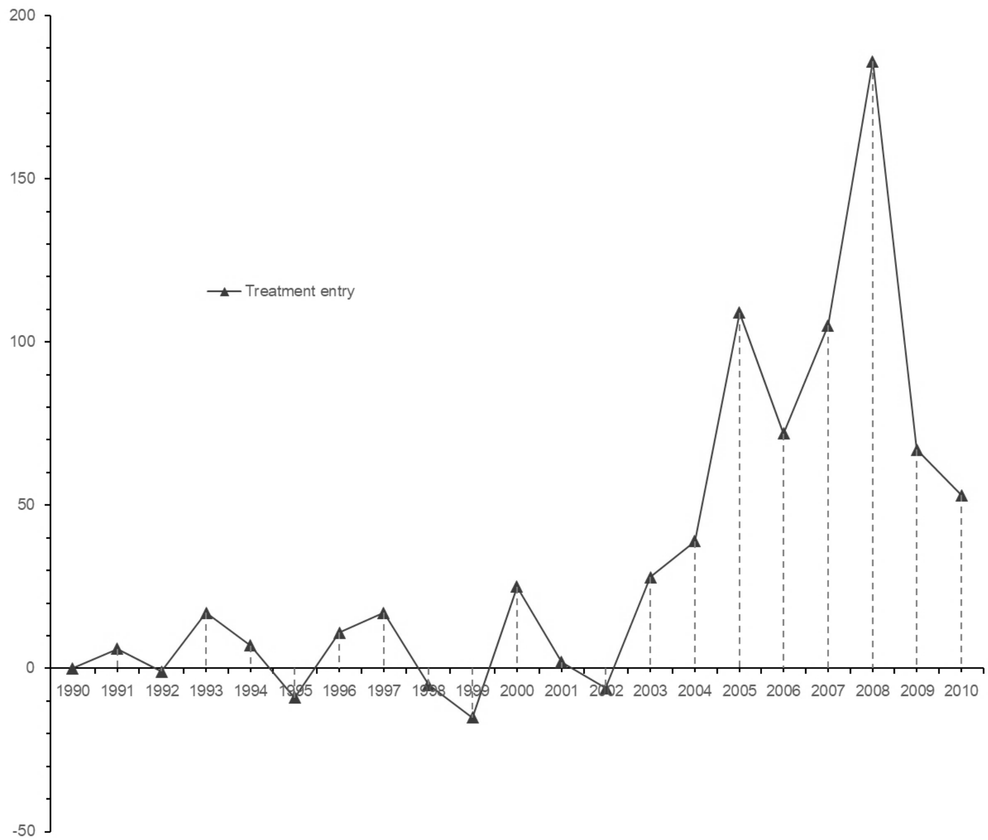
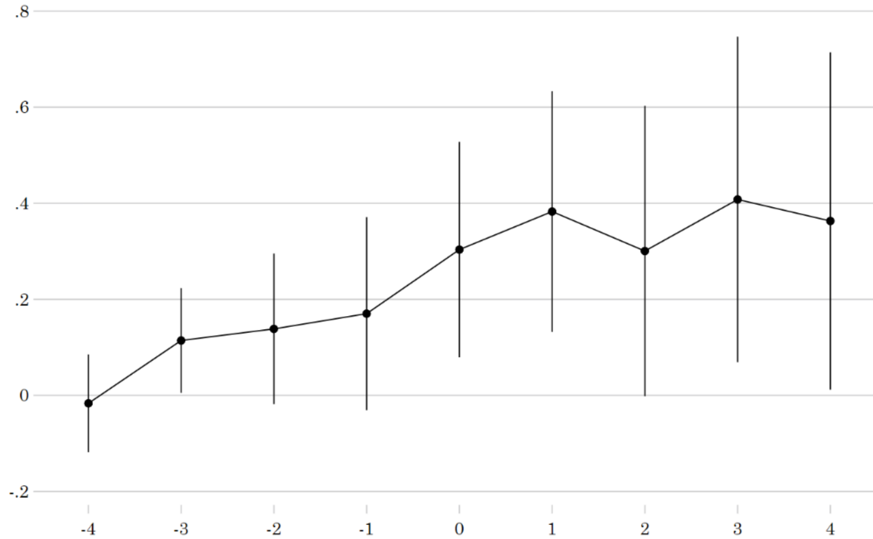


Figure 3. Treatment effects on local banking activity, event time

The graph below shows estimated coefficients for regression models with the specification $Y_{it} = \alpha_i + \sum_j \beta_j \mathbb{1}\{t = \mathcal{T}\} + \varepsilon_{it}$, in which α_i is a municipality FE, and $\mathcal{T}$ is the year around first cartel activity observed for municipality i: Year 0 is the first period in which each unit is treated. The models above are run *only* on the treatment group. Panel A shows effects on total deposits; Panel B shows effects on active bank branches. The omitted category is years in excess of five before first treatment.

Panel A: Deposits



Panel B: Branches

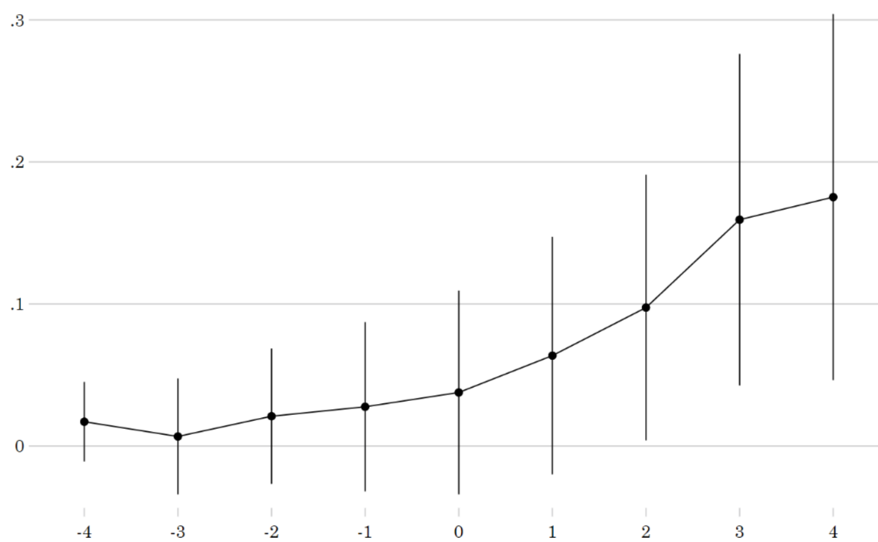


Figure 4. Supply and demand factors, U.S. cannabis market 2005-2018

The figure below graphs the time variation in cannabis seizures by weight (lbs./agent) performed by the United States Customs and Border patrol from 2005 to 2018 and the percentage of the U.S. population reporting any cannabis use of the month prior for the same period. Also graphed are extant miles of border fencing on the U.S.-Mexico border, also for 2005-2018, and the number of states with active recreational marijuana dispensaries from 2014 to 2018.

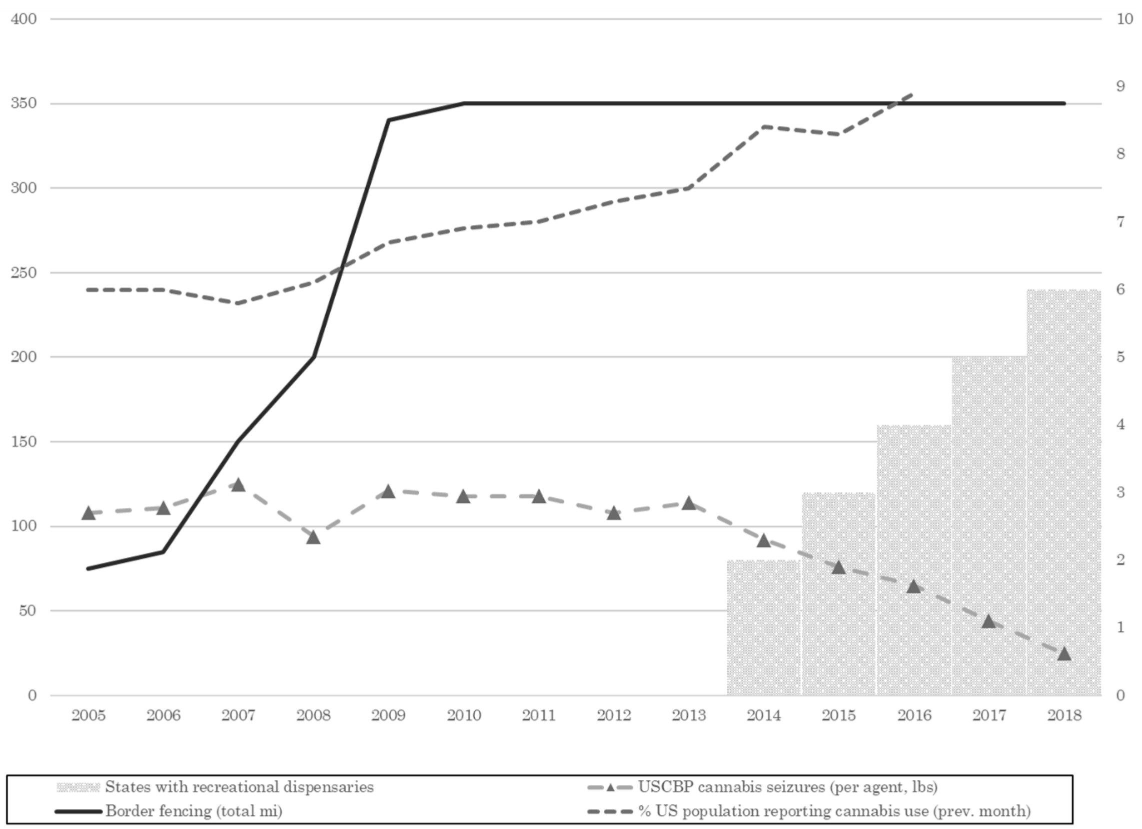
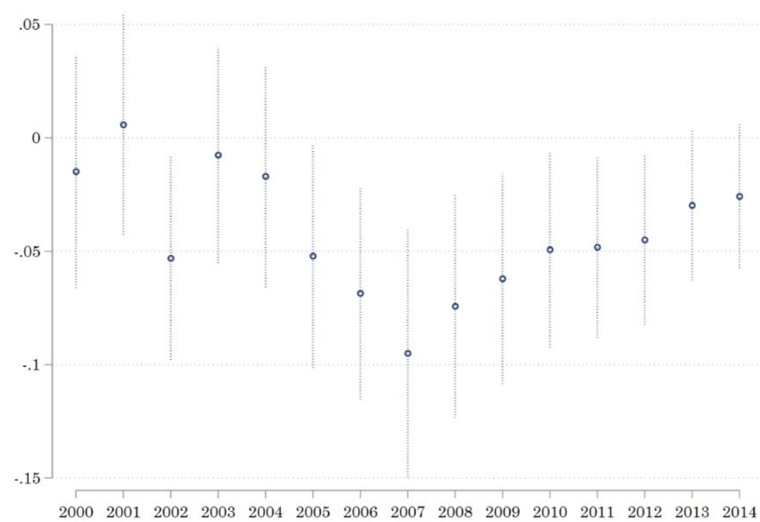


Figure 5. Coefficient dynamics, distance-to-border regressions

This set of figures shows estimated coefficients of interest for a set of regressions with the following general specification: $Deposits_{it} = \alpha_i + \gamma_t + \beta_1 Distance_{it} + \beta_2(Distance_{it} \times Post_\tau) + \varepsilon_{it}$
In these regressions, τ takes values in the sequence $[2000, 2001, \ldots, 2014]$. Each value of τ defines a regression estimation, and the calculated coefficient of interest for interest is plotted, together with its standard error. The years 1995-1999 are excluded from the exercise, although they form part of the estimation sample for the regression analyses presented elsewhere in the text. The reason for this is that the NAFTA came into effect on January 1, 1994 and had deep impacts on capital flows among North American nations. Hence, the early years in the sample period considered do not constitute an adequate "control" for the subsequent period. In Panel A above, $Distance_{it}$ represents the least geodesic distance from a bank branch to the Mexican border. In Panel B, $Distance_{it}$ represents the "distance-to-treatment" measure detailed in the Appendix. In Panel C above, $Distance_{it}$ represents the "distance-to-seizure" measure.

Panel A



Panel B

Panel C

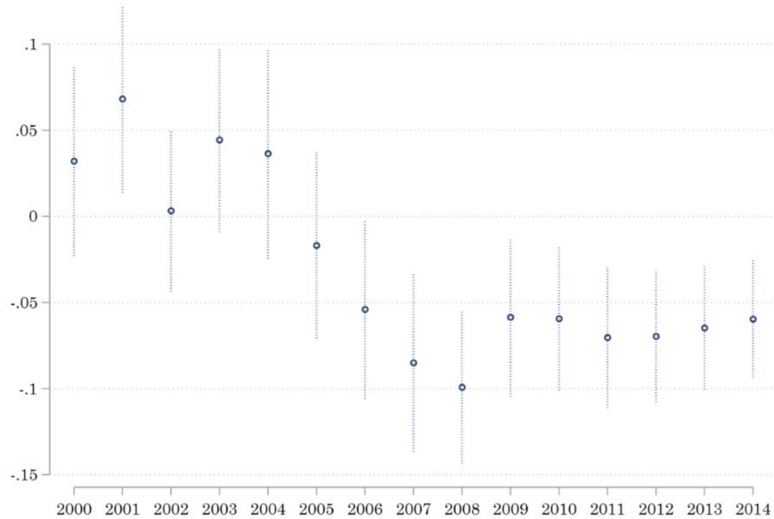


Figure 6. Active branches and total deposits, U.S. banking system 1997-2017

The figure above plots active bank branches and total bank deposits for the 1997-2017 period in the United States. Up to around 2007, both series covary closely, but this covariance breaks down thereafter, with the number of bank branches experiencing a slight decline over the next ten years.

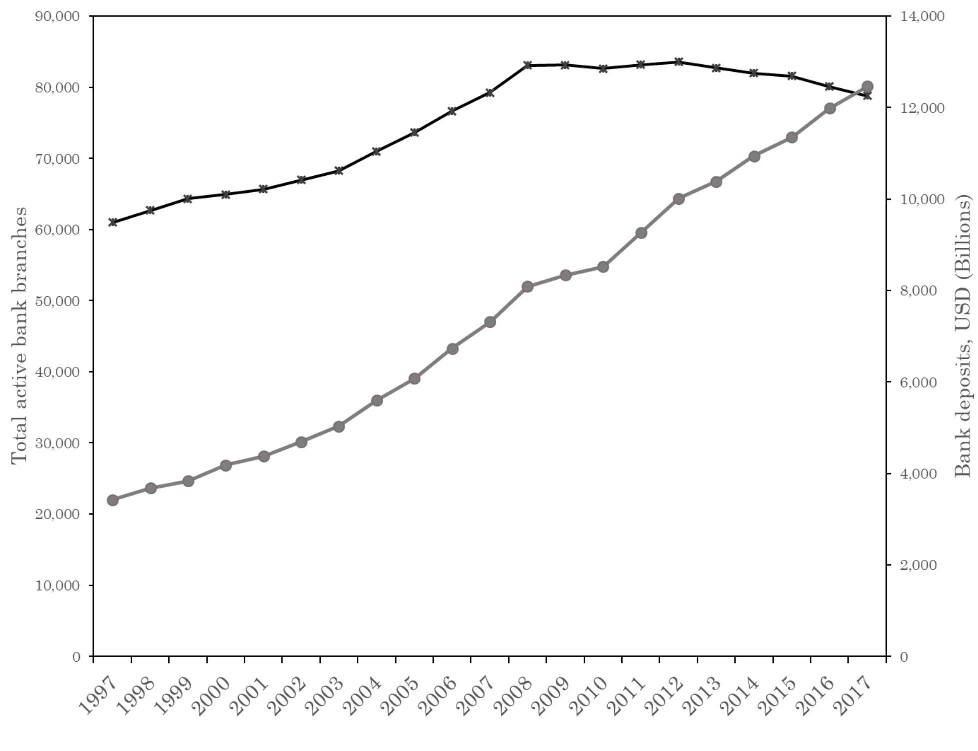


Table 1. Summary statistics

This table presents summary statistics of outcome variables and covariates for observational units in both empirical settings used in this paper. Panel A contains data for Mexico, at the municipality-year level. Panel B contains U.S. data, at the branch-year level.

Panel A: Mexico, municipality level

	Full sample			*Treatment sample*			*Control sample (never treated)*		
	Obs.	**Mean**	**Std. Dev.**	**Obs.**	**Mean**	**Std. Dev.**	**Obs.**	**Mean**	**Std. Dev.**
Bank branches	12,895	6	21	6,190	11	30	6,705	1	2
Credit card contracts (count)	13,286	9,798	60,099	6,453	19,262	85,133	6,833	861	3,795
Deposits (total: MXN, Millions)	12,978	990	5,090	6,284	1,760	6,800	6,694	269	2,410
Demand deposits (MXN, Millions)	12,978	562	2,640	6,284	995	3,440	6,694	156	1,450
Term deposits (MXN, Millions)	12,978	428	2,760	6,284	765	3,800	6,694	113	994
Population	11,904	37,572	43,109	5,235	58,837	51,517	6,669	20,879	24,501
Unemployment rate (%)	10,334	2.97%	2.34%	5,373	2.88%	1.78%	4,961	3.07%	2.82%
Homicides	11,904	5	9	5,235	8	12	6,669	2	4
Gov't expenditure (municipal)	10,866	85	116	5,046	127	148	5,820	50	56

Panel B: United States, bank branch level

	Obs	**Mean**	**Std. Dev.**
Metropolitan area dummy	30,594	0.952	
Micropolitan area dummy	30,594	0.038	
Distance from U.S.-Mexico border *(miles)*	30,396	42.936	82.973
"Distance to seizure" *(miles, weighted)*	27,879	627.514	132.974
"Distance to treatment" *(miles)*	17,163	972.697	371.143
Deposits *(USD, thousands)*	30,594	71,548	164,195

Table 2. Treatment effects on local banking outcomes, Mexico

The table below shows estimated coefficients for regressions of (**Panel A**) (logged) bank deposits, (**Panel B**) (logged) number of bank branches, and (**Panel C**) local lending (as proxied by the log number of active credit card contracts), on treatment measures. Binary treatment measures appear in columns (1)-(2) and binary/continuous measures in column (3), respectively. These regressions also contain full batteries of municipality fixed effects, and year fixed effects/control variables (year FE only: columns (1) and (3), year effects and controls: column (2)). Control variables included are (log) municipal population, unemployment rate, (log) number of homicides, and (log) annual municipal government expenditures. In untabulated regressions, (log) active credit card contracts is included as an additional control, with results remaining qualitatively unchanged. These regressions cover a sample period of 1995-2010; standard errors are clustered at the municipality level.

Panel A

	(1)	(2)	(3)
		ln(Deposits)	
Treated	**0.287*** **	**0.342*** **	**0.311*** **
	(0.088)	(0.0938)	(0.119)
Cartels			**-0.016**
			(0.070)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Yes	No
Observations	12,978	9,038	12,978
R-squared	0.323	0.398	0.323
Number of clusters (municipalities)	2,379	2,240	2,379

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B

	(1)	(2)	(3)
		ln(Credit cards)	
Treated	**-0.171** **	**-0.182** **	**0.235** **
	(0.082)	(0.090)	(0.108)
Cartels			**-0.277*** **
			(0.051)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Yes	No
Observations	13,286	9,218	13,286
R-squared	0.639	0.671	0.641
Number of clusters (municipalities)	2,416	2,276	2,416

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel C

	(1)	(2)	(3)
		ln(Branches)	
Treated	**0.117*****	**0.071*****	**-0.006**
	(0.017)	(0.016)	(0.023)
Cartels			**0.084*****
			(0.013)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Yes	No
Observations	12,895	8,851	12,895
R-squared	0.513	0.442	0.520
Number of clusters (municipalities)	2,416	2,276	2,416

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Treatment-on-the-treated effects on local banking outcomes, Mexico

The table below shows estimated coefficients for regressions of (logged) bank deposits, (logged) number of bank branches, and local lending (as proxied by the log number of active credit card contracts), on treatment measures. These regressions are run exclusively on the "assigned-to-treatment" subsample, and contain full batteries of municipality fixed effects, and year fixed effects/control variables (year FE only: columns (1), (3), and (5); year effects and controls: columns (2), (4), and (6)). Control variables included are (log) municipal population, unemployment rate, (log) number of homicides, and (log) annual municipal government expenditures. In untabulated regressions, (log) active credit card contracts is included as an additional control, with results remaining qualitatively unchanged. All regressions cover a sample period of 1995-2010; standard errors are clustered at the municipality level.

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Deposits)		*ln(Branches)*		*ln(Credit cards)*	
Treated	**0.138***	**0.179****	**0.033****	**0.026***	**0.059**	**-0.036**
	(0.072)	(0.085)	(0.015)	(0.016)	(0.080)	(0.091)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Observations	6,284	4,234	6,190	4,083	6,453	4,328
R-squared	0.484	0.523	0.595	0.518	0.692	0.707
Number of clusters (municipalities)	717	623	737	642	737	642

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Trend analysis, Mexico

This table presents results of regressions of logged outcomes on a treatment dummy (Column (1)), and this dummy with: (i) a dummy which turns on in years -1 *and* -2 around *first* treatment and is zero for every other year (Column (2)); (ii) a dummy which turns on *only* in year -1 (Column (3)); and (iii) a dummy which turns on *only* in year -2 (Column (4)), respectively. Panel A contains results for deposits, Panel B for bank branches, and panel C for credit cards.

Panel A

	(1)	(2)	(3)	(4)
		ln(Deposits)		
Treated	**0.287*** **	**0.319*** **	**0.310*** **	**0.292*** **
	(0.088)	(0.098)	(0.092)	(0.091)
Pre-period dummy (t = -1 OR t = -2)		**0.077**		
		(0.059)		
Pre-period dummy (t = -1)			**0.085**	
			(0.077)	
Pre-period dummy (t = -2)				**0.033**
				(0.052)
Municipality FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	12,978	12,978	12,978	12,978
R-squared	0.323	0.323	0.323	0.323
Number of code	2,379	2,379	2,379	2,379

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B

	(1)	(2)	(3)	(4)
		ln(Branches)		
Treated	**0.117*** **	**0.122*** **	**0.122*** **	**0.117*** **
	(0.017)	(0.021)	(0.019)	(0.017)
Pre-period dummy (t = -1 OR t = -2)		**0.013**		
		(0.019)		
Pre-period dummy (t = -1)			**0.020**	
			(0.019)	
Pre-period dummy (t = -2)				**-0.000**
				(0.015)
Municipality FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	12,895	12,895	12,895	12,895
R-squared	0.513	0.513	0.513	0.513
Number of code	2,416	2,416	2,416	2,416

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel C

	(1)	(2)	(3)	(4)
		ln(Credit cards)		
Treated	**-0.171****	**-0.171***	**-0.179***	**-0.167****
	(0.082)	(0.096)	(0.091)	(0.083)
Pre-period dummy (t = -1 OR t = -2)		**-0.001**		
		(0.091)		
Pre-period dummy (t = -1)			**-0.030**	
			(0.093)	
Pre-period dummy (t = -2)				**0.030**
				(0.083)
Municipality FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	13,286	13,286	13,286	13,286
R-squared	0.639	0.639	0.639	0.639
Number of clusters (municipalities)	2,416	2,416	2,416	2,416

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Municipal characteristics and cartel presence

This table presents results for estimations of regression models with the general specification $Treated_{it} = \alpha_i + \gamma_t + \boldsymbol{X_t}\beta + \varepsilon_{it}$, where i indexes municipalities and t indexes years. In Column (2), the binary variable $Treated_{it}$ is replaced with the continuous measure of treatment $Cartels_{it}$, a count variable which registers the number of cartels active in municipality i for year t. Both models reported contain a full set of municipality- and year fixed effects. The unit of observation is the municipality-year; standard errors are clustered at the municipality level.

	(1)	(2)
	Treated	*Cartels*
"Marginalization index"	**-0.007**	**-0.014**
	(0.006)	(0.012)
ln(Gov't expenditure)	**0.011**	**0.012**
	(0.010)	(0.018)
Unemployment rate (%)	**0.257**	**0.447**
	(0.180)	(0.340)
ln(Population)	**0.004**	**0.017**
	(0.008)	(0.017)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Observations	8,355	8,355
R-squared	0.330	0.319
Number of code	2,185	2,185

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Treatment effect dynamics

This table reports coefficients for estimations of the equation $ln(Deposits_{it}) = \alpha_i + \gamma_t + \beta_2(Treated_{it} \times \mathbb{1}_\tau) + \varepsilon_{it}$ for $\mathcal{T} = \{2006, \dots, 2009\}$. Column (1) reports average treatment effect (ATE) coefficients, while Column (2) reports ToT estimates. Both models contain municipality and year fixed effects. Standard errors are clustered at the municipality level; the unit of analysis in these models is the municipality-year.

	(1)	(2)
	ln(Deposits)	
	ATE	ToT
Treated	**0.170****	**0.194*****
	(0.070)	(0.070)
Treated × 2006	**-0.123****	**-0.179*****
	(0.061)	(0.062)
Treated × 2007	**-0.042**	**-0.008**
	(0.082)	(0.118)
Treated × 2008	**0.240**	**-0.185**
	(0.211)	(0.210)
Treated × 2009	**0.217**	**-0.120**
	(0.195)	(0.261)
Treated × 2010	**0.375***	**0.173**
	(0.192)	(0.308)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Observations	12,978	6,284
R-squared	0.324	0.485
Number of clusters (municipalities)	2,379	717

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7. Differences-in-differences regressions, bank deposits in border counties (U.S.)

This table shows estimated coefficients for differences-in-differences regressions of (logged) bank deposits on the interaction of a measure of distance and post-2006, and post-2012 indicator variables (and corresponding factorial terms). The models presented are fully saturated with a full set of branch and year fixed effects. The unit of observation in these models is the branch; standard errors are clustered at this level. Although the coefficient on $Distance_{it}$ is not in a strict sense identified, year-to-year changes in the physical location of branches introduces slight time variation in this (cross-sectional) variable. Branches retain their unique identifier even when their street address changes due to bank commercial strategy or acquisition by a different banking institution.

	(1)	(2)	(3)
		ln(Deposits)	
Distance	**0.046**	**0.026**	**0.047**
	(0.070)	(0.070)	(0.069)
Post-2006 × *Distance*	**-0.068*** **		**-0.076*** **
	(0.024)		(0.023)
Post-2012 × *Distance*		**0.001**	**0.024**
		(0.019)	(0.017)
Branch FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	5,713	5,713	3,342
R-squared	0.147	0.147	0.147
Number of clusters (branches)	312	312	309

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Differences-in-differences, bank-level profitability (U.S.)

The table above shows estimated coefficients for differences-in-differences regressions of a vector of (logged) indicators of bank-level profitability on the interaction of a measure of bank exposure to drug-cartel activity and $Treatment\ period_t$, an indicator variable which turns on post-2006 and off again post-2012 (and corresponding factorial terms). The measure of exposure used is the fraction of branches for a banking institution that lie within counties straddling the Mexican border, as a proportion of all branches in the U.S. The models presented are fully saturated with a full set of bank and year fixed effects. The unit of observation in these models is the bank; standard errors are clustered at the bank level.

	(1) ln(Deposits)	(2) Net Income/Assets	(3) Interest Expense/Assets	(4) Noninterest Income/Assets
Exposure	**0.890*** **	**-0.002**	**0.000**	**0.002**
	(0.314)	(0.005)	(0.001)	(0.003)
Exposure x Treatment period	**-0.167**	**0.005*** **	**0.001**	**-0.002**
	(0.112)	(0.002)	(0.001)	(0.001)
Small	**0.192**	**-0.006**	**0.001**	**-0.001**
	(0.305)	(0.005)	(0.002)	(0.003)
Small x Exposure	**-1.159*** **	**-0.004**	**-0.002**	**-0.000**
	(0.386)	(0.006)	(0.002)	(0.004)
Small x Treatment period	**-1.074* **	**-0.018** **	**0.006** **	**-0.003**
	(0.603)	(0.008)	(0.003)	(0.002)
Small x Exposure x Treatment period	**1.185* **	**0.020** **	**-0.008** **	**0.005* **
	(0.626)	(0.008)	(0.003)	(0.003)
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,062	2,062	2,062	2,062
R-squared	0.273	0.280	0.896	0.357
Number of clusters (banks)	234	234	234	234

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9. Differences-in-differences, active bank branches in border counties

The table below shows estimated coefficients for differences-in-differences regressions of the (log) number of active bank branches on the interaction of a measure of distance and a vector of indicator variables that "turn on" in 2006 and 2012, respectively. The models reported above contain also corresponding factorial terms. The models are fully saturated with a full set of county and year fixed effects. The unit of observation in these models is the ZIP code-year; standard errors are clustered at the ZIP code level.

	(1)	(2)	(3)	(4)	(5)
			ln(Branches)		
Measure:	Geodesic		"Distance to treatment"	"Distance to seizure"	
Distance	*(Absorbed)*		**0.006**	**-0.099*** **	**-0.099*** **
			(0.006)	(0.028)	(0.028)
Post-2006 × Distance	**-0.047*** **	**-0.037*** **	**-0.016*** **	**-0.029** **	**-0.025** **
	(0.014)	(0.013)	(0.005)	(0.012)	(0.011)
Post-2012 × Distance		**-0.019**			**-0.009**
		(0.012)			(0.008)
ZIP Code FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	5,713	5,713	3,342	5,495	5,495
R-squared	0.116	0.117	0.137	0.129	0.130
Number of clusters (ZIP codes)	312	312	309	317	317

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10. Probability models, bank enforcement actions 1995-2017

This table presents results for estimations of the equation $\mathbb{1}\{Enforcement_{it}\} = \beta\left(ln(Deposits_{it}^{Border}) \times Post_{2006}\right) + \boldsymbol{X}\theta + \varepsilon_{it}$ where i indexes banks, and t indexes years. $Deposits_{it}^{Border}$ is the quantity of deposits a bank holds in the 24 counties straddling the Mexican border; the sample considered for this test is of all banks with branches in AZ, CA, NM, and TX for the period spanning 1995-2017. Results in Column (1) are for a model with bank fixed effects; results in (2) are for a model with bank- and year fixed effects. Both models include regulator fixed effects, as well as a bank-asset decile control.

	(1)	(2)
	Dependent variable: Enforcement (binary)	
ln(Deposits) × Post-2006	**0.017*** **	**0.022*** **
	(0.005)	(0.005)
ln(Deposits)	**0.022*** **	**-0.002**
	(0.007)	(0.006)
Post-2006	**-0.137** **	
	(0.055)	
Bank asset decile	**-0.023** **	**-0.007**
	(0.009)	(0.009)
Regulator		
FED	**-0.033**	**-0.059** **
	(0.029)	(0.030)
OCC	**0.122*** **	**0.116*** **
	(0.036)	(0.038)
OTS	**-0.004**	**0.010**
	(0.057)	(0.057)
Bank FE	Yes	Yes
Year FE	No	Yes
Observations	19,725	19,725
R-squared	0.035	0.060
Number of clusters (banks)	2,138	2,138

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11. Probability models, municipal election results, 2000-2017

This table presents the results of estimations of the regression equation

$$\mathbb{1}\{Incumbent\ loss\}_{it} = \alpha_i + \gamma_t + \beta(Treated_{it} \times Deposits_i^{2006} \times Post_{2006}) + \boldsymbol{X}\theta + \varepsilon_{it}$$

where i indexes municipalities and t indexes years, and $\boldsymbol{X}$ is a vector of factorial terms associated with the interaction term $Treated_{it} \times Deposits_i^{2006} \times Post_{2006}$, for a repeated cross-section of municipal elections spanning 2000-2017. Column (1) contains the results of a linear probability estimation, while Column (2) reports results of a panel logistic-regression estimation. Both models are saturated with municipality- and year fixed effects and contain party dummies for the three major political parties in Mexico during the sample period (PAN, PRI, PRD). The unit of observation is the municipality-year; reported standard errors are clustered at the municipality level for the LPM, and heteroscedasticity-robust for the logit.

	(1)	(2)
	Incumbent voted out = 1	
Estimation:	*LPM*	*Panel logit*
Treated	**0.620**	**3.544**
	(0.808)	(3.945)
Deposits, log 2006 level	**0.124**	**0.871**
	(0.102)	(0.872)
Treated × Deposits	**-0.031**	**-0.171**
	-0.038	(0.189)
Treated × Post-2006	**-1.713***	**-9.651****
	(0.901)	(4.485)
Deposits × Post-2006	**-0.063****	**-0.341****
	(0.030)	(0.153)
Treated × Deposits × Post-2006	**0.088****	**0.492****
	(0.043)	(0.220)
Observations	1,880	1,373
R-squared	0.063	
(Pseudo) R-squared		0.033
Number of clusters (municipalities)	544	373

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

10 Appendices

Appendix A: Cartel activity data

Coscia & Ríos (2012) develop an algorithm to identify criminal organizations active in Mexican municipalities at yearly frequency, for a sample spanning 1990-2010. The algorithm of Coscia and Ríos queries a pre-defined set of 176 cartel and cartel-member names ("actor terms"), including aliases, in online news sources archived by Google News, using the API provided by Google for mass data retrieval, deprecated in late 2010.

Then, the algorithm repeats this exercise with municipality names and place names associated with municipalities and tallies the co-occurrence of these terms with the "actor terms," to construct a matrix of pairwise occurrence frequencies. When names are approximate or exact duplicates, auxiliary search terms are added to the municipality query to disambiguate results.

To flag a municipality-year as "treated," the algorithm then follows a sequence of steps. First, it calculates the probability of observing a given joint frequency by mere chance, using the hypergeometric probability distribution as the null model. More precisely, the following ratio is calculated (i.e. the hypergeometric probability mass function, PMF_{HG}).

$$PMF_{HG} = P(t_i) = \frac{\binom{T_i}{t_i}\binom{\mathcal{M} - T_i}{m_i - t_i}}{\binom{\mathcal{M}}{m_i}} \qquad (A1)$$

where t_i is the calculated frequency of a given "actor term" within a municipality m, T_i is the *total* number of hits obtained for this given "actor term," m_i is the total number of hits for municipality m, and $\mathcal{M}$ is the total number of hits obtained, irrespective of set. Then, the cumulative probability of obtaining t_i *or less* results, (that is, the cumulative hypergeometric distribution function, CDF_{HG}) is calculated:

$$CDF_{HG}(x \leq t_i) = \sum_{x=0}^{t_i} PMF_{HG}(x) \tag{A2}$$

Finally, a municipality-year is marked as “treated” if this cumulative probability exceeds an arbitrary threshold of 0.95. The final sample constructed by Coscia and Ríos contains 2,449 municipalities and 176 “actor terms,” representing thirteen drug cartels, which are then aggregated into seven wider cartel “families,” and a residual category.

To validate their measure, Coscia and Ríos perform two exercises: first, they compare the geographies marked as areas of influence for a given cartel by the algorithm to cartel areas of influence identified by the Mexican government, yielding a high degree of overlap. Second, they use the algorithm to identify municipalities “associated” with the incumbent governor of the State the municipality resides in. The algorithm can pair municipality names with the name of State governors with a high degree of precision.

Appendix B: Construction of alternative distance measures

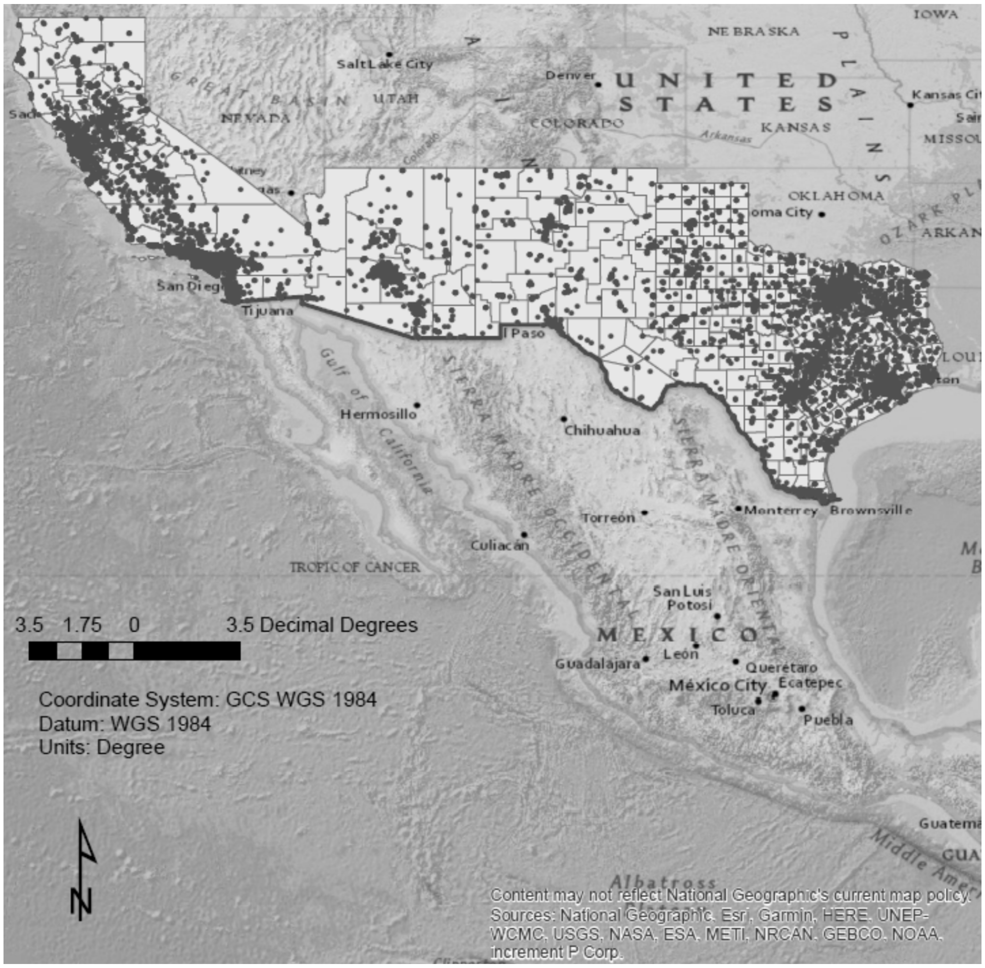
As a robustness check, as an alternate measure of exposure to the Mexican drug trade, I construct a measure of least distance to the geographic center of drug-cartel activity for the sample period 1995-2010. I use this measure in tests that complement parsimonious regressions using least distance to the border as a proxy of exposure to treatment.

To construct this measure, I use variation in cartel presence in Mexican municipalities as measured using the data of Coscia and Ríos (2012). For each year $t \in \{1995, \dots, 2010\}$, I flag a municipality as treated iff at least one cartel is found to operate within. Once I have determined the set $D_t = \{1, \dots, J\}$ of treated municipalities for a given year, I calculate the geographic centroid of each element j in this set, using shape-files available on the ESRI ArcGIS public repository. I thus obtain the set $D_t^c = \{c_t^1, \dots, c_t^J\}$ of centroid coordinates for each of the elements of D_t.

I then calculate the geodesic distance[44] between each element of this set of centroid coordinates and each zip code used in the regression models presented in **Table 5**. Hence, if each year t a set Z_t of zip codes appears in the corresponding regression, I will obtain a set $J \times Z_t$ of distances. Lastly, for every (i-th) element of Z_t I calculate the average distance, which reduces the number of elements of this set to only $\#Z_t$. Each element of this set I call a "distance-to-treatment."

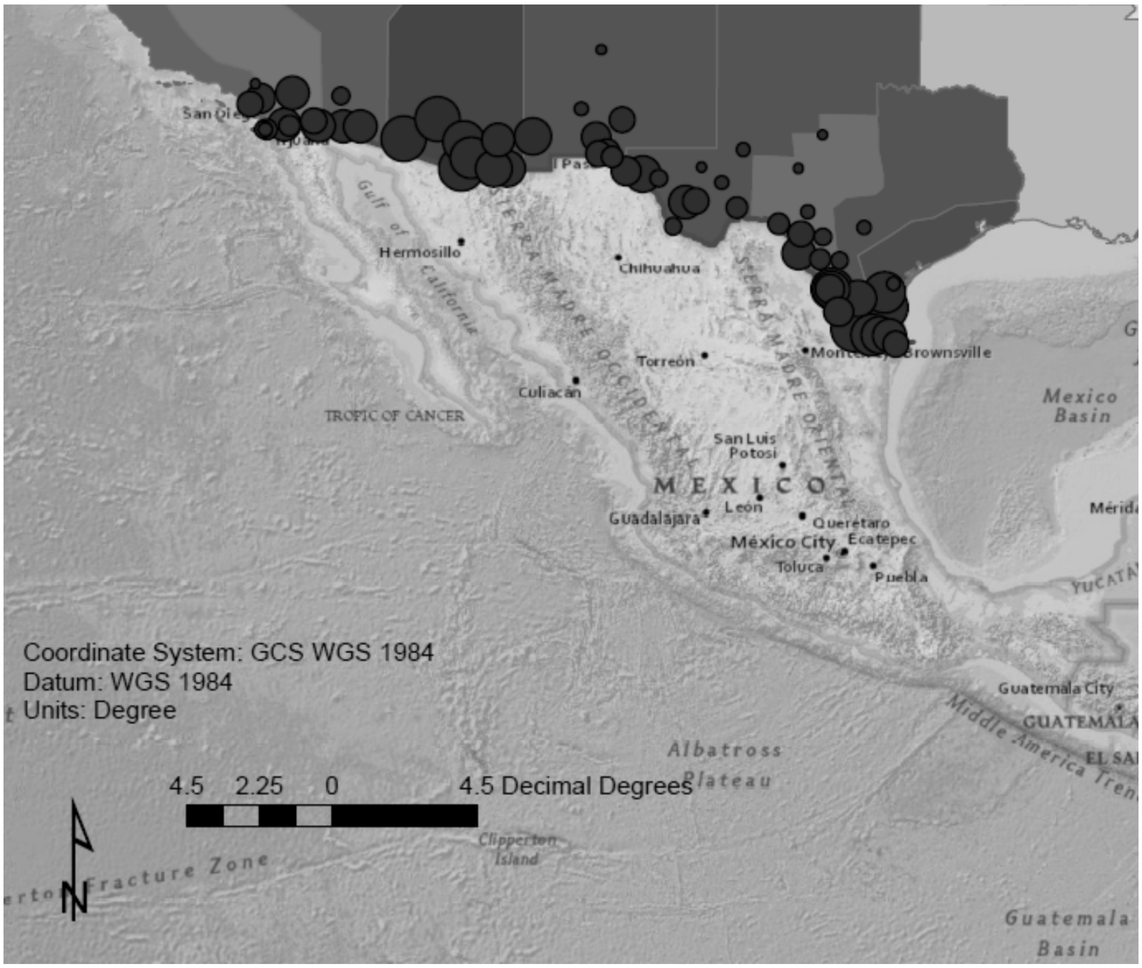
[44] See Vincenty, T. (1975).

Figure A1. Retail bank branch locations in Border States



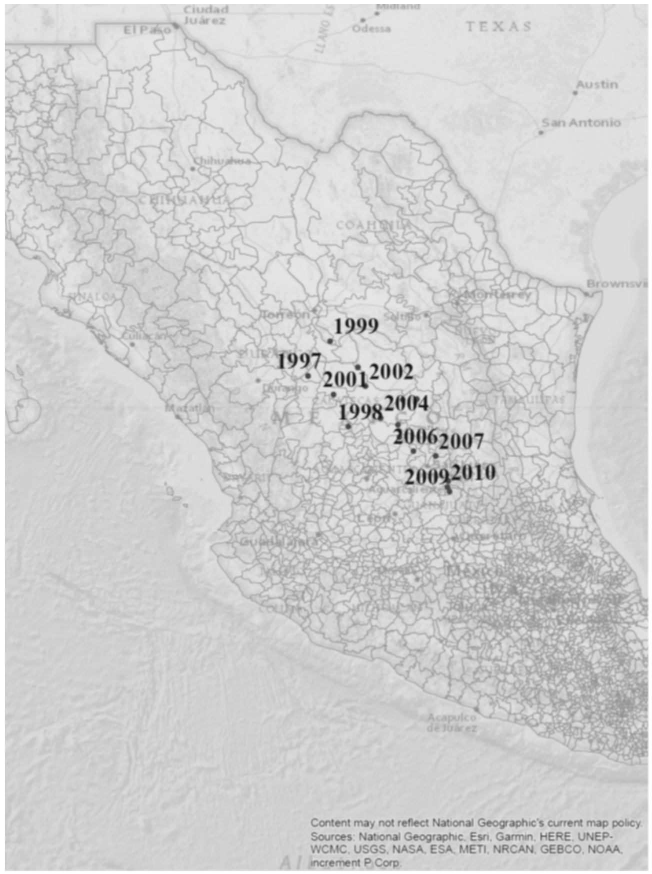
This figure shows all retail-bank branches in Arizona, California, New Mexico, and Texas (all lightly shaded) as of December 2015. Although the scale of the map is such that observing individual branches is impossible, it does identify areas of heightened banking activity. The border itself appears darkly bolded.

Figure A2. Drug seizures by USCBP



This figure shows dollar-value weighted drug seizures at U.S. Customs and Border Patrol (USCBP) checkpoints from 2011 to 2017. Drug seizures used for the elaboration of this figure are solely of cannabis (marijuana) and cocaine. Drug price data was retrieved from a variety of sources, including time series published by the UN Office for Drugs and Crime (UNODC) and prices retrieved from drug retail sites on the Dark Web. USCBP checkpoints include all points of entry into the United States along the Mexican border, as well as static checkpoints along highway transport corridors in the interior of the country. These checkpoints are identified as small, dark circles in the map above. Circle radii are proportional to the (log) dollar value of drugs seized at that checkpoint. Border States (AZ, CA, NM, and TX) are identified by shading in various tones of gray, which vary in tone by USCBP sector. Tonalities of gray represent the quintile to which the sector pertains in the cross-sectional distribution of undocumented-alien detentions for the same sample period. As can be seen, there is no obvious correlation between drug seizures and alien detentions in the small cross-section of sectors presented.

Figure A2. Geographic center of cartel activity, 1995-2010



This figure shows the shifting geographic center of drug-cartel activity in Mexico from 1995 to 2010. This map is constructed using data on local treatment as determined by the measure of Coscia & Ríos (2012).

In addition to the aforementioned "distance-to-treatment" measure, I also calculate a "distance-to-seizure" as an alternative metric of exposure to criminal activity. This measure is constructed as follows: for each point of entry into the United States from Mexico, as well as from each USCBP checkpoint in the interior of the U.S. and lying within one of the States straddling the southern border (AZ, CA, NM, TX), I retrieve information on drug seizures from 2011 to 2017.

Data on drug seizures made public by USCBP consists of tonnage seized of cocaine and cannabis (marijuana) by checkpoint and year. I am thus able to construct a checkpoint-specific panel of seizures for each of these two categories of illicit drugs. Once I have constructed this two-dimensional panel, I weigh each data-point, respectively, by the going price of cocaine or marijuana in that year. Data for the prices of illicit drugs was pieced together from several sources, including notably data provided by the UNODC, as well as from drug-retail websites on the Dark Web.

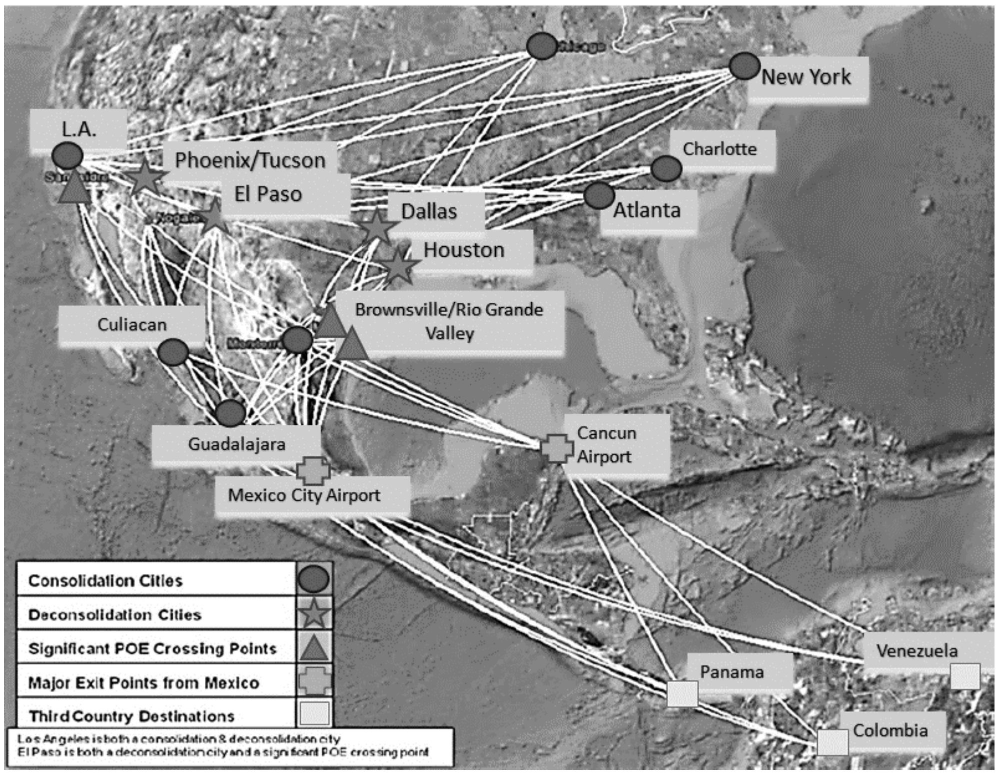
Once I have done this, I can calculate for each unit of observation i the following measure:

$$d(t)_i^s = \sum_j d_{i \to j} \times (p_t^m q_t^m + p_t^c q_t^c) \qquad (A3)$$

The equation above gives the general form of the "distance-to-seizure" measure. In this expression, $d_{i \to j}$ symbolizes the geodesic distance between the unit of observation i and checkpoint j, and p_t^k the time-t price of drug k (with quantities q indexed analogously).

Appendix C: Additional figures

Figure A4. Nodes and edges in the US-Mexico cash-smuggling network



This figure shows cities that operate as consolidation, deconsolidation, and destination nodes in the cash smuggling operations of organized crime syndicates operating between the United States and Mexico. Cash from illicit activities originates –and is prepared for transport—in cities highlighted in red, then moving to cities highlighted in yellow for "layering" and packaging for cross-border shipping. Flowing through border cities (highlighted in teal), cash is then re-consolidated on the Mexican side of the border in cities highlighted in red. Exit points from Mexico are highlighted in green: cash payments to drug producers and to fiscal havens are traced out from these points into Central and South America.

Source: Department of Homeland Security

Figure A5. MXN/USD exchange rate, 2000-2012

This figure plots the daily spot MXN/USD market rate from 01/01/2000 to 12/31/2012. There are no discernible large-scale swings around 2006 that might be driving results presented in this paper.

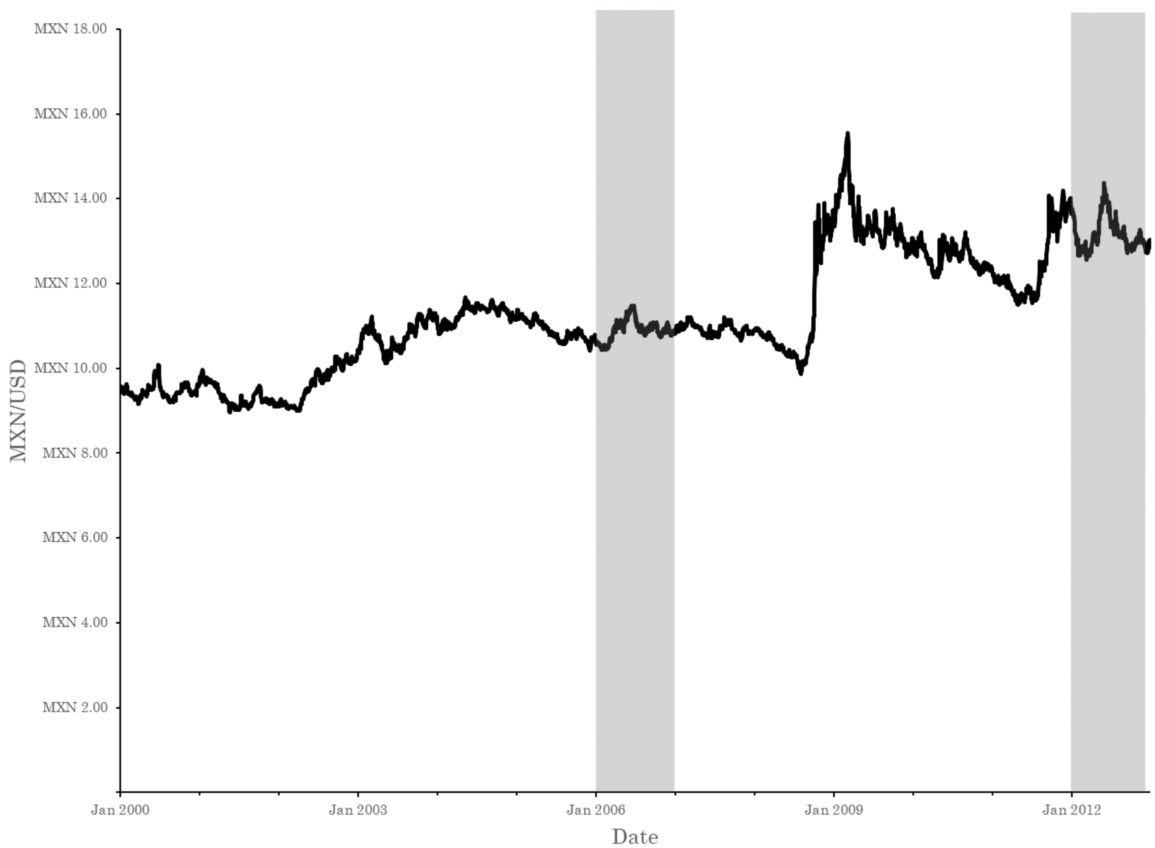


Figure A6. Illegal Border crossing arrests, USCBP Southwestern Sectors 2000-2014

This figure plots monthly arrests made by the U.S. Customs and Border patrol for misdemeanor (unauthorized) border crossings in all nine patrol sectors straddling the Mexican border. There are no discernible large-scale variations in this statistic, ostensibly a proxy for the growth rate of illegal immigration from Mexico to the U.S., and thus of migrant remittances, which could be driving results reported in this paper for the period around 2006.

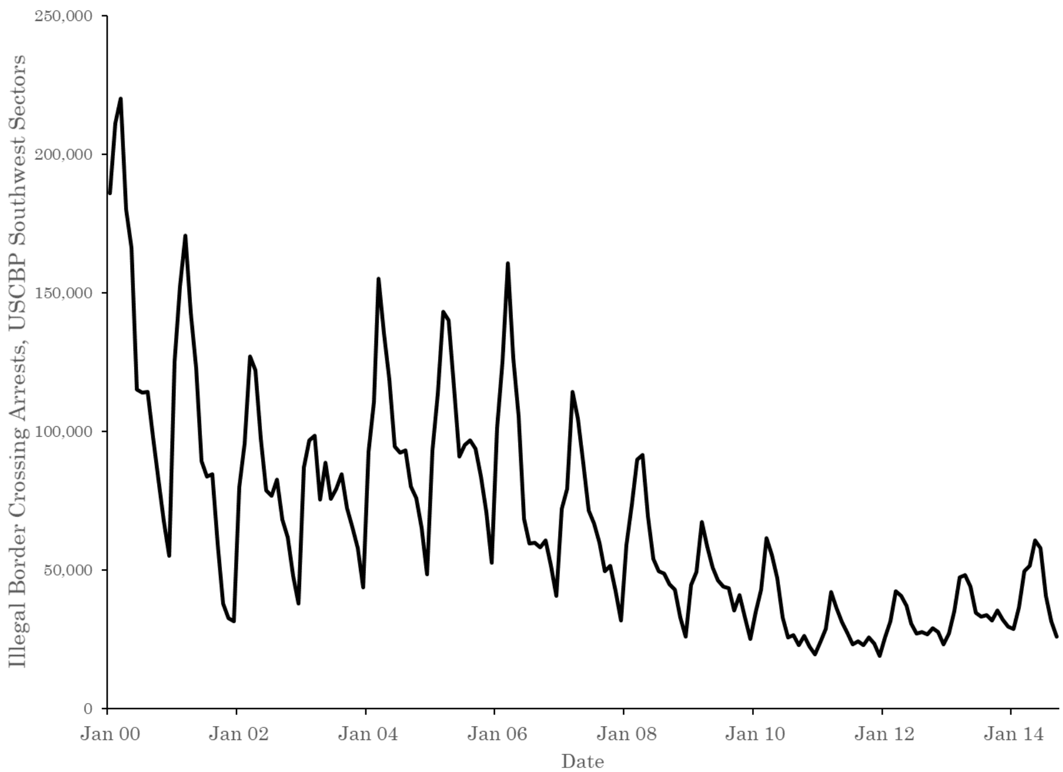
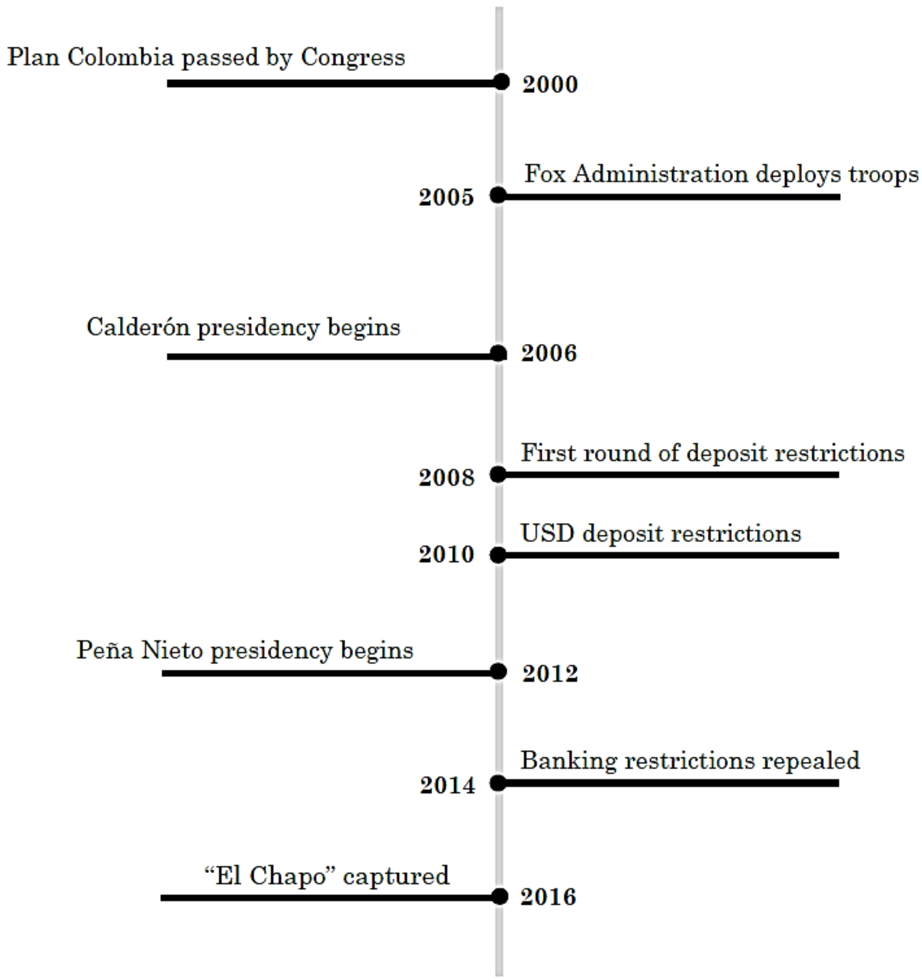


Figure A7: Timeline of the illicit drug trade in Mexico



Appendix D: Additional institutional detail

A. The HSBC laundering case

The particulars of this case suggest that HSBC executives anticipated a low chance of prosecution, or a lenient penalty if prosecuted. Interactions with drug cartel elements were quite brazen, with traffickers depositing "hundreds of thousands of dollars in bulk U.S. currency each day into HSBC Mexico accounts."[45] To place this cash efficiently through the bulletproof glass of teller windows at HSBC Mexico branches, drug traffickers used custom-built boxes, tailored to the precise dimensions of teller window-holes.[46]

B. The rise of Mexican cartels

By the mid-2000s, Mexican drug trafficking organizations had "established overland transportation networks to transport cocaine, marijuana, methamphetamine, and heroin" robust enough to become the incumbent players in cocaine wholesale shipments into the United States (National Drug Intelligence Center, 2006). This rapid ascent in preeminence can be explained in significant measure by the displacement of the Colombian cartels, the market incumbents up to this point. Castillo, Mejía, & Restrepo (2014), using high-frequency administrative data, document how cocaine seizure activity in Colombia led to increases in the street price of narcotics in final consumer markets. This contributed in turn to both a rise in the profit margins of Mexican cartels and to increased violence in Mexico, as market entry by new competitors led to confrontations. By 2010, the leading Mexican cartel (*Sinaloa*, headed by the notorious Joaquin "*El Chapo"* Guzman) was generating an annual profit estimated at $3 billion, a figure comparable to the contemporaneous net income of Netflix or Facebook (Radden Keefe, 2012). Violence, on the other hand, reached a febrile pitch by 2005, as turf wars between the Sinaloa and Gulf cartels over border emplacements boiled over

[45] United States DOJ Case 1:12-cr-00763-ILG, Attachment A.
[46] *Ibid.*

(Molzahn, Ríos & Shirk, 2012). In response to this spike in violence, the administration of President Vicente Fox, in an unprecedented act, deployed a small contingent of troops to the border city of Nuevo Laredo in the summer of 2005.

C. Money laundering and illicit financial flows

In practice, the three phases of money laundering are often intertwined: for instance, a criminal organization might set up "fronts," businesses with no economic purpose other than being a façade for illicitly produced cash flows.[47] These businesses might hold accounts at banking institutions, and deposit these cash streams in them, disguised as revenue from operations. In this example, placement and layering would happen concurrently.

According to the Financial Action Task Force (on Money Laundering) (FATF), the preeminent intergovernmental AML organization, "the amount of proceeds generated by predicate crimes committed in and outside of Mexico is high" and "proceeds derived from foreign predicate crimes [are] mostly related to Mexican transnational organized crime" (FATF, 2018). It follows that, in principle, the level of money laundering activity in Mexico is quite high.

Laundering of the cash flows produced by Mexican criminal organizations is not confined to Mexico, however. In 2017, the Dutch bank Rabobank admitted to multiple instances of wrongdoing committed by its U.S. affiliate, reaching a $369M settlement with the DOJ. In the complaint presented against Rabobank, U.S. attorneys document how bank executives sought to enhance the operations of "star" branches in the Imperial Valley of California, flush with cash deposits from Mexican cartels, by downgrading and selectively enforcing the AML controls the bank had in place. This forbearance effectively provided a safe harbor for the laundering of millions of dollars of drug-trafficking revenue.[48]

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[47] Apart from "front" businesses, common methods of "layering" are phantom invoicing, particularly in international trade, and "smurfing," in which large deposits are fractioned into multiple smaller accounts, spread over a wide number of financial instruments and/or institutions.

[48] United States District Court, Southern District of California. Criminal Complaint, Title 18, USC Sec. 371: Case 18-cr-0614-JM (United States v. Rabobank National Assoc.), Filed Feb. 07, 2018.

The above anecdote illustrates a particular "use case" for "illicit financial flows." By this term, in this paper I mean capital or monetary transfers, across national borders, of cash flows obtained from illegal activity. If Mexican cartels repatriate a fraction of their profits, once these monetary proceeds had been laundered through Rabobank, a next step was to somehow move these "newly clean" deposits across the Mexican border.

The mechanisms by which drug cartels might have accomplished this are many. However, the anecdotes point to two major modes of operation: first, "smurfing," in which many deposits (under the USD $10,000 BSA reporting threshold) are made, either at several banks, or serially by several distinct individuals (ACAMS, 2014). Alternatively, cartel operatives might liaise with bank officers directly, agreeing to kickbacks in return for forbearance in AML controls (U.S. Attorney's Office, Central District of California, 2019).

Bank Branch Networks, Banking Relationships, and Organized Crime

Abstract

In this paper, I explore if banks develop long-standing implicit contracts with criminal organizations, exploiting spatial variation in drug-cartel activity. I use Mexico, where local banking markets have been differentially exposed to this activity, as an empirical laboratory. I test whether banks with prior exposure to criminal activity are more likely to enter areas where cartels operate, as well as whether previous exposure to specific cartels predicts entry into local banking markets which said cartels have entered. Results suggest that certain banks do establish these: bank characteristics that have significant effects on differential behavior regarding "relationship-like" interactions with organized criminal organizations are domestic majority equity ownership and bank size.

1 Introduction

This paper seeks to answer the following questions: i. do banks establish implicit long-term contracts (Elsas, 2005) –analogous to relationship-lending interactions documented in the literature—with criminal agents in lax regulatory environments? ii. if so, what types of banks exhibit this behavior?

The results I present in this paper suggest that, in the setting explored, certain banks do establish these "relationships." Other banks appear to actively choose to operate in regions rife with organized crime, perhaps seeking deposit windfalls, but not to establish long-standing implicit contracts with criminal organizations. Bank characteristics that have significant effects on differential behavior regarding organized criminal organizations are, in this setting, foreign vs. domestic majority equity ownership, as well as size. Results regarding the effect of having been designated as systemically important by bank regulators are inconclusive. Namely, the response of *large*, *domestically held* banks to prior organized criminal activity in terms of branch expansion in cartel-occupied areas is around 28 percentage points higher than that of other banks, and the response of domestic banks in general to previous "relationship-like" interactions is around 20 percentage points higher than that of foreign banks.[49]

In the previous paper of this dissertation (Aldama-Navarrete, 2019), I have presented evidence that banks actively choose to enter markets in which organized crime operates, despite the excess exposure to regulatory and operational risk this exposes them to. Although this evidence is *consistent* with banks operating with intent to facilitate financial services to criminal organizations, by no means is it a "smoking gun." A priori, there are many narratives consistent with these findings; in particular, cartel entry might be endogenous to the very drivers of bank market entry. It may also be the case that banks are not particularly good at assessing this excess risk burden, and that they enter these markets "chasing" deposits without correctly trading off expected costs and benefits. Lastly, banks might passively cooperate with organized crime, by either choosing to internalize "risk rents" by turning a blind eye to the nature of the operations carried out by criminal agents, or by acquiescing to the provision of financial services to these parties under duress, such as threats to the well-being of their employees or physical capital.

It remains relevant, however, to determine whether financial institutions willingly engage with criminal groups, or even –without intent—act *as if* they did, establishing long-standing working relationships with these. For one, answering these questions is of high relevance to policymakers in determining the optimal course of bank supervision policy. From an academic standpoint, investigating whether banks form relationships with economic actors that are *extremely* opaque informationally contributes to our understanding of what makes banks special.

The well-known and numerous literature on relationship banking, beginning with Petersen and Rajan (1994) and Berger and Udell (1995), has long argued that one of the principal ways

[49] See tables (5) and (3), respectively.

in which banks are unique is their capacity to liaise with firms and individuals for whom information is costly to come by. According to this view, banks produce information about these agents' credit risk, using "soft inputs" garnered through lengthy relationships. This alternative information allows banks to better assess sources of risk that are not apparent in "hard" data such as financial statements, either because these risks are correlated with standard metrics in a noisy manner, or because they are wholly orthogonal to them. This literature has built upon, among others, the theoretical insights of Diamond (1984), Ramakrishnan and Thakor (1984), and Fama (1985), who discuss the mitigation of agency and asymmetric information problems through the distinctive monitoring technologies possessed by banks, as well as the unique incentive structures faced by banks, vis-à-vis disperse claimants like bondholders.[50]

Even within the set of firms that are reliant on relationship banking, there is evidence of heterogeneity in the importance of bank-firm relationships driven by firm opacity. Kano, Uchida, Udell, and Watanabe (2011), for instance, document how informational "verifiability" mediates firm-bank relationships: firms that are unable to obtain third-party verification for their operational results tend to value bank relationships more, and are more likely to be "captured" by financial intermediaries which service them.

In the context I explore in this paper, it is crucial for the "firms" at stake to establish relationships with banks or conduct business with them in a way tantamount to this. Indeed, the business carried out by drug syndicates is the paragon of "unverifiability," by design. Conversely, however, I argue that it is also quite valuable *for banks* to establish and maintain relationships with them. This is unintuitive, as these firms are awash in liquidity, and have effectively zero demand for credit. From the perspective of banks, therefore, the incentive to cultivate and maintain relationships with these economic actors must be quite different from the ones documented in previous papers.

Indeed, it is neither necessary nor useful for banks to produce information on the credit risk of criminal syndicates: it is useful, however, to anticipate where and when these businesses

[50] Other noteworthy papers in this literature include Blackwell & Santomero (1982), Neuberger & Rathke-Doppner (2015), Greenbaum, Kanatas, & Venezia (1989), and Haubrich (1989).

will produce excess liquidity, as to capture rents derived from low-cost funding. This informational advantage becomes especially relevant if these rents have a "winner-take-all" nature, which is feasible if switching costs for these firms grow with the complexity of operations.

All but the totality of the relationship banking literature focuses on the asset side of banks' balance sheets: the canonical questions posed in these papers are i. if and how banks offer distinct loan terms to firms with which they have relationships ii. how banks change their form –operational, capital structure, etc.—in the face of these relationships; and iii. how firms react to the establishment –or disruption—of long-standing credit relationships. It is a completely open question if banks establish and maintain relationships with firms based on their funding needs –seeking to capture deposits generated by firm activity. Hence, this paper contributes to the relationship banking literature by proposing a different motivation for relationship banking: demand by banks for deposit flows.

Although deposits are a major source of funding for retail banks, little is known regarding the influence that deposit seeking has on bank behavior. This is somewhat surprising, as we know that deposits remain valuable to banks even under bank risk neutrality, and that access to deposits shapes bank capital structure (Allen, Carletti, & Marquez, 2015). We also know that deposit seeking is endemic during bad times, especially among banks with weaker balance sheets (Acharya & Mora, 2015), and that deposit rates are at least in part determined by banks' demand for funds (Ben-David, Palvia, & Spatt 2017). In particular, there is a dearth of literature on the *spatial* effects of bank deposit-seeking behavior (LeSage, 1995). Namely, we lack an understanding of how banks expand their retail operations in response to their funding needs. Such an understanding would allow us to partially endogenize local bank market structure, which has been documented to have real effects both through passive (deposit) interest rates (Allen, Saunders, & Udell, 1991; Ho & Ishii, 2011, Cortés & Strahan, 2017), active rates (Corvoisier & Gropp, 2002), and financial inclusion and the financing of local economic activity (Nguyen, 2019).

In this sense, this paper contributes to the wider understanding of the determinants of the growth of branch networks, which is still an under-researched question. Although there is a robust literature documenting the *effects* of branch-network growth (Chong, 1991; Demsetz and Strahan, 1997; Loutskina and Strahan, 2011; Goetz, Laeven, and Levine, 2016), few papers investigate the causes of this growth (Gropp, Noth, & Schüwer, 2019).[51] Among the papers that *do* explore this question are Kim and Vale (2001) and Cohen and Mazzeo (2010): these papers, however, dwell on the determinants of the *aggregate* growth of branch networks, without accounting for the spatial dimension of this growth. Chang, Chaudhuri, & Jayaratne (1997), Chaudhuri *et al.* (1997) and Qi *et al.* (2019) provide analyses of the spatial expansion in branch networks, but only as a function of characteristics of the banking system, not of spatial units themselves. Lastly, other papers, such as Deller and Sundaram-Stukel (2012), who research spatial patterns in the location of credit unions in the United States, and Alamá *et al.* (2015), who investigate the growth of retail-bank networks in Spain as a function of geographic variables, provide little economic motivation for their research.

A key question in these related literatures on branch network growth has been if bank geographical expansion leads to diversification of location-specific risks. In other words, if geographies carry endemic risks, is it the case that expanding banking operations to other locales hedges these risks? This paper contributes to this conversation. Entry into markets in which organized crime operates exposes banks to unique forms of regulatory and operational risk. However, banks choose to operate expand into these. Why?

One possible answer is that banks specialize in assessing risks to which they have been exposed to through their past business, and expand into regions that expose them to similar, yet imperfectly correlated risks.[52] Another –perhaps less socially desirable—answer is that banks specialize in producing information about particular criminal organizations, through data obtained via direct communication. This paper seeks to disentangle these potential causal channels.

Demand for banking services by criminal organizations

I have (tacitly) introduced the hypotheses that banks exhibit willingness to i. operate in local markets with organized criminal presence and ii. provide financial services –especially the

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[51] Other related papers are Akhigbe and Whyte (2003), Emmons, Gilbert, and Yeager (2004), Carlson and Mitchener (2009), and Meslier et al. (2016).

[52] Gropp, Noth, & Schüwer (2019) document this type of bank behavior.

escrow of deposits—to criminal organizations. However, the question remains: do organized crime syndicates –like drug trafficking organizations (DTOs)—demand banking services? I posit they do.

This is manifest in the empirical record (see Aldama-Navarrete, 2019), as it is in numerous anecdotes.[53] Among these, a notable one is the attempt to purchase a banking group outright by agents of Amado Carrillo Fuentes, an infamous drug lord known in Mexico by the moniker *"El Señor de los Cielos"*—between 1995 and 1996. Before the seizure and liquidation of *Grupo Financiero Anáhuac* by Mexican regulators in late 1996, agents of the Ciudad Juárez-based kingpin had purchased stock in this banking group to the tune of almost $13 million dollars; by then, this bank had become a key part of their cartel's business strategy, serving as the conduit for remitting profits from the sale of illegal drugs in the United States to accounts in the Cayman Islands (Preston, 1998).[54]

Not only do bank connections serve organized crime as channels through which to launder illicit proceeds, but also to de-risk their operations. Criminal organizations have no recourse to the legal system to safeguard their contracts and property rights; hence, they are especially vulnerable to agency problems. Securing a third-party custodian for the cash flows they produce is thus key to avoiding the excessive consumption of perquisites by lieutenants, as well as outright graft by these –or competitor groups. Lastly, cash proceeds which have been deposited into the banking system are at diminished risk of being impounded by authorities.

Considering the above, in this paper I seek to document whether banks form long-standing relationships with illicit firms or conduct business as if these relationships existed. Moreover, I ask which, if any, bank characteristics predict the likelihood of these "relationships." By doing so, this paper contributes to the nascent finance literature that seeks to shed light on the interactions between banks and organized crime.[55]

[53] In the previous chapter of this dissertation, I have provided evidence that deposit services are likely used by economic agents connected to the "drug economy," both in the form of causal inference of the positive effect of local drug cartel presence on bank deposits, and in anecdotes retrieved from judicial cases.
[54] Notably, the seizure of the bank's assets was unrelated to narcotics, and due to other fraudulent activity.
[55] See Aldama-Navarrete (2019); Williams, Slutzky, & Villamizar-Villegas (2019); Agca, Slutzky, & Zeume (2020).

The rest of the paper is organized as follows: in Section 2 I describe the data used, as well as sample construction. Section 3 discusses the identification strategy pursued, while Section 4 covers results. Section 5 concludes.

2 Data

I use two main sources of data in this paper. First, I use data on the geographical areas of operation of Mexican drug cartels, from 1995 to 2010. This data is obtained from the database constructed by Coscia and Ríos (2012), which is described in detail in the appendices of the first chapter of this dissertation.

In a nutshell, however, Coscia and Ríos construct a cartel-specific measure of activity, at municipal scale, from 1990 to 2010. This measure is assembled from print news-media mentions of cartel activity, mostly from local news sources, aggregated by Google News. The Coscia-Ríos (henceforth, CR) dataset covers the activity of seven major criminal organizations, and also reports three "catch-all" categories: two of these tally mentions of minor groups allied with a couple of the major cartels, while the remaining one tallies all other mentions of minor criminal groups involved in wholesale drug trafficking.

Figure 1 plots the spatial distribution of cartel activity at the end of the sample period of this study. In this figure, municipalities with two or more cartels operating within are shaded in red, while those with only one drug cartel present are shaded orange. Municipalities in which no drug cartels operated are colored cream, while those for which there is missing data appear shaded gray. As can be seen, drug cartel activity was widespread across the Mexican territory by 2010, with little discernible clustering: (minor) clustering does appear along the border with the United States, and along the coastal regions, however. This is most likely a consequence of the layout of the major highway networks in Mexico, which are roughly oriented along a North-South axis and hug the Pacific and Gulf coasts.

Second, I use data from reports published by the *Comisión Nacional Bancaria y de Valores* (CNBV), the primary regulator of the banking sector in Mexico. In particular, I use the "*Base Operativa*" report, which details municipality-level information for all banks with retail operations in the country. This information includes number of branches per municipality/bank, as well as personnel employed and number of accounts for a variety of bank products, at the same level of aggregation. I also collect balance-sheet data, at monthly frequency, for all banks reporting it, which is available within the *"Balance general"* report made public by the CNBV.[56]

Sample construction

I merge this branch data with the Coscia-Ríos cartel activity data, using state and municipality names to perform a fuzzy match, which I then verify manually to ensure "clean" correspondence. Although I start with 97 banks having operated in Mexico between 1995 and 2010, consolidating bank holding companies and dropping bank-years with no branches reported leaves 39 banks with branches across 2,457 municipalities. Inconsistencies in matching between the banking data and the Coscia-Ríos dataset reduce municipality coverage to 2,415.

Branch reporting, however, is quite spotty in Mexican regulatory reports, leading to many municipality years being unobserved. My final sample includes 282,946 municipality-bank-year combinations: I then further merge this data with bank asset data from the CNBV and geolocation data obtained from INEGI, the official Mexican statistical agency.

Table 1 (Panel A) presents summary statistics for key dependent and independent variables used in this study.

These variables are as follows: $Branches_{ijt}$ is a count of the number of branches in municipality i operated by bank j in year t. $Treated_{it}$ is a dummy variable which takes on the value of one for municipality-year pair (i, t) if any number of drug cartels operate therein contemporaneously; $Cartels\ active_{it}$ is analogous to this binary treatment measure, but tallies the total number of cartels which are in operation in municipality i. $MA_5(BranchesTreated_{ijt})$ is a five-year moving average measure of exposure to cartel activity. To construct this measure of exposure, I first define:

[56] Report number 040-5A-R0.

$$BranchesTreated_{jt} = \sum_{i} \left(Branches\ _{it}^{j} \times\ Treated\ _{it} \right) \tag{1}$$

That is, the sum of branches of bank j over all municipalities i in year t, each of these interacted with an indicator which takes the value of one if the municipality is contemporaneously treated. Hence, only branches in *treated* municipalities will be counted toward the branch total of bank j.

Once I have constructed this variable, I subtract for each municipality i the number of *treated* branches therein from $BranchesTreated_{jt}$. This gives me a three-dimensional measure $BranchesTreated_{ijt}$, which is purged from the endogenous contribution that locally treated branches make to the bank-level aggregate $BranchesTreated_{jt}$.

Using this variable, I then construct a moving average of bank-level exposure, using five lags of $BranchesTreated_{ijt}$.

$$MA_5(\ BranchesTreated\ )_{(i)jt} = \sum_{t-l} \sum_{i} \frac{BranchesTreated\ _{ijt}}{5} \quad \forall l \in \{1,2,\dots,5\} \tag{2}$$

Assets is a bank-specific control variable, defined as the total bank assets, at monthly frequency, reported by bank j, averaged over year t.

$Relationships_{ijt}$ is constructed almost exactly as is $BranchesTreated_{ijt}$, except that in equation (1), $Treated\ _{it}$ is replaced with its cartel-specific analogue, $\mathbb{1}_{it}\{Cartel_k\}$ (a municipality-year dummy which is one if cartel k is active in municipality i in year t, and zero otherwise).[57] That is:

[57] Panel B of Table 1 presents correlation coefficients for $BranchesTreated_{ijt}$ and $Relationships_{ijt}$ across a variety of subsamples. As can be seen, the covariation of these measures is quite modest across these.

$$Relationships_{jt} = \sum_i \sum_k \mathbb{1}_{it}\{Cartel_k\} \sum_{i' \in -I} \left(\mathbb{1}_{i't}\{Cartel_k\} \times Branches_{it}^{j}\right) \tag{3}$$

Lastly, $BankExposure_{ikt}$ is a cartel-level analogue of $BranchesTreated_{ikt}$. It is a measure of exposure to banks that operate at time t in municipality i, for a cartel k that has contemporaneous presence in this municipality. Importantly, again, this measure only captures exposure experienced in *other* municipalities (that are not i).

3 Identification strategy

Determining whether criminal activity has any bearing on the decisions of banks to enter and carry out operations in certain markets is nontrivial. There are several reasons this is the case: for one, reverse causality is a potential issue. Since crime syndicates prize their connections to the financial system, they might locate in areas with more robust bank-branch networks. Omitted variables might also confound the effect of local drug-cartel presence on bank operations. For one, local economic conditions might influence both the market entry –and permanence—decisions of banks, and the analogous choices of criminal groups. Certainly, the market entry decisions of banks are contingent on expectations regarding credit demand among markets in their choice set. However, criminal cartels are also businesses, and profit from having access to more robust factor markets, better transportation networks, etc. Hence, as both criminal enterprises and banks might flock to the same geographical regions; namely, those with higher levels of economic activity, or expectations of economic growth. This would entail that the estimated effect of cartel activity on banks would be biased upward.

Another important issue is that if certain banks indeed do form relationships with criminal groups, or conduct business in a way tantamount to this, the risk tolerance of these intermediaries is bound to be quite unique vis-à-vis the modal bank. This risk tolerance –or close correlates thereof—is bound to also influence decisions of where to operate. In this case, the propensity to form business alliances with criminal organizations and the location pattern of a bank's retail activity would both be endogenous to unobserved characteristics.

Additionally, if there does exist a positive relationship between cartel presence and the likelihood of market entry or branch network expansion on behalf of banks, this could also introduce the issue of reflexivity to the estimation of parameters measuring this association. Namely, if banks do "chase" deposits generated by drug-trading activity, and criminal organizations demand financial services (and thus desire to locate where these are available), there is bound to be feedback from banking supply to demand, and vice-versa.

To circumvent these issues, I exploit variation both in the timing of the treatment of municipalities, and the joint presence of banks and cartels in *other* municipalities to identify the impact of previous exposure to criminal activity on the branch-network expansion decisions of a bank.

Since the fixed costs of operation in a given municipality are fairly low for a given cartel, and as these organizations shift their areas of operation at relatively high frequency, the presence of a cartel in a given municipality, in a given year, is plausibly orthogonal to the local characteristics which make the presence of a bank therein more –or less—likely. Indeed, these characteristics would evolve at much lower frequency.

Furthermore, I have shown in Aldama-Navarrete (2019), exploiting the same setting as the one explored in this paper, that the arrival of drug-cartel activity into a municipality is an exogenous shock to local bank deposits: there are no clear pre-trends which differentiate treated vs. untreated municipalities along this dimension before this arrival. Hence, cartel presence may be thought as instrumenting for the expected supply of deposits. Lastly, the exposure of a bank to organized criminal activity outside of municipality i prior to time t is plausibly conditionally exogenous to the contemporaneous decision to establish a branch in municipality i.

Hence, the identification strategy I follow can be thought of as akin to a dynamic differences-in-differences estimation, with the key difference that it is the inclusion of an observation into the regression which is conditional on treatment, not the independent variables "turning on."[58] The baseline regression specification I run is of the form:

[58] I also run models in which the criterion for inclusion is assignment to treatment: this is further detailed in Section 4.

$$\ln\left(Branches_{ijt}\right) = \alpha_{ij} + \tau_{it} + \beta_1 \ln\left[MA_5\left(1{+}BranchesTreated_{\ ijt}\right)\right] + \beta_2 \ln\left[MA_5\left(1{+}Relationships_{ijt}\right)\right] + \varepsilon_{ijt} \quad (4)$$

In equation (3) above, $Branches_{ijt}$ is the number of branches of bank j in municipality i, at time *t,* while α_{ij} is a municipality-bank fixed effect, and τ_{it} is a municipality-year fixed effect. $BranchesTreated_{\ ijt}$ and $Relationships_{ijt}$ are defined as before (see equations (1)-(3)); given their construction, it is plausible that these variables are conditionally orthogonal to the error term in equation (4).

Simply put, I regress branches of a given bank, in a given municipality i, in year t on a lagged five-year moving average of "treated" branches and a lagged five-year moving average of exposure to cartels present in i, for this same bank. The purpose of this exercise is to determine if banks "chase after" either i. deposits generated by organized criminal activity, ii. deposits generated by organized criminal groups with which they have had a history of interactions, or both.

4 Results

Table 2 presents results of several estimations of equation (4). In all these estimations, the sample period begins in 2000, to allow the construction of the variables $MA_5\left(BranchesTreated_{ijt}\right)$ and $MA_5\left(Relationships_{ijt}\right)$ for all observational units, although the data for variable construction spans 1995-2010.

Column (1) reports unsaturated models, while columns (2) and (3) report results for models in which municipality × year fixed effects have been included, to control for municipality-specific demand shocks. In the latter, municipality × bank and bank × year fixed effects have also been included, to control for bank-specific unobservables, both static and time-varying. The upper panel of Table 2 contains results for the sample of municipalities "assigned to treatment" (ATT); i.e. those that are *either* treated contemporaneously or were treated in a prior period, but no longer are. The bottom panel contains results for the sample of contemporaneously treated municipalities.

In all estimations, the estimated elasticity of $Branches_{ijt}$ to treated branches is positive and significant at the 1% level, and in the first two columns range from ~6% ($Treated$ panel, column (1)) to 16% ($Treated$ panel, column (2)), depending on the specification; estimated elasticities to $Relationships_{ijt}$ are for the most part negative, although also mostly non-significant.

Column (3) and (4), however, report much higher coefficient loadings on $BranchesTreated_{ijt}$, once bank × year fixed effects are included: estimated elasticities jump to anywhere from around 27% (ATT panel, column (4)) to around 40% ($Treated$ panel, column (3)).

These results, taken together, point at the following: i. there is evidence of banks seeking deposits by responding to prior exposure to organized criminal activity and ii. there is unobserved cross-sectional bank heterogeneity driving the pattern of variation in the dependent variable.

A. Heterogeneous effects

To better understand this heterogeneity, I run tests in which I interact $MA_5(BranchesTreated)$ with dummies for two bank characteristics that may be of importance: majority domestic equity ownership (*Domestic*) and G7 status, or membership in the group of seven largest banks in the Mexican market.[59] This definition coincides also with the CNBV definition of systemically important banks within the Mexican banking system. (Juárez, 2018). I perform these tests on both municipality subsamples: ATT municipalities and municipalities that are contemporaneously treated; in this battery of tests, I remove bank × year fixed effects, as otherwise time-varying bank characteristics would be absorbed. Likewise, asset decile fixed effects are removed, to prevent collinearity with controls described below. I take this battery of tests to be the "baseline" set of specifications in this paper: headline quantitative estimates are derived from this set of estimations.

Table 3 reports results of these tests; the first two columns report estimations on the ATT sample, while columns (3) and (4) report results for the treatment sample. All models contain

[59] These seven banks are BBVA, Banorte, Banamex, HSBC, Inbursa, Scotiabank, and Santander.

municipality × year and municipality × bank fixed effects; in addition, models reported in columns (2) and (4) contain bank assets, bank ROA and total deposits (scaled by assets) as controls. As can be seen in this table, interaction terms absorb the lion's share of the variation in branches: interactions between branches treated and G7 status yield strongly *negative* and significant loadings (row 2), while both the interaction of relationships and domestic status (row 6) and the triple interaction of branches treated with *Domestic* and *G7* dummies (row 7) yield strongly positive and significant loadings.

Two preliminary conclusions might be drawn from results reported so far: i. the positive response of branch networks to (indiscriminate) previous organized criminal activity is driven almost exclusively by domestic G7 banks, and ii. domestic banks exhibit an anomalously positive response to prior cartel-specific exposure (around 12% higher branch expansion in ATT municipalities, and around 20% higher in treated municipalities, as compared to foreign banks).

How to interpret these results? It is perhaps somewhat unsurprising that interaction terms carrying the *Domestic* dummy exhibit positive loadings, signifying that previous cartel exposure predicts greater propensity to enter municipalities in which organized crime groups currently reside. After all, foreign banks must contend with dual regulation, both in Mexico and in their home country, which might improve governance. Furthermore, as most foreign banks in Mexico are majority-owned by bank holding companies residing in either the EU or the United States, they must also contend with extraterritorial anti-corruption and anti-money laundering norms, extant in both legal systems. Hence, out of greater aversion to engage in corrupt activity –or simply less willingness to shoulder operational risk—foreign banks might actively avoid markets in which they know drug-trading organizations (DTOs) are active.

It is perhaps more surprising that a correlate of bank size, such as the $G7$ dummy, would load positively in these models. However, there is some evidence that the "too big to fail" status afforded to these banks by regulators through their classification as systemically important might engender moral hazard. For instance, regulatory reports released in 2015 note that 80% of all bank sanctions imposed in 2015Q1 were concentrated in just two of the G7 banks; the reports also document one of these two banks had also led the tally of sanctions for FY2014 (*Notimex,* 2015).

Further, there is also some evidence that the G7 banks are relatively opaque, as compared to their smaller peers. With monthly frequency, banks must submit to the CNBV several reports on their operations, including up-to-date financial information. Once processed, regulators make this information public, typically with a lag of one quarter. If a bank fails to submit information in a timely manner to the banking authorities, it will receive a sanction, which might carry a fine. Failure to comply in time with reporting requirements, however, is quite rampant. For reference, in the first semester of FY2017, 21.7% of required reports were not delivered on time (CNBV, 2017).[60] In untabulated tests, I find that G7 status positively and significantly predicts the likelihood of a bank being sanctioned for late reporting by the CNBV, from FY2000 to FY2010.

B. Risk stance or relationships?

The evidence produced by tests presented so far points to previous bank exposure to organized-crime activity, both aggregate and cartel-specific, as a predictor of market entry or market presence in localities with contemporaneous criminal activity. Further, results presented in Table (3) point to both domestic and G7 status as mediating these treatment effects. However, what is the relative importance of these bank characteristics in driving this?

Also, can we shed light on the channels through which prior exposure to organized-crime activity might make entry into markets with cartel presence more likely? For instance, are results obtained consistent with well-diversified banks entering areas which expose them to excess risk, as they can shoulder it? Is bank-cartel collusion the sole explanation for results obtained? To address these questions, I perform subsample analyses parallel to tests already reported, in which I split the sample by i. G7 status and ii. bank size. This allows me to gather evidence regarding which banks engage in "cartel-chasing" behavior, in a more interpretable way than that reported in Table (3).

[60] A likely reason for this is that penalties levied on banks are quite lenient: from FY2010 to the present, the maximum fine imposed on a bank by the CNBV amounts to MXN 8,331,600; this is roughly equivalent to $424,000 at the mean MXN/USD exchange rate for 2017 (CNBV, 2020).

Tables (4) and (5) present the results of these exercises. In Table (4), I report results of estimating equation (4) on nested subsamples defined by G7 status and foreign/domestic ownership. As can be seen in this panel, no significant loadings appear in estimations performed on the G7 subsample: this is most likely a result of low statistical power; consequently, there is not much that can be concluded regarding this part of the exercise. However, within the non-G7 sample, the foreign/domestic split yields an interesting result: the branching-elasticity calculated for relationships treated loads negative (~ -15%) and significant in the sample of foreign banks, but positive and significant (~3%) in the sample of domestic banks. This strengthens the contention that *only domestic banks* exhibit behavior consistent with relationship banking vis-à-vis DTOs.[61]

In Table (5), I report results obtained from estimating equation (4) below:

$$\ln\left(Branches_{ijt}\right) = \alpha_{ij} + \tau_{it} + \gamma_{jt} + \bar{X}\boldsymbol{\beta} \times Domestic_j + \varepsilon_{ijt} \tag{5}$$

In (4), γ_{jt} is a bank-year varying asset decile fixed effect, $\bar{X}$ is a vector with entries $BranchesTreated_{ijt}$ and $Relationships_{ijt}$, $Domestic_j$ is a bank-specific domestic dummy, and all other variables are as before. I perform estimations of (4) for both ATT and treatment samples and split the sample into "large" and "small" banks, defined as being above (below) the yearly median of size by assets.[62]

Results of these tests are presented in Table (5). The sign pattern, as well as the pattern coefficient magnitudes obtained, are quite striking. Again, as in previous tests, only domestic banks appear to exhibit "cartel-chasing" behavior: positive and significant loadings are obtained *only* for interaction terms including the *Domestic* dummy. Furthermore, the large/small bank split appears to be relevant: loadings on $BranchesTreated_{ijt} \times Domestic$ terms are insignificant for the latter group, but large and significant (~27%) for the former

[61] Table (4) presents results of estimations carried out only on the ATT sample; tests performed on the treatment sample are not tabulated, as they too suffer from low power.
[62] Dividing the sample along this cut, rather than along the G7 dimension allows for greater power, as does not splitting the sample into foreign/domestic subsamples, but rather using an interaction term.

(large banks). Interaction terms including $Relationships_{ijt}$ display the opposite pattern: loadings are non-significant for large banks, but positive (~14%) and significant for small banks.

C. Robustness

Several concerns might be brought up regarding results presented so far. For one, branch exposure to cartel activity may be mechanically higher for banks with a geographically concentrated market presence, such as regional banks: these banks will have higher exposures to cartels centered around the same regions.[63] If these banks were to expand their branch networks, but their spatial choice sets were constrained to areas in which they already operate, average treatment effect estimates would be biased upward.

There is also the issue of reflexivity/reverse causality. It may be that the pattern of observed variation is driven by the locational choices of cartels, and not those of banks: since it is profitable for DTOs to have ready access to the banking system, it is to be expected that cartels will operate in areas with greater banking presence. Moreover, even if the observed variation *is* driven by the endogenous location choices of banks, driven by deposit-seeking of illegal monies, it may be that this is itself a consequence of previous location choices by cartels, and so on.

To address the first concern, I both estimate the equation

$$\ln\left(Branches_{ijt}\right) = \alpha_{ij} + \tau_{it} + \bar{X}\boldsymbol{\beta} \times Large_{jt} + \varepsilon_{ijt} \tag{6}$$

[63] In their infancy, DTOs tend to be centered around a city, and control drug traffic flowing through the surrounding area. This is reflected in cartel names such as the Juárez Cartel (after Ciudad Juárez) or the Tijuana Cartel. Mature cartels, such as the Sinaloa Cartel, have diversified operations spanning multiple Mexican states, and even straddling international boundaries. This latter cartel, for instance, was thought to have operations in no less than thirteen countries as early as 2012 (Pachico, 2012).

and re-estimate equation (5) on the ATT sample, and include municipality × year and municipality × bank fixed effects; additionally, I split the sample into bins defined by distance from observation ijt to the geographical center of branching activity for bank j in year t.[64]

Importantly, the bins into which the sample is split are nested: each successively larger category contains preceding ones. I consider seven distance categories, in which the radius for included observations increases by either 100 or 200 miles.[65]

Results for these tests are reported in Table 6; Panel A contains estimations of (5), while Panel B reports estimations of (6). If it were true that what drives the estimated treatment effects was the overlap of bank and cartel areas of influence, we should see the point estimates for coefficients on both $BranchesTreated_{ijt}$ and $Relationships_{ijt}$ decrease as the estimation radius increases. This is not the case: in both panels, the interaction terms all but absorb variation, rendering loadings on $BranchesTreated_{ijt}$ and $Relationships_{ijt}$ non-significant. Moreover, loadings calculated on interaction terms are quite stable across bins, and *do not* exhibit a monotone pattern.

I now turn to the potential objection of reverse causality/reflexivity. As robustness checks for this potential source of bias, I conduct two sets of tests. First, I re-estimate equation (4) using the Arellano-Bond dynamic panel estimation procedure (Arellano and Bond 1991). This allows me to relax the assumption of strict exogeneity for the key independent regressors, $BranchesTreated_{ijt}$ and $Relationships_{ijt}$, and to model the dependent variable as dynamic, depending on its own past realizations, as we would expect under recursive reverse causality.

Results for these tests are presented in Table 7: column (1) contains results for the full ATT sample, column (2) for the sample of domestic banks, (3) for large banks, and (4) for large domestic banks.[66] The sign pattern obtained in these estimations is fully consistent with the findings put forth so far: relationships load positive and significant only for domestic samples, while branches treated only does so for the sample of large domestic banks. Point estimates obtained are quite distinct from the ones yielded by "similar" specifications presented

[64] *Large* is an indicator variable consistent with the definition of large banks used in previous estimations: it takes on the value of one if a bank is above median in size as measured by assets in year t, and zero otherwise. In untabulated tests, I also try interacting with a G7 dummy; results are qualitatively similar.
[65] Results are robust to alternative distance thresholds.
[66] Large banks are defined as before.

elsewhere in this paper. This is not surprising, however: the Arellano-Bond procedure "sweeps away" observation fixed effects by first differencing data –and, in the setting explored, as has been established, bank- and municipality-level heterogeneity account for a good deal of the variation in outcomes.

The second robustness check I perform to address reverse causality/reflexivity is running estimations in which the structure of the data has been reconfigured to vary at the municipality-cartel-year level. In these tests, the key independent variables are lags of the exposure of a cartel *k* to a bank *i*; namely, a binary indicator of cartel activity (for cartel *k*) in this municipality is regressed on lags of this exposure metric. I begin by using five lags of the data, to match the lag structure of previous tests, and then successively drop lags for which loadings are non-significant in previous model iterations.[67] As "controls," corresponding lags of the dependent variable are included in the model specifications.

These tests might be thought of as naïve dynamic-panel models, in which I allow for violations of strict exogeneity of the independent variable at different lags by including a variety of maximum lags of the dependent variable on the right-hand side of the estimated regression equation.[68]

Results for these exercises are reported in Table 8. Significance stabilizes at a maximum lag of two: for this number of lags, (prior) marginal exposure of a cartel *k* to a market in which a bank *j* is present –given that this *same* bank is present contemporaneously in the municipality *i* under consideration—is associated with a 0.01 percentage-point increase in the likelihood of *k* entering municipality *i*, for lags of both one and two years.

The mean of prior cartel-level exposure to markets in which banks active in *i* were active (*Bank exposure*) is 51.65 (SD =138.46); hence, a one-standard deviation increase in cartel exposure to banks in *i* would increase the likelihood of cartel presence in this municipality by ~1.39 percentage points. In spite of this low economic significance, however, the global significance F-statistic for the two-lags model (Column (9) in Table 7) is 29.62, which yields

[67] The Akaike Information Criterion is minimized for three lags of the data. Recall that in previous tests, the effective maximum lag of the data used in estimation was five, due to how the key independent variables are constructed (see equations (1)-(3)).
[68] An important caveat is that the point estimates obtained in these tests should not be taken too seriously – these estimations most likely suffers from Nickell (1981) bias.

a p-value that is equal to zero up to the third significant digit.[69] This is a valid Granger-causality test statistic for the response of the dependent variable to *Bank exposure*; hence, the null hypothesis of non-causation of cartel activity by *Bank exposure* is soundly rejected.

5 Discussion and Conclusions

Previous papers have documented that i. banks learn about risk through their business activity (Bouwman & Malmendier, 2015); ii. banks diversify away risk through geographical expansion of their activity –in particular, their retail-branching business (Morgan & Samolyk, 2003; Deng & Elyasiani, 2008; Aguirregabiria, Clark, & Wang, 2016); and iii. lower transparency, poorer governance, and low competition are associated with higher levels of bank corruption (Beck, Demirgüç-Kunt, & Levine, 2006; Barth *et al.*, 2009; Houston, Lin, & Ma, 2011).

This paper adds to these findings by documenting that both previous exposure to drug-cartel activity and previous exposure to specific DTOs predict bank entry into areas rife with organized crime. Although certainly the setting explored in this paper is unique in many ways, Mexico is far from being peculiar in its lax legal enforcement, oligopolistic banking market structure, and low standards of bank transparency. Thus, there is bound to be some generalizability to the results presented in this paper.

Namely, these (key) results are as follows: large domestic banks, perhaps keen to capture the cheap funding streams provided by deposit activity in areas where DTOs are active, enter these markets through the establishment of branches. These banks, however, *do not establish* long-standing relationships with cartels. There is evidence, however, that smaller domestic banks, may establish these relationships –either outright, or functionally so. This is perhaps to be expected, as these banks are more reliant on steady deposit flows as funding sources,

[69] The claim of low statistical significance is true under the *assumption* of non-cointegration (which would entail long-run causal dependence) of these variables.

as they face financial constraints in the interbank market, and must pay a steep premium for funding obtained through this medium (Martinez-Jaramillo *et al.*, 2014; Kim, 2017).

Why are results driven by domestic banks? A priori, one might hypothesize that one reason might be that these have informational advantages over foreign ones. Experience as market participants might enable them to better assess the risk/reward trade-off associated with entering areas which might expose them to both deposit windfalls *and* significant operational and regulatory risks. However, a cursory look at the history of the Mexican banking system reveals this to not be the case: five constituent banks of the G7 are foreign; in all these cases, foreign ownership was effected through M&A deals, not *de novo* entry (Citigroup acquired Banamex in 2001, BBVA acquired Bancomer in several operations between 2000 and 2004; Santander acquired Serfin in 2000, HSBC acquired Bital in 2002, and Scotiabank acquired Inverlat in 2000).[70]

Since it would be quite far-fetched to claim that institutional memory or expertise were displaced suddenly upon the advent of foreign equity ownership for these intermediaries, the hypothesis of differential governance quality takes on greater credence. However, from the data at hand, it is impossible to conclude if it is better external governance (legal institutions) or better internal governance which make these banks less likely to engage –be it passively or actively—with illicit actors

Lastly, is it the case that "systemically important" status makes banks more willing to engage in (apparently) disreputable activities, perhaps through a moral hazard channel? The evidence obtained in this study is inconclusive: bank size appears to almost crowd out G7 status fully in explanatory power, but given the high collinearity of these variables, any definite conclusion in this sense lies beyond the scope of this paper.

[70] See González and Peña, 2012.

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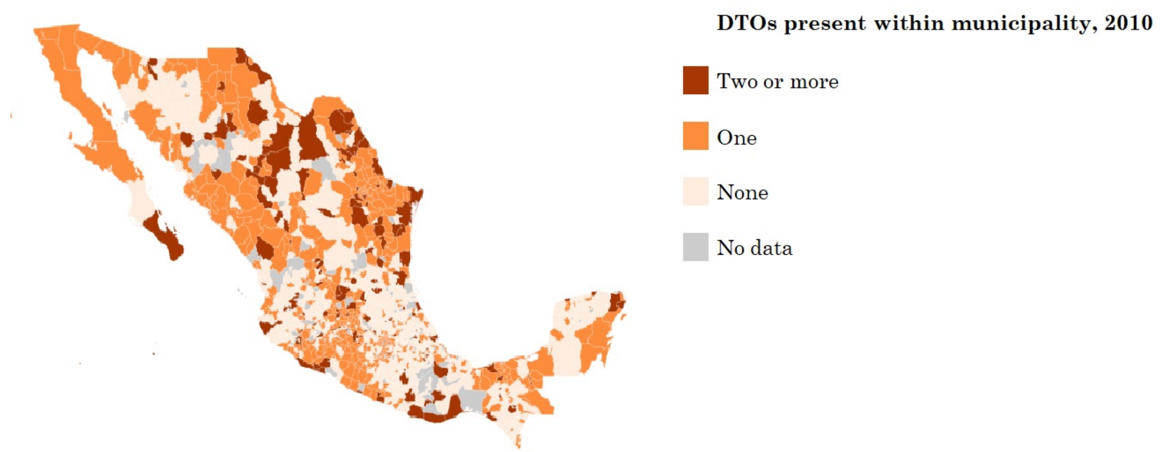
7 Figures

Figure 1



8 Tables

Table 1. Summary statistics for key variables

Panel A

Variable	*Level of variation*	*Units/Type*	*N*	*Mean*	*Std. Dev.*	*Min*	*Max*
Branches	Muni-bank-year	Count	282,946	0.302	1.899	0	134
Treated	Muni-year	Binary	282,946	0.225	0.417	0	1
Cartels active	Muni-year	Count	282,946	0.436	0.971	0	9
MA_5*(1+BranchesTreated)*	Muni-bank-year	Count, 5-yr moving average	217,995	225.030	297.850	-1	1,321
MA_5*(1+Relationships)*	Muni-bank-year	Count, 5-yr moving average	217,994	1.503	3.782	0	50
Assets	Bank-year	MXP, millions	248,238	168,649.400	287,070.500	0	1,183,483
BankExposure	Cartel-year	Count, market-level	141,630	51.646	138.460	0	1,786

Panel B

This panel reports estimated Pearson correlations for the key independent variables used in this paper: $MA_5(BranchesTreated_{ijt})$ and $MA_5(Relationships_{ijt})$. The unit of observation for this calculation is the municipality-bank-year. Correlation coefficients are reported for the full sample, the subsample of domestic banks, G7 banks, and large banks (defined as having above median assets in year *t*): these subsamples are not mutually exclusive.

		MA_5*(1+BranchesTreated)*			
		Full sample	Domestic banks	G7 banks	Large banks
MA_5*(1+Relationships)*	Full sample	0.206			
	Domestic banks		0.202		
	G7 banks			0.161	
	Large banks				0.160

Table 2. Baseline treatment effects, municipality-level branching

This panel reports estimates of regressions of the natural logarithm of (one plus) the number of branches of bank *j* in municipality *i* at time *t* on a five-year moving average of the natural logarithm of (one plus) the number of treated branches of bank *j* and a five-year moving average of the natural logarithm of (one plus) the number of "cartel relationships" held by bank *j*. Depending on the specification, estimated regression equations include municipality by year, municipality by bank, bank by year, or asset-decile (at yearly frequency) fixed effects. The reported R-squared is the estimated adjusted within-cell statistic. Standard errors are heteroskedasticity-robust, and clustered at the bank-year level. The sample period for all regressions is 2000-2010.

	(1)	(2)	(3)	(4)
	ln(1+Branches)			
	ATT			
ln MA_5 *(1+BranchesTreated)*	**0.086*** **	**0.134*** **	**0.353** **	**0.271** **
	(0.011)	(0.013)	(0.137)	(0.118)
ln MA_5 *(1+Relationships)*	**0.018**	**-0.115*** **	**-0.023**	**-0.024**
	(0.020)	(0.033)	(0.018)	(0.019)
Muni × Year FE	No	Yes	Yes	Yes
Muni × Bank FE	No	No	Yes	Yes
Bank × Year FE	No	No	Yes	Yes
Asset decile FE	No	No	No	Yes
Observations	61,887	61,887	57,666	54,473
R-squared	0.101	0.508	0.913	0.914
	Treated			
ln MA_5 *(1+BranchesTreated)*	**0.061*** **	**0.164*** **	**0.404** **	**0.317** **
	(0.015)	(0.022)	(0.156)	(0.136)
ln MA_5 *(1+Relationships)*	**0.086** **	**-0.197*** **	**-0.070**	**-0.067**
	(0.036)	(0.060)	(0.053)	(0.052)
Muni × Year FE	No	Yes	Yes	Yes
Muni × Bank FE	No	No	Yes	Yes
Bank × Year FE	No	No	Yes	Yes
Asset decile FE	No	No	No	Yes
Observations	53,919	53,919	48,527	46,020
R-squared	0.101	0.507	0.910	0.912

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Treatment effects on municipality-level branching, including heterogeneous effects

(Core results)

This panel reports estimates of regressions of the natural logarithm of (one plus) the number of branches of bank *j* in municipality *i* at time *t* on a five-year moving average of the natural logarithm of (one plus) the number of treated branches of bank *j* and a five-year moving average of the natural logarithm of (one plus) the number of "cartel relationships" held by bank *j*. Depending on the specification, interactions of the independent variables with a G7 dummy or a dummy for domestic majority equity ownership are included. Estimated regression equations include municipality by year and municipality by bank fixed effects. Models reported in columns (2) and (4) contain bank assets, bank ROA and total deposits (scaled by assets) as controls. The reported R-squared is the estimated adjusted within-cell statistic. Standard errors are heteroskedasticity-robust, and clustered at the bank-year level. The sample period for all regressions is 2000-2010. Columns (1) and (2) report estimations on the ATT sample, while columns (3) and (4) report results for the treatment sample.

	(1)	(2)	(3)	(4)
		ln(1+Branches)		
$ln\ MA_5(1{+}BranchesTreated)$	**-0.013**	**-0.013**	**0.015**	**0.013**
	(0.028)	(0.028)	(0.057)	(0.057)
$ln\ MA_5(1{+}Relationships)$	**-0.099**	**-0.094**	**-0.175**	**-0.162**
	(0.067)	(0.066)	(0.131)	(0.131)
$ln\ MA_5(1{+}BranchesTreated) \times G7$	**-0.228*****	**-0.229****	**-0.285*****	**-0.292*****
	(0.085)	(0.094)	(0.100)	(0.108)
$ln\ MA_5(1{+}Relationships) \times G7$	**0.085**	**0.093**	**0.120**	**0.146**
	(0.060)	(0.058)	(0.115)	(0.114)
$ln\ MA_5(1{+}BranchesTreated) \times Domestic$	**-0.032**	**-0.038**	**-0.074**	**-0.076**
	(0.035)	(0.033)	(0.058)	(0.056)
$ln\ MA_5(1{+}Relationships) \times Domestic$	**0.128****	**0.117****	**0.211***	**0.191***
	(0.058)	(0.055)	(0.112)	(0.111)
$ln\ MA_5(1{+}BranchesTreated) \times Domestic \times G7$	**0.307*****	**0.302*****	**0.355*****	**0.356*****
	(0.097)	(0.109)	(0.109)	(0.121)
$ln\ MA_5(1{+}Relationships) \times Domestic \times G7$	**-0.100***	**-0.103***	**-0.130**	**-0.150**
	(0.059)	(0.057)	(0.118)	(0.117)
Muni sample	ATT	ATT	Treated	Treated
Muni × Year FE	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	57,666	53,826	48,527	45,524
R-squared	0.897	0.902	0.893	0.899

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Subsample tests, foreign v. domestic and G7 status

This panel reports estimates of regressions of the natural logarithm of (one plus) the number of branches of bank *j* in municipality *i* at time *t* on a five-year moving average of the natural logarithm of (one plus) the number of treated branches of bank *j* and a five-year moving average of the natural logarithm of (one plus) the number of "cartel relationships" held by bank *j*. Estimated regression equations include municipality by year and municipality by bank fixed effects; models reported in columns (2) and (4) include asset-decile fixed effects as well. The top panel in this table reports estimation results from tests performed on the subsample of G7 banks, while the bottom one reports results from estimations on non-G7 banks. Columns (1) and (2) (for both panels) report estimations performed on the sample of foreign banks; the latter two columns report results from the sample of domestic banks. The reported R-squared is the estimated adjusted within-cell statistic. Standard errors are heteroskedasticity-robust, and clustered at the bank-year level. The sample period for all regressions is 2000-2010.

	(1)	(2)	(3)	(4)
	ln(1+Branches)			
	G7			
	Foreign		*Domestic*	
ln MA_5 *(1+BranchesTreated)*	**-0.017**	**-0.084**	**-0.032**	**-0.017**
	(0.101)	(0.079)	(0.116)	(0.078)
ln MA_5 *(1+Relationships)*	**-0.057**	**-0.041**	**0.027**	**-0.008**
	(0.150)	(0.138)	(0.028)	(0.025)
Muni × Year FE	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes
Asset decile FE	No	Yes	No	Yes
Observations	14,842	11,929	5,964	5,964
R-squared	0.935	0.940	0.953	0.956
	Non-G7			
	Foreign		*Domestic*	
ln MA_5 *(1+BranchesTreated)*	**0.011**	**0.010**	**-0.052****	**-0.049****
	(0.026)	(0.025)	(0.023)	(0.020)
ln MA_5 *(1+Relationships)*	**-0.150****	**-0.151****	**0.032***	**0.024***
	(0.066)	(0.066)	(0.017)	(0.014)
Muni × Year FE	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes
Asset decile FE	No	Yes	No	Yes
Observations	4,993	4,985	31,861	31,589
R-squared	0.771	0.771	0.895	0.891

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Subsample tests, small v. large banks

This panel reports estimates of regressions of the natural logarithm of (one plus) the number of branches of bank *j* in municipality *i* at time *t* on a five-year moving average of the natural logarithm of (one plus) the number of treated branches of bank *j* and a five-year moving average of the natural logarithm of (one plus) the number of "cartel relationships" held by bank *j*, and interactions of these independent variables with a dummy for domestic majority equity ownership. Columns (1) and (2) report results from models estimated on small banks, defined as having below-median assets in year *t*. Columns (3) and (4) report tests for large banks (above median asset holdings). Estimated regression equations include municipality by year and municipality by bank fixed effects; models reported in columns (2) and (4) include asset-decile fixed effects as well. The top panel in this table contains estimations performed on ATT municipalities; the bottom panel contains results for estimations performed on treated municipalities. The reported R-squared is the estimated adjusted within-cell statistic. Standard errors are heteroskedasticity-robust, and clustered at the bank-year level. The sample period for all regressions is 2000-2010.

	(1)	(2)	(3)	(4)
	ln(1+Branches)			
	Small banks		*Large banks*	
	ATT municipalities			
ln MA_5(1+BranchesTreated)	**-0.011**	**-0.011**	**-0.221***	**-0.263***
	(0.027)	(0.027)	(0.084)	(0.086)
ln MA_5(1+BranchesTreated) × Domestic	**-0.048**	**-0.043**	**0.248***	**0.275***
	(0.034)	(0.033)	(0.083)	(0.092)
ln MA_5(1+Relationships)	**-0.095***	**-0.095***	**-0.032**	**-0.014**
	(0.056)	(0.055)	(0.034)	(0.025)
ln MA_5(1+Relationships) × Domestic	**0.104**	**0.104**	**0.028**	**0.012**
	(0.047)	(0.046)	(0.018)	(0.014)
Muni × Year FE	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes
Asset decile FE	No	Yes	No	Yes
Observations	25,071	25,071	32,326	29,133
R-squared	0.871	0.871	0.921	0.929
	Treated municipalities			
ln MA_5(1+BranchesTreated)	**0.008**	**0.006**	**-0.228***	**-0.277***
	(0.054)	(0.054)	(0.085)	(0.088)
ln MA_5(1+BranchesTreated) × Domestic	**-0.079**	**-0.072**	**0.268***	**0.295***
	(0.053)	(0.053)	(0.086)	(0.094)
ln MA_5(1+Relationships)	**-0.145**	**-0.142**	**-0.129**	**-0.074**
	(0.115)	(0.115)	(0.098)	(0.079)
ln MA_5(1+Relationships) × Domestic	**0.168***	**0.169***	**0.057**	**0.026**
	(0.094)	(0.093)	(0.040)	(0.035)
Muni × Year FE	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes
Asset decile FE	No	Yes	No	Yes
Observations	20,880	20,880	27,181	24,674
R-squared	0.869	0.870	0.918	0.928

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Spatial robustness tests

Panel A

This table reports estimates of regressions of the natural logarithm of (one plus) the number of branches of bank *j* in municipality *i* at time *t* on a five-year moving average of the natural logarithm of (one plus) the number of treated branches of bank *j* and a five-year moving average of the natural logarithm of (one plus) the number of "cartel relationships" held by bank *j*, and interactions of these independent variables with a dummy for either "large bank" status (Panel A) or domestic majority equity ownership (Panel B). Estimations are performed on municipalities within a sequence of growing radii, centered on the geographical center of branch operations for bank *j* in year *t*. Importantly, these radii are nested: each successively larger category contains preceding ones. Estimated regression equations include municipality by year and municipality by bank fixed effects. The reported R-squared is the estimated adjusted within-cell statistic. Standard errors are heteroskedasticity-robust, and clustered at the bank-year level. The sample period for all regressions is 2000-2010.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				ln(1+Branches)			
Radii (mi)	*< 100*	*< 200*	*< 400*	*< 600*	*< 800*	*< 1000*	*< 2000*
Large	**0.075**	**0.043**	**0.036**	**0.022**	**0.013**	**0.005**	**0.002**
	(0.130)	(0.076)	(0.083)	(0.077)	(0.077)	(0.077)	(0.078)
ln MA $_5$(1+BranchesTreated)	**-0.050**	**-0.048*****	**-0.041****	**-0.042*****	**-0.042****	**-0.042****	**-0.042****
	(0.035)	(0.016)	(0.017)	(0.015)	(0.016)	(0.017)	(0.017)
ln MA $_5$(1+Relationships)	**0.037**	**0.051****	**0.046***	**0.042***	**0.042***	**0.042***	**0.044***
	(0.036)	(0.023)	(0.024)	(0.021)	(0.022)	(0.022)	(0.022)
ln MA $_5$(1+BranchesTreated) × Large	**-0.008**	**-0.006**	**-0.022**	**-0.021**	**-0.019**	**-0.022**	**-0.025**
	(0.035)	(0.030)	(0.031)	(0.029)	(0.028)	(0.029)	(0.031)
ln MA $_5$(1+Relationships) × Large	**-0.033***	**-0.028***	**-0.016**	**-0.010**	**-0.011**	**-0.009**	**-0.007**
	(0.020)	(0.017)	(0.018)	(0.017)	(0.017)	(0.018)	(0.019)
Muni × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,739	14,255	36,373	47,740	53,532	56,209	57,666
R-squared	0.951	0.933	0.899	0.896	0.896	0.894	0.895

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				ln(1+Branches)			
Radii (mi)	*< 100*	*< 200*	*< 400*	*< 600*	*< 800*	*< 1000*	*< 2000*
ln MA $_5$ (1+BranchesTreated)	**-0.102****	**-0.050*****	**-0.050*****	**-0.050*****	**-0.047*****	**-0.050*****	**-0.050*****
	(0.051)	(0.012)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)
ln MA $_5$ (1+Relationships)	**-0.030**	**-0.031**	**-0.037**	**-0.035**	**-0.037**	**-0.034**	**-0.034**
	(0.037)	(0.022)	(0.024)	(0.022)	(0.024)	(0.022)	(0.023)
ln MA $_5$ (1+BranchesTreated) × Domestic	**0.062**	**0.015**	**0.017**	**0.019**	**0.017**	**0.016**	**0.019**
	(0.056)	(0.025)	(0.032)	(0.032)	(0.032)	(0.032)	(0.033)
ln MA $_5$ (1+Relationships) × Domestic	**0.046****	**0.054*****	**0.080*****	**0.078*****	**0.080*****	**0.078*****	**0.079*****
	(0.021)	(0.018)	(0.026)	(0.026)	(0.026)	(0.028)	(0.028)
Muni × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Muni × Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,739	14,255	36,373	47,740	36,373	56,209	57,666
R-squared	0.952	0.933	0.900	0.897	0.900	0.895	0.896

Table 7. Arellano-Bond regressions, multiple subsamples

This panel reports estimates of regressions of the natural logarithm of (one plus) the number of branches of bank *j* in municipality *i* at time *t* on a five-year moving average of the natural logarithm of (one plus) the number of treated branches of bank *j* and a five-year moving average of the natural logarithm of (one plus) the number of "cartel relationships" held by bank *j*. Model estimation is performed using the dynamic-panel procedure of Arellano and Bond (1991). Column (1) reports results for an estimation performed on the ATT sample of municipalities; column (2) reports an estimation performed on the intersection of ATT municipalities and domestic banks; column (3) the estimation on a subsample defined by an ATT/large banks intersection, and column (4) ATT/large domestic banks. Standard errors are heteroskedasticity-robust. The sample period for all regressions is 2000-2010.

	(1)	(2)	(3)	(4)
	ln(1+Branches)			
L1.ln(1+Branches)	**-0.139*** **	**0.079**	**-0.138*** **	**-0.020**
	(0.016)	(0.061)	(0.017)	(0.073)
ln MA_5*(1+BranchesTreated)*	**-0.035*** **	**-0.060*** **	**-0.003**	**0.018*** **
	(0.003)	(0.007)	(0.007)	(0.006)
ln MA_5*(1+Relationships)*	**-0.013*** **	**0.020*** **	**-0.022*** **	**0.006*** **
	(0.002)	(0.002)	(0.002)	(0.002)
Sample	ATT	Domestic banks	Large banks	Large domestic banks
Observations	46,285	29,820	28,463	15,069
Number of clusters (muni-bank)	18,375	12,511	9,913	6,248

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Naïve dynamic-panel estimations

This panel reports estimates of regressions of a binary indicator of cartel activity for cartel *k* in municipality *i* at time *t* on lags of itself and of a measure of prior exposure of cartel *k* to banks contemporaneously present in this municipality, in municipalities *other than i.* Columns (1)-(3) report specifications with five lags, and the lag structure is shortened as one moves from left to right across the table. Depending on the specification, estimations contain municipality by cartel, municipality by year, or cartel by year fixed effects. The reported R-squared is the estimated adjusted within-cell statistic. Standard errors are heteroskedasticity-robust, and clustered at the cartel-year level. The sample period for all regressions is 2000-2010.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Cartel activity (binary)				
Cartel activity									
(t-1)	**0.146*** **	**0.160*** **	**0.140*** **	**0.172*** **	**0.188*** **	**0.166*** **	**0.178*** **	**0.196*** **	**0.172*** **
	(0.032)	(0.030)	(0.032)	(0.031)	(0.029)	(0.031)	(0.031)	(0.029)	(0.031)
(t-2)	**0.050***	**0.060****	**0.043**	**0.071*** **	**0.087*** **	**0.069*** **	**0.081*** **	**0.100*** **	**0.079*** **
	(0.026)	(0.025)	(0.026)	(0.024)	(0.023)	(0.024)	(0.024)	(0.023)	(0.024)
Lag *(t-3)*	**0.002**	**0.016**	**0.000**	**0.014**	**0.032**	**0.014**			
	(0.024)	(0.022)	(0.024)	(0.022)	(0.022)	(0.022)			
(t-4)	**-0.019**	**-0.007**	**-0.020**						
	(0.022)	(0.022)	(0.022)						
(t-5)	**-0.034**	**-0.009**	**-0.035**						
	(0.030)	(0.028)	(0.030)						
Bank exposure									
(t-1)	**0.0003*** **	**0.0001****	**0.0001***	**0.0003*** **	**0.0001*** **	**0.0001*** **	**0.0004*** **	**0.0002*** **	**0.0001*** **
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.00004)	(0.0001)
(t-2)	**0.000**	**0.000**	**0.0001****	**0.000**	**0.000**	**0.0001***	**0.000**	**0.0001***	**0.0001****
	(0.000)	(0.000)	(0.00004)	(0.000)	(0.000)	(0.0001)	(0.000)	(0.0001)	(0.0001)
Lag *(t-3)*	**0.0004***	**0.0005*** **	**0.0003***	**0.000**	**0.0003****	**0.000**			
	(0.0002)	(0.0002)	(0.0002)	(0.000)	(0.0001)	(0.000)			
(t-4)	**0.000**	**0.000**	**0.000**						
	(0.000)	(0.000)	(0.000)						
(t-5)	**-0.0005****	**-0.0003***	**-0.0004****						
	(0.0002)	(0.0002)	(0.0002)						
Muni × Cartel FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Muni × Year FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Cartel × Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	54,490	54,490	54,490	65,750	65,750	65,750	71,110	71,110	71,110
R-squared	0.507	0.456	0.516	0.492	0.438	0.501	0.490	0.435	0.499

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1