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Financial Institutions and the Real Economy

by

Todd Messer

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Committee in charge:

Professor Pierre-Olivier Gourinchas, Chair
Professor Christina Romer
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Abstract

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This dissertation examines the role of financial institutions as they relate to foreign currency payments and financial stability. The first chapter of this dissertation examines how the foreign currency component of international payments can be costly for importers and exporters by studying the introduction of a payments system between Brazil and Argentina established in 2008. The second chapter of this dissertation examines the reasons behind short-term funding vulnerabilities of financial institutions by studying Building and Loan Associations in California during the Great Depression. Finally, the last chapter of this dissertation studies the COVID-19 pandemic, which is one of the most important public health and economic events of recent history. This chapter studies the effect of stay-at-home orders, enacted to combat the spread of COVID-19, on local labor markets.

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My time in graduate school was full of ups and downs. There are many people who helped me in ways both large and small complete this journey. In lieu of a list of names that may leave important ones out, I prefer to say thanks to all who helped me by remembering the moments themselves. Family, friends, the UC Berkeley economics department, and many more people helped me through the hardest moments and celebrated the successes. From poor exam results to passing field exams, bad presentations to conference acceptances, dumped research ideas and published papers, the support and encouragement I received every step of the way was, in some sense, the only reason I finished the journey. I extend my dearest gratitude and thanks to all involved.

Finally, I would be remiss if I didn't explicitly acknowledge the support I received during the CoVID-19 pandemic. When the COVID-19 pandemic hit and the world shut down, there was no guarantee I would be able to graduate at all, let alone on time. As the department moved to work from home and communications were delayed, I felt anxious about my path to graduation. I am thankful that I was able to thrive during this period, thanks to advisors being available and helpful over Zoom, co-authors that helped make sense of the pandemic, and friends and family that helped me adjust.

Chapter 1

Introduction

As early as the merchant banks of Lombardy in the medieval era, financial institutions have played an important role channeling savings to borrowers. Modern banking, which began to take form in the eighteenth century, has continued this tradition. Across Europe as well as in the United States, financial institutions in the form of commercial banks continue to take deposits and made loans to industrial firms.[1] The relationships between this business model (of borrowing short and lending long) and economic growth continues to be actively debated. (Demirgüç-Kunt & Levine, 2009; Levine, 2005)

The role of financial institutions in a modern economy, however, extends well beyond that of simply allocating loanable funds. This dissertation begins by studying two additional roles that financial institutions have typically taken on. First, this dissertation studies the role of financial institutions in facilitating foreign exchange transactions in international trade. The usage of foreign exchange in international trade, especially in emerging markets, is ubiquitous. Second, this dissertation studies whether and how financial institutions may fail by examining a flightiness channel. If the liabilities of financial institution are structured such that investors or depositors are more likely to run on a bank, then ex-ante these institutions may be more susceptible to failure with potential negative effects on local economies.

The first chapter of this dissertation studies the introduction of a payments system between Brazil and Argentina in 2008. This payments system eliminated the usage of foreign currency by permitting Brazilian firms and Argentine firms to settle trade using only their respective local currencies. The central banks act as intermediaries to settle transactions daily in United states Dollars. The system was taken up by a sizeable share of Brazilian exporters, as a approximately 10% of exporting firms used the system within a few short years compared with essentially zero local currency usage beforehand.

The key contribution of this chapter is in showing that the overwhelming dominance of the United States Dollar in international trade may in fact be a problem for emerging market economies. This problem can manifest itself in the form of a trade cost for exporting firms. (Atkin & Khandelwal, 2020) This work supports recent work studying second-order

[1] For an in depth discussion of the roots of modern banking, see Cassis et al. (2016).

effects of the dominance of the United States Dollar. The academic literature has found that the usage of the United States Dollar in trade has been an important factor in causing prices (when expressed in a common currency) to deviate across locations (i.e. violations of the aptly named Law of One Price). These deviations, in turn, have consequences for the conduct of monetary policy and the transmission of shocks across borders.[2]

This first chapter leverages the introduction of the payments system to study whether the usage of foreign currency actually functions as a trade cost. Identifying exogenous usage of the payments system via access to authorized financial institutions, I find that the usage of foreign currency constitutes a barrier to trade. Complementary evidence using confidential customs administrative data on Brazilian exporters confirms this result. I also investigate which types of firms took up the program. I find that firms of all sizes and across a wide variety of sectors take up the program. However, the effect is concentrated in firms that have some price setting power and have a large share of exports to Argentina.

The second chapter of this dissertation focuses on how the structure of financial institutions can generate or amplify economic shocks. Previous work in this area has focused on either the endogenous quality asset side of the balance sheet quality of loans (Calomiris & Mason, 1997) or exogenous liquidity shocks affecting liability side (Diamond & Dybvig, 1983). Instead, this chapter studies whether there may be endogenous quality changes on the liability side of the balance sheet. Financial institutions may structure their liabilities to attract investors/depositors that are more or less "flighty," or more likely to withdraw funds during a crisis.

Financial institutions play an important role in risk management. By diversifying both their sources of funding and sources of lending, they can reduce the transmission of idiosyncratic shocks onto the aggregate economy. But such benefits may come with a cost. Since at least the Great Depression, and continuing in earnest after the Great Recession, economists have debated whether financial institutions can also be detrimental for the real economy. This may be simply amplifying underlying shocks (e.g. Gertler and Kiyotaki, 2010) or even as a source of instability itself (e.g. the firm leverage constraints in Jermann and Quadrini, 2012 or the risk-bearing capacity of the financial sector in Gilchrist and Zakrajšek, 2012).

This chapter studies building and loan associations in California during the Great Depression. Building and loan associations are financial institutions that specialize in mortgage lending. All building and loan associations in California during this time had over 90% of their assets in such lending. However, the liability side of these associations' balance sheets differed. Older institutions operated under what was called a permanent plan, which featured high withdrawal fees and regular savings plans. Newer institutions, operating under what was called a Dayton plan did not have withdrawal fees and allowed members to save/withdrawal with relative ease in order to compete with commercial banks. Comparing failure rates of Building and Loan associations under permanent and Dayton plans allow one to observe the

[2]This discussion is about the consequences of invoicing in foreign currency, taking the decision as exogenous. There is also an important literature studying why firms select an invoicing currency, which takes the decision as endogenous. This chapter has less to say about this latter literature.

effect of the liability structure on financial instability.

I find that Dayton plan institutions failed at a much higher rate than permanent plans, suggesting that the quality of liabilities matters for the vulnerability of financial institutions. This effect is robust to the inclusion of observable balance sheet controls and non-parametric controls for geographic location. Evidence using archival data suggests that the asset side of the balance sheet was not at play, as lending rates were relatively similar, but Permanent plans paid out higher returns to attract higher quality members. Finally, counties with a higher share of Dayton plans had a larger decline in farmland value, suggesting that this financial instability had real effects.

This dissertation concludes in the third chapter by studying the economic effects of the COVID-19 pandemic. First declared a pandemic by the World Health Organization on March 11, 2020, the size and scale of this global health event has had not only a human cost, but an economic one as well. These economic costs may to persist over time, with the potential to transform labor markets, to reshape trade patterns, and even to reorganize the operations of financial institutions.

Using the econometric techniques and theoretical tools from the previous chapters, Chapter 3, which is joint work with ChaeWon Baek, Preston Mui, and Peter McCrory, studies the short-term effects of one of the most important public health measures, stay-at-home orders, on local labor markets. We find that the imposition of stay-at-home had a negative effect on local labor markets. However, this effect accounts for at most half of the rise in unemployment during this time period. The remaining effect is likely due to declining consumer sentiment as individuals chose to stay home. Our results suggest that the economic disruption is likely to persist until the underlying public health issues are resolved. This chapter marks one of the earliest empirical studies of the effects of COVID-19 on the real economy.

Chapter 2

Foreign Currency as a Barrier to Trade: Evidence from Brazil

2.1 Introduction

This first chapter of the dissertation studies the role that financial institutions play in facilitating foreign exchange transactions across borders. Emerging market firms frequently use foreign currency to buy and sell goods abroad. There may be substantial costs of such foreign currency usage, such as exchange rate risk or foreign exchange (FX) market imperfections. These costs can be large both because the overwhelming majority of export transactions in emerging markets rely on the United States Dollar (USD) and because emerging markets' exchange rates are known to experience high volatility. Although there is a substantial body of evidence detailing how unexpected movements in exchange rates affect export behavior, we know comparatively less about the effects of foreign currency itself.

A potential reason for the omission of the effects of FX usage is the observed stability of invoicing decisions. Empirically, there is almost no variation, let alone exogenous variation, with which to estimate the causal effect of foreign currency usage. Recent evidence has found this to be true both for individual firms and for entire countries. (Amiti et al., 2018; Gopinath, 2016) Theoretically, international macroeconomic models mainly focus on the effects of exchange rate pass-through, or the relationship between prices and exchange rates. These models typically do not assume any first-order effects from the choice of invoicing currency. There is therefore no clear answer as to whether the usage of foreign currency limits emerging market firms in trading abroad.

This paper estimates that the effect of foreign currency usage on export volumes is large. I leverage the introduction of a policy change in Brazil: the "Local Currency Payments" (SML) system.[1] This payments system reduced reliance on foreign currency by Brazilian exporters and Argentine importers and accounted for nearly 10% of the total value of exports from Brazil to Argentina by 2012. Using financial and customs data in Brazil, this paper

[1]The acronym SML comes from the Portuguese "Sistema de Pagamentos em Moeda Local".

estimates the effect of reducing foreign currency usage is similar to lowering variable trade costs by between approximately 10%. I do so using two complementary research designs. First, I leverage municipal variation in access to financial institutions that were authorized to use the SML system. Second, I use disaggregated, confidential customs data to study the firm-level effects of taking up the policy. In the last part of this paper, I discuss potential reasons for the effect I find in the empirical analysis, such as reducing exchange rate risk or eliminating costly FX payments for importers and exporters.

The introduction of the SML system between Brazil and Argentina in 2008 reduced reliance on foreign currency for Brazilian exporters and increased the share of Brazilian exporters invoicing in their home currency. By opting into the system, Brazilian exporters would receive revenues directly in their currency, the the Brazilian Real (BRL), while Argentine importers paid directly in their currency, the Argentine Peso (ARS). As part of the requirements to use the SML system, exporters in Brazil were required to invoice the export in their local currency, the Brazilian Real (BRL). The central banks of the two countries manage the exchange of currencies between the two firms. The introduction of the SML system led to a large increase in the share of exports from Brazil to Argentina that were invoiced in BRL, which went from essentially 0% prior to the introduction of the program to slightly less than 10% by 2012.[2]

The first empirical strategy in this paper uses variation in municipalities' historical access to SML-eligible financial institutions. I estimate the causal impact of SML using a triple differences design. I define access by calculating the share of corporate loans within a municipality that come from SML eligible financial institutions. This approach then amounts to comparing changes in relative exports to Argentina between high- and low-access SML municipalities. Under a parallel trends assumption, that I can provide suggestive evidence for, I provide municipal-level evidence of the effect of the elimination of exchange rate risk.

I find that municipalities with high shares of corporate loans by SML authorized banks saw exports rise by approximately 20% relative to municipalities with low SML corporate loan shares. The lack of any obvious pretrends provides suggestive evidence that in the absence of the SML system, the the growth of export volumes to Argentina would have developed similarly.

The second empirical strategy uses firm-level data to study the effects of foreign currency usage. Because exporters that used the SML system were required to invoice in BRL, I can study the effects of the policy by observing changse in BRL invoicing behavior. I use confidential firm level data available from the Brazilian customs administration that records detailed information about international trade including the invoicing currency. First, I investigate the types of firms that took up the SML system. Unlike other papers that lack temporal variation in the currency of invoicing, I am able to leverage a large change

[2]In this paper, I focus on Brazilian exports to Argentina, rather than Argentine exports to Brazil. Takeup of the SML system by Argentine exports was close to zero. In the conclusion, I offer some reasons for this lack of takeup, specifically that Argentine monetary policy is much more volatile than Brazilian monetary policy, leading Argentine exporters to prefer to receive payment in USD. The variation I exploit in my empirical analysis is orthogonal to such effects, which allows me to estimate the effects on export behavior.

in invoicing behavior following the introduction of the SML system in Brazil. Prior to the introduction of the SML system, the usage of the BRL in exports to Argentina was effectively zero. By 2012, the share of exports invoiced in BRL had gradually risen to nearly 10%. Such a large increase has not been observed in other studies of invoicing behavior. The rise in BRL usage creates time-variation in invoicing shares that permit an analysis of the effect of invoicing currency on trade. Argentina is one of the largest trading partners of Brazil, so the overall share of firms using BRL is noticeable, albeit small.[3]

Perhaps surprisingly, the time variation in the currency of invoicing is not specific to any individual sector or firm characteristic. Commodity exports, such as raw minerals, saw the same rise in BRL invoicing as differentiated goods sectors. Such a result contrasts with ideas that, since commodities are traded on international exchanges and priced in USD, exporters prefer to invoice in USD as well. While smaller firms more exposed to Argentina were most likely to switch to invoicing in BRL, larger firms exporting to over ten countries also switched.

I then exploit variation both across time and across destinations in the invoicing decision by Brazilian exporters to estimate the effect of foreign currency risk. I do so by leveraging the highly disaggregated customs data and using a restrictive fixed effect design that aims to control for any endogenous selection effects both at the individual firm-sector level and over time by sector and destination. Specifically, I include firm-sector fixed effects to control for time-invariant determinants of selection as well as sector-time and destination-time fixed effects to control for aggregate demand and cost shocks. Under the assumption that the endogenous selection effect is time-invariant at the firm-sector level, this specification identifies the causal effect of the elimination of foreign currency risk on trade.

I find that that eliminating foreign currency usage via the SML system has a large positive effect on firms' exports. For an individual firm-sector, switching to BRL-invoiced shipments to Argentina raises the size of the shipment by approximately 0.44 log points relative to exports to other locations. This effect is not driven by changes in sectoral composition or overall destination-specific export growth. Additionally, I find essentially no effect on relative prices, suggesting that transaction costs or hedging as a function of the shipment size is unlikely to be the mechanism at work. This increase in trading volume is reflected in overall firm exports. BRL-invoicing firms increase the share of sales to Argentina by around 22 percentage points. There also does not seem to be any cannibalism of exports to other export destinations. Aggregating across all destinations, I show that while firms that switch to BRL invoicing had a higher share of sales towards Argentina, this did not come at the expense of exports to other destinations.

I also perform a series of heterogeneity analyses to understand which firms benefited most from the SML system. While firms of a wide variety of sectors and sizes used the SML system, the benefits accrued almost exclusively for firms in non-commodity sectors

[3]Much like other emerging market countries, I document that Brazil heavily relies on the United States Dollar (USD) for most of its external trade: over 90% of exports are invoiced in USD. This fraction is relatively stable both across countries and in the years before the introduction of the SML system.

with a high share of exports to Argentina. This result suggests that models of imperfect competition and price setting, ubiquitous in international macroeconomics, are likely to be able to capture the main effects of the SML system.

I investigate potential concerns in each approach in a series of robustness checks. For the municipality evidence, the presence of SML eligible financial institutions may be endogenous to local firms' export behavior to Argentina. Specifically, firms may have petitioned their financial institutions to become authorized to use the SML system due to the anticipation of higher sales to Argentina. In a robustness check, I drop municipalities that account for a large share of a financial institutions' loan portfolio, and find similar results. Additionally given the timing of the SML system, it may simply be the case that SML authorized institutions may have been more resilient to changes in global economic conditions, either due to the collapse in trade or the global financial crisis. While the regression design mostly accounts for this issue by comparing exports to Argentina relative to South America across municipalities, it still may be the case that heterogeneity across the control group masks differential effects of global economic conditions across export destinations. In placebo tests to other South American countries, there is no significant effect of the SML system on export behavior, suggesting that the SML shares I calculate are not simply capturing resiliency to global economic conditions.

For the customs-level evidence, which relies on fixed effects to control for selection effects, it may be the case that the determinant of policy takeup varies at the firm-time level. In robustness checks, I show that results are relatively unchanged when including firm-time dummies, which aim to control for time varying, unobserved determinants of selection into the SML system at the firm level. I also instrument the decision to invoice in BRL with the an instrument that captures whether a firm exporting to Argentina is in a municipality with a high or low SML banking share. I find that the estimates are attenuated only slightly, suggesting that the selection effect, although small, biases estimates upwards. However, the 2SLS point estimates are imprecise at the firm level, so these robustness results should be interpreted with caution.

In the last part of this paper, I argue that the effect of the SML system was to shift out both importer demand and export supply. Workhorse models of currency choice, such as Gopinath et al. (2010) or Boz et al. (2018) emphasize that exchange rate movements affect demand only through relative price movements. While firms choose their invoicing currency based on maximizing expected profits in a sticky price environment, final demand is only determined by the exchange rate adjusted local price. I instead present a stylized model of exports, and discuss potential mechanisms by which the SML system works. In particular, I emphasize the role of risk aversion or the depth of financial markets. While the SML system likely only reduced uncertainty for exporters (given the way the SML system worked), both importers and exporters could benefit from avoiding foreign currency, which may come with additional costs. I also discuss how to microfound such frictions.

Literature Review

This paper contributes to two strands of literature. First, this paper contributes to the literature on the determinants and effects of invoicing currency. With nominal rigidities, invoice currency plays an important role in explaining short-term deviations from the law of one price, which has implications for purchasing power parity.[4] My paper builds on this work by arguing that not only does the role of foreign currency, such as the USD, affect export volumes via relative price adjustments, but that also by limiting trade directly. This puts my work closer to the literature studying the invoice decision itself. Engel (2006), Gopinath et al. (2010), and Mukhin (2018) have argued that currency choice is endogenous and determined by minimizing variation around optimal prices.[5] My paper tests this assumption and studies how invoicing currency, specifically risk around the realized price, can have first-order effects on export volumes without relative price movements.[6]

Second, this paper contributes to the vast literature in international trade on trade costs. Many papers have investigated the importance and sources of a wide variety of trade costs (see Head and Mayer (2014) for a survey). This paper specifically relates to a growing literature on the importance of foreign currency as a trade cost. In their survey of the sources of trade costs in developing countries, Atkin and Khandelwal (2020) note that the widespread usage of foreign currency has implications for trade in developing countries. Lyonnet et al. (2016) study how hedging via financial derivatives affects invoicing decisions and sales. Closely related is the literature on trade effects of currency unions. Rose (2014a). While the currency union literature has in some cases found large trade effects as a result of ascension to the currency union, these estimates also capture the effects of a number of other structural changes. This paper, by contrast, focuses on the role of currency only. This also relates to a broader literature on the role of capital markets in international trade. Frictions in the financial market may relate to credit constraints (Manova (2013); Paravisini et al. (2015)) or substantial upfront fixed costs.

Other papers in international macroeconomics and international trade focus on how movements in the exchange rate may have permanent effects on trading volumes. The "beachhead effect," studied in Baldwin (1988) and Baldwin and Krugman (1989) argues that large devaluations may have persistent effects on trade if firm entry or exit decisions are affected. Later

[4] This literature has at its roots the seminal argument of Friedman (1953) that exchange flexibility can be a source of needed relative price adjustment in response to negative economic shocks. Burstein and Gopinath (2014) emphasize that the relationship between bilateral exchange rates and prices exhibits incomplete pass-through over time, with substantial heterogeneity across countries. Part of this incomplete pass-through is due to the dominant role of the USD in international trade (e.g. Gopinath (2016) or Cravino (201)), and part is due to the role of strategic complementarities in pricing (e.g. Amiti et al. (2014)).

[5] Early work in this area, such as Obstfeld and Rogoff (1995) or Betts and Devereux (2000) studied how invoicing currency affects optimal monetary policy, but took the invoicing decision as given.

[6] More recently, research has begun to investigate the effects of invoicing on other firm-level outcomes. For example, Barbiero (2020) studies how currency mismatch between foreign currency import payments and export revenues may result in valuation effects that can affect investment. My paper focuses only on export volumes.

work by Devereux et al. (2019) extends this argument to forming new trade relationships. My paper relates to this strand of the literature due to its emphasis on the role of foreign exchange risk in affecting trade volumes. However, my paper emphasizes persistent effects on demand rather than market structure.

2.2 Setting and Institutional Background

> "With elimination of a third currency in direct transactions among companies, exporters will set their prices in the currency of their own countries. Thus, they will be better able to calculate their margins precisely, since they will no longer be exposed to exchange rate risk" - Henrique Meirelles, Governor of the BCB (October 2008)

To understand how the usage of foreign currency affects export behavior, I study a policy change in Brazil that made it easier for Brazilian exporters to use the Brazilian currency, the real (BRL), to invoice trade. Latin America has a long history of managing foreign currency usage in regional trade dating back to at least the 1960s. The introduction of a new mechanism by which trade could be operated exclusively in their own currencies, the SML system, eliminated the usage of foreign currency without resorting to extreme measures such as a currency union.[7] In this section, I describe the evolution of foreign currency usage in Brazilian export trade.

A Brief History of Latin American Currency Treaties

In the 1960s, trade among Latin American countries at the time was predominantly denominated in USD owing to the lack of convertibility of many countries' currencies. So, central banks would clear international trade on a frequent and bilateral basis, meaning all banks needed constant access to dollar liquidity to make payments on behalf of importers and exporters. Latin America's long history of balance of payments difficulties and capital controls (specifically with respect to free convertibility of currency) made executing payments for international trade purposes difficult. (Mathis, 1969)

An early attempt among Latin American countries to rectify this problem was made through the Latin America Free Trade Agreement (LAFTA).[8] In 1965, a multilateral mechanism was created, known as the Reciprocal Payments and Credit Agreement (CPCR).[9] The

[7] I do not discuss the Unified System for Regional Compensation (SUCRE), as neither Brazil nor Argentina are a part of it. For more information, see Caldentey et al. (2013).

[8] LAFTA was created by the 1960 Treaty of Montevideo, and had as its major focus a reduction of tariffs and the creation of a free trade area within Latin America. LAFTA replaced the The Economic Commission for Latin America (ECLA), which was created in 1948.

[9] This and the following paragraphs rely heavily on information from Flax-Davidson (1985) and Caldentey et al. (2013)

CPCR accomplished two goals: it created lines of credit among Central Banks and established a multilateral settlement system among its signatories. The agreement allowed any such trade transaction, such as letters of credit or open accounts, to be settled within its mechanism. Under the 1965 system, international balances would be calculated by the Central Bank of Peru and settled bimonthly via the Federal Reserve Bank of New York. Trade was required to be in USD due to its convertibility. Firms and banks within countries would record import and export transactions with their respective central banks, who would only need to access dollar liquidity a handful of times per year in order to settle cross-country balances.

LAFTA had a goal of creating an efficient trade zone with low tariffs by 1980. Failing to finalize agreements on free trade, LAFTA reorganized in 1980 into the Latin American Integration Association (ALADI) after the signing of the 1980 Montevideo Treaty. In August 1982, the Reciprocal Payments and Credit Agreement was formally extended by ALADI and operated similarly. Now, domestic financial institutions would contract directly with their central bank, paying or receiving either local currency or USD, depending on the local regulations. Net amounts among central banks would be settled at the end of four month periods.

The CPCR improves international trade by reducing reliance on foreign currency, minimizing risk, and reducing transaction costs. The lines of credit and multilateral settlement scheme limit the number of foreign currency transactions that are made by both public and private agents. It minimizes risk by having the central banks take on credit risk (ensuring that payment will be made regardless of whether the importing firm actually does) and ensuring convertibility in that the Central Bank is always willing and able to exchange local currency for the vehicle currency, typically the USD, that might not be available in private markets. Finally, it reduces transaction costs by eliminating reliance on correspondent banks that are possibly overseas. The usage of the CPCR was highest during the 1980s, although since the 1990s it has become largely unimportant. In fact, in April 2019, Brazil formally withdrew from the CPCR. [10]

Alongside ALADI, Mercosur was created in 1991 by Brazil and Argentina by the Treaty of Asunción and the Protocol of Ouro Preto in 1994. Mercosur is in some sense a specialization of ALADI with the goal of creating a common market.[11] Unlike ALADI, which focused on reducing trade barriers and promoting regional harmony, Mercosur focused additionally on, for example, building free movement of people, capital, and currency. Since, however, it has operated more as a customs union.

The development of Mercosur included discussions about a common currency for trade. Arguably the first discussions regarding establishing a mechanism for invoicing and settling bilateral trade between Brazil and Argentina without the use of foreign currency involved the Gaucho. In 1987, the presidents of Argentina and Brazil met in Viedma, Argentina

[10] Link

[11] Another subregional South American cluster is the Andean Community of Nations, which involves Bolivia, Colombia, Ecuador, and Peru.

and signed Protocol 20. This Protocol established the Gaucho. The Gaucho was not a common currency per se, but rather a common monetary unit created in response to large trade imbalances between Brazil and Argentina. Holders of Gauchos were to be penalized, encouraging usage of the currency in international Trade. However, this idea was quickly abandoned as other macroeconomic issues arose. (Gardini, 2011)

Over the 1990s, changes in the currency policy by Brazil and Argentina limited discussion of common currencies through Mercosur or LAIA.[12] Both economies grew and became increasingly integrated via trade. However, in 1998 the President of Argentina, Carlos Menem, suggested that Mercosur should consider a common currency. or a series of currency boards pegging Latin American currencies to the USD. Of course, a currency union is much more extreme then simply reducing exchange rate variability in regional trade. However, the idea of a Mercosur currency union was little more than an academic exercise over the following years.[13]

The SML System

In 1992, the BCB released the Consolidation of Foreign Exchange Standards (CNC)[14]. One of the implications of this regulation was that export trade by Brazil, even for exports not through the ALADI treaty, were required to be in foreign currency and accompanied by a foreign exchange contract.Central Bank of Brazil (1992) This foreign currency was typically in USD. Foreign exchange contracts were written and directly linked to the export operation recorded by the trade registry.

In March of 2005, the Central Bank of Brazil greatly reduced exchange rate controls. The BCB began allowing exports to be settled in BRL so long as they are recorded as such in the trade register. (Central Bank of Brazil, 2005a, 2005b) Still, as in most countries, over 95% of total Brazilian trade even in the years following 2005 was invoiced in USD. More broadly, this law removed control from the BCB to monitor and control any issues related to foreign exchange receipts by exporters.[15]

To reduce reliance on foreign currency in trade, Brazil and Argentina negotiated a new mechanism by which trade between the two countries could be operated exclusively in their own currencies. The Local Payments System (SML) was created in 2008 in order to facilitate trade between Argentina and Brazil. This facility was a payments mechanism and was managed by the two countries' central banks. One part of the payments system allows

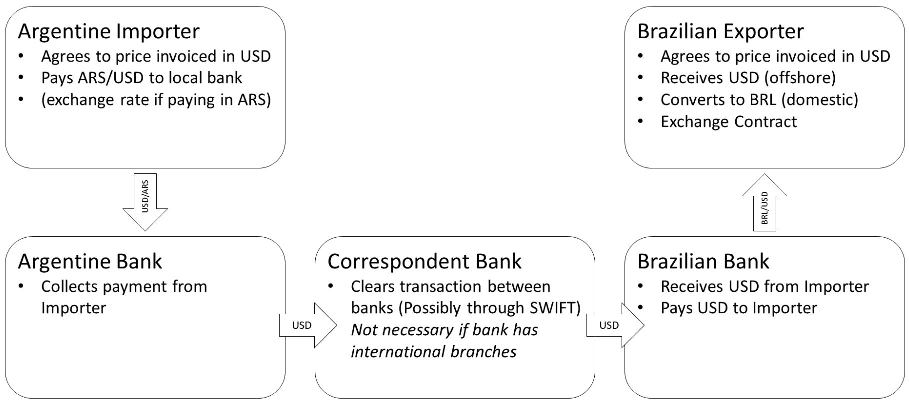
[12]The Real Plan of 1994 introduced the BRL. For more information, see Ayres et al. (2019). In Argentina, the Convertibility Plan of 1991 pegged the ARS to the USD.

[13]See, for example, Edwards (2006)

[14]In Portuguese: Consolidação das Normas Cambiais

[15]The March 2005 reforms did not eliminate the requirement for foreign exchange coverage (in Portuguese: Cobertura cambial). Foreign exchange coverage refers to converting foreign currency receipts to BRL. In 2006, the *requirement* for foreign exchange coverage was eliminated, mainly due to the increased costs to Brazilian firms who wished to use foreign currency receipts to purchase imports. (Presidência da República, 2006) Instead, the Brazilian IRS (Reçeita Federal) began tracking foreign currency coverage and for tax purposes. (Central Bank of Brazil & Reçeita Federal, 2006)

Figure 2.1: Example Transaction



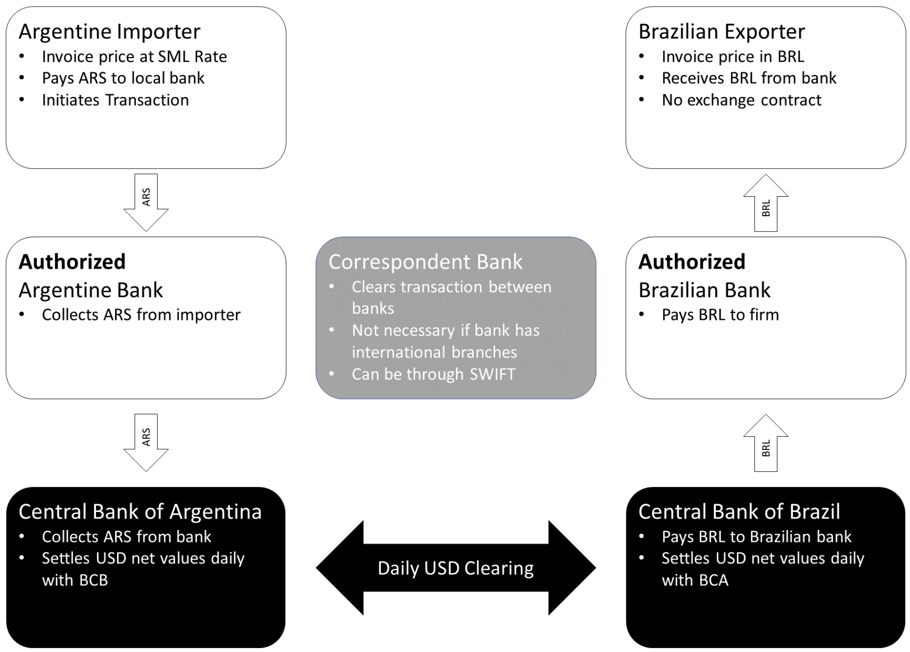
This figure shows the flow of payments from an Argentine importer to a Brazilian exporter prior to the introduction of the SML system. **Source**: Caldentey et al. (2013)

for Brazilian exporters to operate in their local currency, the Brazilian real (BRL), and Argentine importers to operate in Argentine pesos (ARS). Trade contracts are therefore settled without the usage of a third vehicle currency such as the United States dollar. As a requirement for using this system, Brazilian exporters must invoice their exports in reals. In this sense, for a Brazilian exporter, the price is set in real prior to the execution of any foreign exchange conversion. For the Argentine importer, the price is paid in pesos, with the peso price determined by the realization of the SML exchange rate.

To understand how the SML system works, consider Figure (2.2). An Argentine importer, who has agreed with a Brazilian exporter to use the SML system, registers the transaction and executes the payment in pesos at her local financial institution. That financial institution then registers and delivers the payment in pesos to the Argentine Central Bank, which clears the transaction with the Brazilian Central Bank. This clearing takes place daily in dollars. Because most foreign exchange reserves are held in dollars anyway, there is no direct exchange risk as a result of the SML system. The Brazilian Central Bank then transmits the funds (in reals) to the financial institution two days later, which releases the funds to the exporter.

The SML system is non-compulsory. Brazilian exporters are allowed to invoice and settle trade in dollars or reals even without using the SML system. What the SML system provides,

Figure 2.2: Example Transaction through the SML system



This figure shows the flow of payments from an Argentine importer to a Brazilian exporter under the SML system. **Source**: Caldentey et al. (2013)

however, is the ability to contract with the Argentine importer so that both parties may avoid the direct usage of the dollar. The main benefit of the SML system is therefore the ability to invoice and settle exclusively in local currency by both parties. This eliminates, for the exporter, uncertainty or the need to rely on expensive derivative contracts to hedge exchange rate risk. For the importer, as the SML system is a payments system, they still benefit from no longer needing to directly use the dollar, but face uncertainty due to movements in the SML exchange rate.[16]

There were two main goals of this policy. (Meirelles, 2008) First, the SML was designed to reduce obstacles to trade for small and medium sized firms. In emerging markets with foreign currency capital controls, such as Brazil, the usage of foreign currency can involve substantial amounts of paperwork and documentation such as foreign exchange contracts. Second, the SML system was to deepen the real-peso exchange rate market. Turnover in

[16]The central bank also takes on some of this exchange rate risk, albeit at a significantly lower marginal cost. The Brazilian and Argentine central banks net out the difference in SML system transactions each business day. The central banks already hold a large amount of dollar reserves. Therefore, while a large change in the value of BRL relative to the dollar would of course affect the balance sheet, this effect would have occurred regardless of the imposition of the SML system.

Figure 2.3: Take-up of the SML System

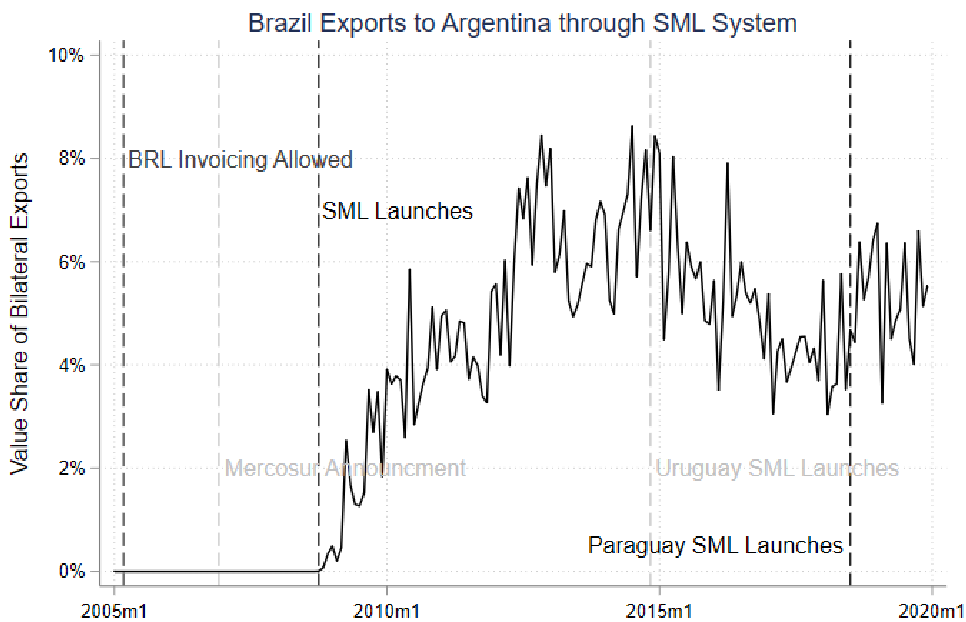


Figure plots SML usage as a share of total exports to Argentina. **Source**: Brazilian Central Bank, World Bank, SECEX

this market was very small. According to the BIS triennial survey of 2010, approximately 5 million dollars per day of spot exchange rate trades involved the Brazilian real in Argentina out of a total of nearly 1.5 billion. This amount has barely moved since. As this paper is focused on the trade effects of the SML system, I do not investigate its role in any foreign exchange markets directly.

Usage of the SML system steadily rose following its introduction. (Reiss, 2015) Figure (2.3) plots take-up of the SML system as a share of total exports to Argentina.[17] Following the introduction of the policy, the share of exports through the SML system rose to nearly 8% by 2012, and has remained elevated at close to 6% after dipping slightly in 2017. These amounts are not small. In 2007, Argentina was the second largest export destination after the United States, just ahead of China. By 2015, Argentina was still a top five export destination, having been overtaken by China and on par with the Netherlands.

[17]Data for these figures comes from publicly available data on export trade, reported by the Brazilian Comércio Exterior, and SML data, reported by the Central Bank. The trade data is in USD, so I convert to BRL using the monthly average exchange rate available from FRED (mnemonic "DEXBZUS")

2.3 Data Sources

Brazilian Transaction-Level Export Data

I use confidential, administrative transaction-level export data from Brazilian customs administration Secretariat of Foreign Trade (SECEX) to construct a quarterly database of export transactions.[18,19] Each transaction is identified by the 14-digit establishment identifier code, known as the *CNPJ*, the eight-digit NCM Code, which includes the standard Harmonized System (HS) code for the first six digits plus two additional digits, and the destination country. While I refer to an individual *CNPJ* as a "firm," it is more akin to an establishment. For each transaction, I observe the value of the trade in USD, the currency of invoicing, and when available either the net weight in kilograms or the statistical quantity. Along with the value V of the shipment, I also construct a measure of prices. I construct prices as unit values, taking the ratio of total value to a measure of quantity. More information on this construction is available in the appendix.

As the main contribution of this study is to examine how currency choice affects trade, I first present aggregate statistics on currency choice across destinations and across sectors. Table 2.1 presents country-level statistics of currency choice across locations. As in most emerging markets, the USD is overwhelmingly used as an invoicing currency across destinations by Brazilian firms, with over 90% of exports invoiced in USD. The share of exports in BRL by value is also consistently smaller than the share by count, implying that smaller firms are more likely to use BRL as an invoicing currency.[20] The notable exception to this pattern is Argentina for the post-SML period, where value in BRL was larger than the count by BRL.

In the benchmark specifications, I focus on exports to South American countries throughout this paper. This assumes that the natural counterfactual to Argentine exports includes only those exports to other South American countries. I do so for three reasons. As discussed in Section 2.2, these countries had the option to use the CPCR.[21] I also focus on South America because exports to advanced economies with deeper financial markets may also involve borrowing in the destination currency, perhaps indirectly through trade credit. Finally, during this time period most advanced economies suffered from the Great Recession in a way that Latin American countries did not.

Table 2.2 reports summary statistics for the full sample, the South American sample, and Argentina.[22] Two facts stand out. First, South America accounts for nearly half of all export transactions. This is unsurprising from a gravity model perspective. Second,

[18] I stress that I did not have direct access to this administrative data. Instead, code was written and sent to SECEX. SECEX ran the code, and only the results were returned.

[19] This data has been used in, for example, Chatterjee et al. (2013) to study the effect of exchange rates on quality upgrading.

[20] A similar result is found in other studies. See for example Amiti et al. (2018).

[21] Although by this point, they rarely used the mechanism.

[22] The South American sample includes the countries in Table 2.1: Argentina, Bolivia, Chile, Colombia, Ecuador, Guyana, Paraguay, Peru, Uruguay, and Venezuela.

Table 2.1: Currency Distribution Across Destinations

	2005Q1-2012Q4				2005Q1-2008Q2				2008Q3-2012Q4			
	Count		Value		Count		Value		Count		Value	
	USD	BRL	USD	BRL	USD	BRL	USD	BRL	USD	BRL	USD	BRL
Argentina	88.0	2.1	94.8	4.4	91.1	0.0	99.4	0.0	85.6	3.8	92.3	6.9
Bolivia	92.7	6.8	97.1	2.6	98.3	1.1	98.7	1.1	89.1	10.4	96.4	3.4
Chile	99.0	0.0	99.3	0.0	99.3	0.0	99.6	0.0	99.0	0.0	99.2	0.5
Colombia	99.3	0.0	98.7	0.0	99.6		99.5		99.1	0.0	98.2	0.0
Ecuador	99.7	0.0	99.4	0.0	99.8		99.8		99.5	0.0	99.4	0.0
Guyana	98.6	1.0	99.4	0.3	99.6		99.6		98.2	1.6	99.3	0.4
Paraguay	73.7	26.1	90.0	9.4	77.0	22.8	90.7	9.2	71.0	28.1	89.8	9.5
Peru	98.2	0.0	99.1	0.2	98.4		99.6		98.9	0.1	98.9	0.2
Uruguay	89.1	10.5	97.3	2.3	87.3	12.3	97.5	2.0	90.3	9.1	97.2	2.4
Venezuela	99.3	0.1	98.9	0.0	99.6		99.7		98.9	0.2	98.5	0.2

Each column presents the total share of exports, either by count (shipment) or by value, in USD and BRL. More details of the sample construction are found in the appendix. Shares are out of totals, which include shipments without the currency recorded. **Source**: SECEX

Table 2.2: Empirical Sample - Firm-Destination-HS6-Quarter

	Full Sample		**South America**		**Argentina**	
Variable	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Observations	4,673,465		2,011,410		392,324	
$\ln Val_{ijst}$	8.450	3.028	8.063	2.773	8.755	2.977
$\ln p_{ijst}$	2.545	2.201	2.686	2.039	2.737	2.103
BRL_Share_{ijst}	0.025	0.155	0.057	0.230	0.022	0.143

This table presents summary statistics for the customs data. **Source**: SECEX

Argentina accounts for around 20% of export transactions to South America. In addition, exports to Argentina are on average much larger than other exports to South America, by almost a full log point.

Municipality Data

I supplement the customs data using financial data at the municipality level. I use the balance sheets of individual financial institutions operating in Brazil, available from the Central Bank of Brazil. For example, the amount of assets and loans by Banco do Brasil in the municipality of São Paulo is observable. These balance sheets are broken down into various categories for assets and liabilities. I focus only on total credit operations.[23]

[23]In Portuguese, this category is "empréstimos e títulos descontados" (161). I choose this category as it includes working capital type loans, which most closely approximate the type of export financing I am

2.4 Causal Evidence from Municipality Data

The first set of evidence I present leverages municipality variation in access to the SML system.[24] I leverage geographic variation in the market share of SML authorized financial institutions to construct a plausibly exogenous measure of takeup in the SML system. I show that municipalities with high SML market shares have 25% higher relative exports to Argentina compared with low SML market share municipalities.

Methodology

As discussed in Section 2.2, in order to use the SML system a firm must do so through a financial institution that is authorized by the country's Central Bank. In the case of Brazil, the Brazilian Central Bank (BCB) must list on its website that such a financial institution is eligible to use the SML system. Spatial differences across Brazilian municipalities in the presence of SML authorized financial institutions creates plausibly exogenous variation that can be used to tease out selection effects by individual firms and to estimate the causal effects of the SML system on municipal export behavior.

I begin by constructing the relative corporate market share of SML eligible banks within a municipality. As there have been additions to the list of SML authorized financial institutions since the introduction of the SML system in 2008, I focus only on the set of financial institutions that were authorized to participate in the SML system by the end of 2008.[25] This effectively controls for entry into the SML system by financial institutions who may have responded to local demand for the SML system due to expected growth in exports to Argentina.

Table (2.3) lists the largest 10 banks in Brazil by assets as of December 2005, along with their overall loan share and whether or not they are authorized to use the SML system.[26] Two facts stand out. First, the largest financial were most likely to be authorized to use the SML system. This is broadly true, as across all financial institutions, while only 12% of total financial institutions were authorized to use the SML system, these financial institutions accounted for 74% of total assets. Second, however, is that by the loan measure used in this paper, SML authorized banks were not overwhelmingly the largest. In fact, by loans, SML authorized institutions account for only 55% of the total.

I construct the share of total corporate loans within a municipality by SML institutions. I construct these market shares for December 2005, nearly three years prior to the launch of the SML system and before any announcement had been made. Formally, let L_{bt} denotes total

interested in (See Plano Contábil das Instituicões do Sistema Financeiro Nacional or COSIF). However, results are qualitatively similar when including "financiamentos" (162). Results including financiamentos are available upon request.

[24]For this section, I use publicly available, aggregated export data available from the SECEX website.

[25]The list on the BCB's website is only available for the current list of authorized financial institutions. I obtain the historical list of entry into the eligible institution list via the Sistema Eletrônico do Serviço de Informação ao Cidadão.

[26]In this table, a financial institution is defined as a unique 8-digit CNPJ.

Table 2.3: Top Ten Banks by Assets (December 2005) and SML Authorization

Bank Name	Assets (Share %)	Loans	SML (2008)
Banco ItauBank S.A.	31.9	2.2	X
Banco do Brasil S.A.	25.2	15.9	X
Banco Bradesco S.A.	6.9	14.9	X
Caixa Economica Federal	5.6	5.8	
Itau Unibanco S.A.	4.6	8.2	X
Unibanco-Uniao Bancos Bras SA	2.6	6.7	X
Banco Real	2.4	7.1	
Banco Nossa Caixa	1.8	2.4	
Banco Santander Brasil	1.7	2.7	
Kirton Bank	1.7	4.0	
Banco Safra	1.5	4.2	

This table presents a summary of the largest financial institutions in Brazil by the share of assets. The first column shows the share of assets, the second columns show the share of loans, and the final column shows whether or not the financial institution was a member of the SML system by the end of 2008. **Source**: Brazilian Central Bank

loans by bank b at time t. The market share of loans by SML eligible banks in municipality m is given by

$$SML\_Share_m = \frac{\sum\limits_{b \in \Omega_m^{SML}} L_{b,2005m12}}{\sum\limits_{b \in \Omega_m} L_{b,2005m12}}$$

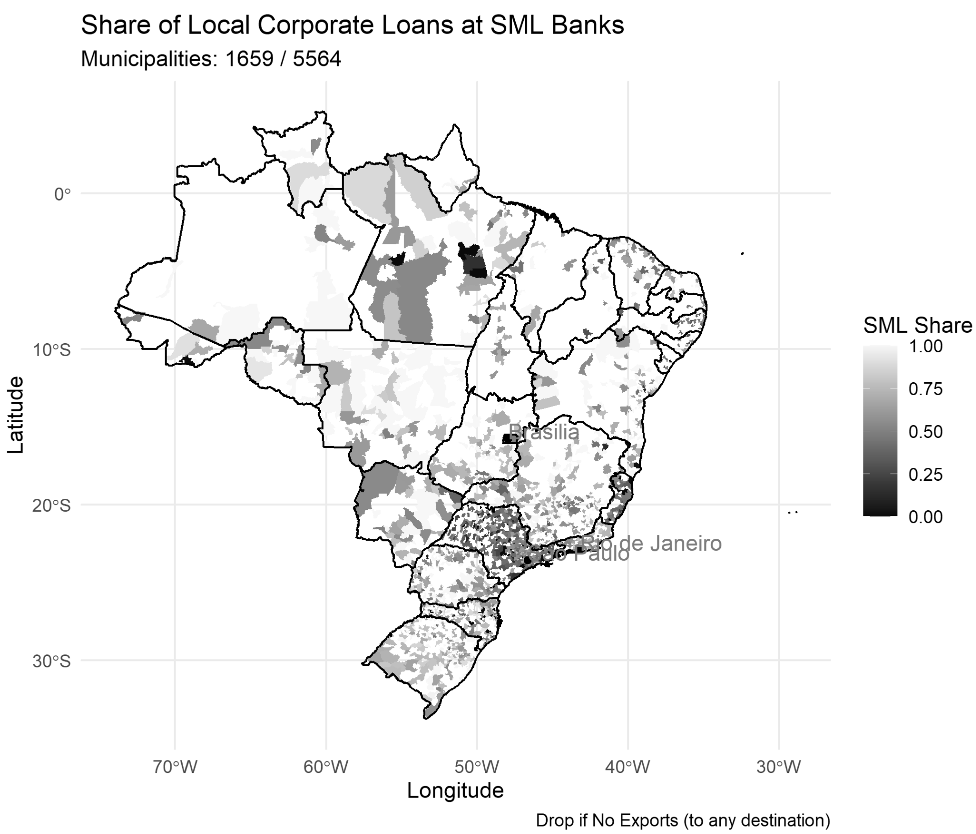
where b indexes banks, Ω_m is the set of all banks in municipality m, and Ω_m^{SML} is the set of all SML eligible banks in municipality m.

To view the spatial variation in SML shares, Figure (2.4) maps the distribution of SML market shares across Brazil. What stands out is the substantial heterogeneity both across and within states. The lighter colors represent higher market shares of SML assets. These municipalities are more likely to be located in the center and southern region of Brazil. The darker colors represent lower market shares of SML assets. These municipalities are occur more often in the South East near the major cities where there is a larger number of unique banks operating and the service sector is more prevalent.

Figure (2.5) plots a histogram of the SML-authorized market shares across municipalities along with a vertical line at the median value. As can be seen, the distribution is slightly top heavy with a large number of municipalities having 100% of assets at SML-authorized financial institutions. However, there is a large mass of municipalities at the bottom of the distribution as well, with between 0% and 25% of assets as SML authorized institutions.

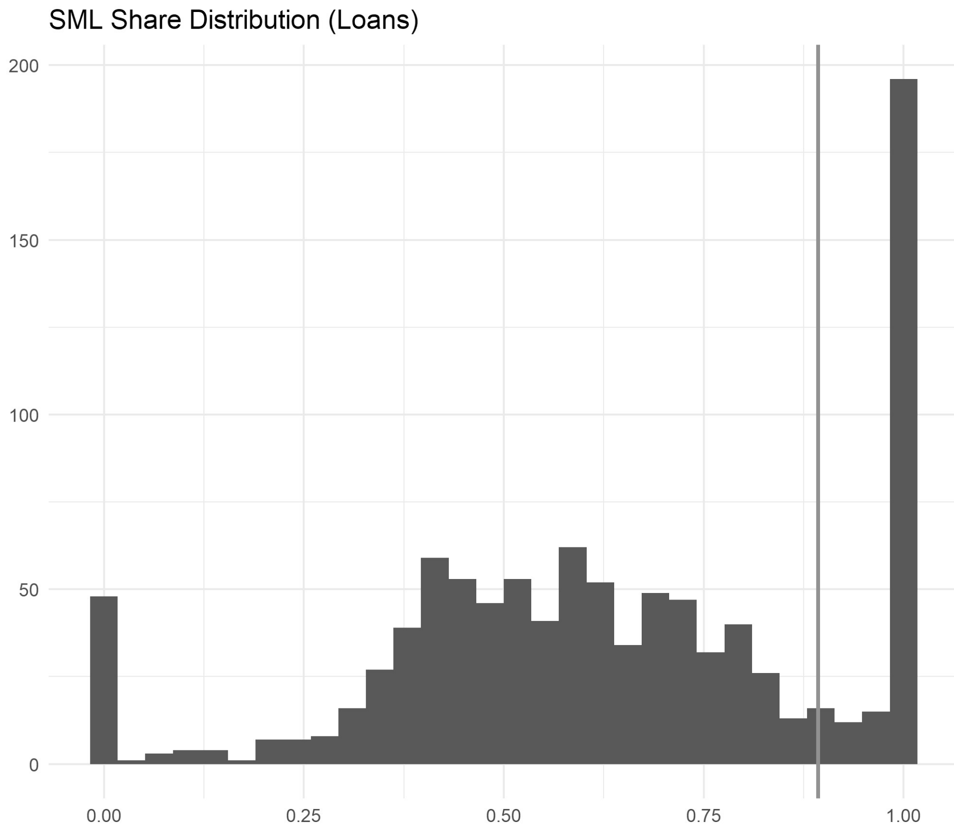
Armed with a measure of SML intensity by municipality, I now turn to estimate the causal effect of the SML system on municipality export behavior. The municipality data is

Figure 2.4: Map of SML Shares



Each polygon within the figure denotes a municipality in Brazil. The color of the municipality signifies the market share of loans at SML authorized institutions. Municipalities with either no financial data or no export data are in white. **Source**: Brazilian Central Bank, SECEX

Figure 2.5: Distribution of SML Shares



This figure plots the distribution of SML_Share_m, with a vertical line drawn at the median value. **Source**: Brazilian Central Bank

aggregated to the two-digit HS sector and destination at the annual frequency. I hypothesize that municipalities with larger SML asset shares had relatively larger export shipments in the years following the launch of the SML system. To estimate this relative effect, I use the following differences-in-differences-in-differences (triple difference) regression design.

$$y_{msjt} = \alpha + \beta_t \left(ARG_j \times \widetilde{SML\_Share_m} \right) + \mathbf{X}_{msjt}\Gamma + \varepsilon_{msjt} \tag{2.1}$$

where y_{msjt} denotes the log export value for HS2 sector s in municipality m to destination j in year t. ε_{msjt} is the regression residual. The definition of treatment at the municipality level that I use, $\widetilde{SML\_Share_m}$, is a dummy variable equal to 1 if the SML share is above the median level across municipalities. As explained in detail below, β_t represents the triple difference estimate of the SML system. I normalize these coefficients to be relative to β_{2007}, which is the year prior to the launch of the SML system. $\mathbf{X}_{msjt}$ denotes controls that vary possibly at the municipality-sector-destination-time level. In the benchmark specification, I include municipality-time, destination-sector-time, and state-destination fixed effects. I also include the main interaction of $ARG_j \times \widetilde{SML\_Share}_m$.

The coefficients of interest are the time series β_t. The triple difference specification lends an interpretation to these β_t's that incorporates two relative effects. First, the β_t's leverage the effect of exports to Argentina relative to exports to other South American destinations within a municipality. Second, they leverage the effect of being in a high SML market share municipality relative to a low SML market share municipality. Taken together, the β_t's represent the effect of being above the median SML share on exports to Argentina relative to exports to other destination, compared with the same relative effect in municipalities below the median in year t.

In order for the coefficients β_t to have a causal interpretation, it must be the case that the evolution of log exports across municipalities to Argentina relative to other South American export destinations would have been the same in the absence of the existence of the SML system. This is the parallel trends assumption common in many difference in difference specifications. There are three main threats to identification.

First, the reason municipalities have a large share of SML authorized banks may be because of demand by local firms. Local exporters may have petitioned or lobbied their local banks to become authorized for the SML system. Such lobbying behavior may be correlated with unobserved determinants of export behavior, such as productivity, that may also adjust at the same time as the the introduction of the SML system.[27] This concern is difficult to directly account for in the absence of an observable predictor of SML authorization that is orthogonal to unobserved determinants of exports to Argentina. Instead, I rely on the fact that obtaining SML authorization was a national decision, rather than a municipality specific decision. I show in robustness checks that conditioning only on municipalities for which no SML authorized bank has more than 2% market share does not qualitatively change the results. To the extent that these municipalities had little effect on the national decision to participate in the SML system, then the selection decision of banks would be accounted for.

A second threat to identification concerns whether the presence of SML-authorized institutions is correlated with other determinants of export behavior that may have occurred at the same time as the introduction of the SML system. In particular, the global trade collapse of 2008 or the global financial crisis may have disproportionately affected municipalities that had fewer SML-authorized institutions if such institutions were more or less resilient to global conditions. The triple difference specification partially alleviates this concern by comparing exports to Argentina relative to other export destinations conditional on destination-time controls. Still, the identification assumption may be violated if there is some other export destination that also experienced a relative increase in export volumes. In placebo tests, I show that there is no relative effect of the SML system towards other export destinations in South America.

[27] A related identification concern that is observable is if municipalities with high SML authorized market shares have higher relative growth in exports to Argentina, thus pushing their local firms to demand SML authorization. Such a concern is observable in the estimated pretrends.

Finally, SML-authorized banks may have simply chosen to increase market share in municipalities for which relative exports to Argentina were growing in order to increase future income. Using market shares in December 2005 helps to alleviate this concern. Market shares in December 2005 are likely to be exogenous to selection into municipalities by individual banks for two reasons. First, the long time lag between the observed market shares and the announcement and launch of the SML system makes it unlikely that financial institutions chose where to locate in anticipation of its usage. Second, import/export financing accounts for a relatively small portion of bank profits, of which Argentina is a moderate fraction. Banks were therefore unlikely to make location decisions based solely on the SML system.

Results

Figure (2.6) plots the coefficients β_t from estimating Equation (2.1).[28] First, note the lack of any significant pretrend prior to the launch of the SML system between 2003-2007. The coefficients are insignificantly different than zero, suggesting that there is no relative difference across municipalities in terms of SML intensity with respect to relative exports to Argentina. This provides confidence that the parallel assumption, that relative growth in exports to Argentina would have been similar across municipalities in the absence of the SML system, holds in the data.

Following the introduction of the SML system in 2008, there is a significant rise in log export values to around 0.25 log points by the end 2012. The effect stays elevated through 2015. Recall that identification leverages only the time-variation before and after the launch of the SML system and cross-sectional variation in access to the SML system. Endogenous selection by individual firms or municipalities into using the SML system is assumed to be orthogonal to the timing of the introduction of the SML system or the cross-sectional distribution of financial institutions. The lack of significant pre trends suggests that firms in high-SML municipalities did not anticipate the opening of the SML system.

To arrive at the overall effect of the SML system, and hence the causal effect of eliminating exchange rate risk, I estimate the following triple differences specification

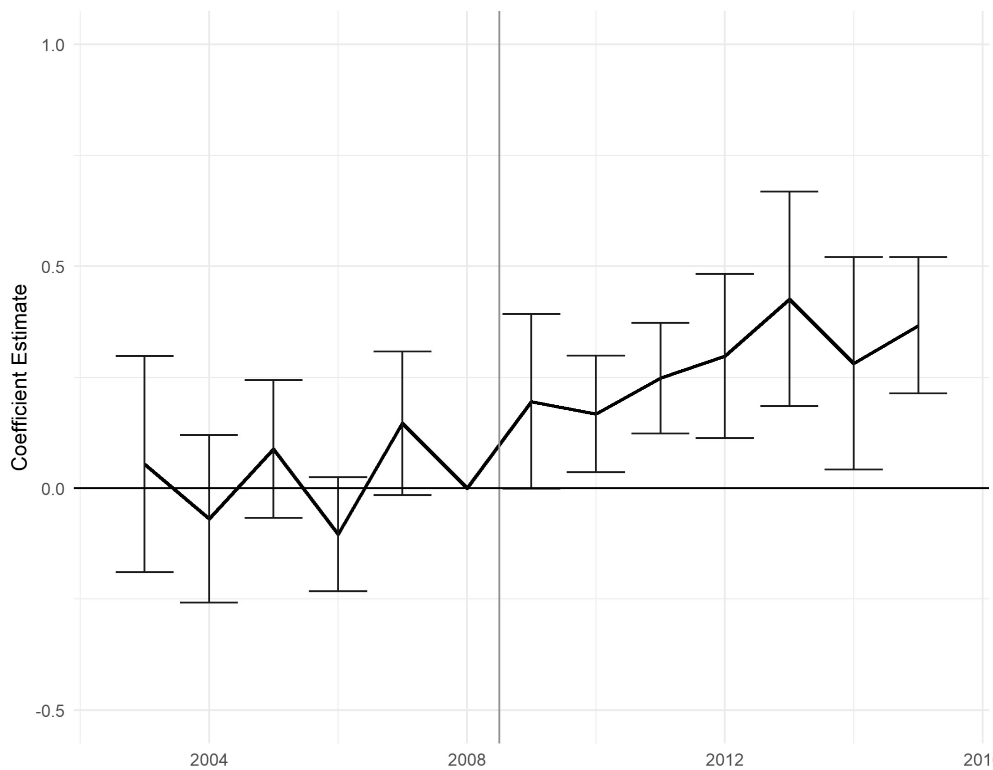
$$\ln y_{msjt} = \alpha + \beta \left(POST_t \times \widetilde{SML\_Share}_m \times ARG_j \right) + \mathbf{X}_{msjt}\Gamma + \varepsilon_{msjt} \tag{2.2}$$

This equation is similar to Equation (2.1), except the effect is assumed to be static rather than dynamic; I replace the β_t's with only one β and include a $POST_t$ dummy that is equal to one in all $t \geq 2008$. Controls, $\mathbf{X}_{msjt}$, are as in Equation (2.1).

The first column of Table (2.4) presents results for this estimation. The coefficient estimate of 0.222 (SE: 0.090) implies that following the introduction of the SML system, exports to Argentina relative to other South American export destinations rose by approximately 22.2% municipalities with a high SML market share relative to municipalities with a low SML market share.

[28] Standard errors are two-way clustered by municipality-HS2 and date and are plotted at the 95% level.

Figure 2.6: Relative Effect of SML Financial Institutions



This figure presents results from estimating the triple difference specification given by Equation (2.1): $y_{msjt} = \alpha + \beta_t \times \left(\widetilde{SML\_Share}_m \times ARG_j\right) + \mathbf{X}_{msjt}\Gamma + \varepsilon_{mst}$, where the outcome y_{msjt} is log exports in sector s from municipality m in year t to destination j, $\widetilde{SML\_Share}_m$ is a dummy variable equal to 1 if a municipality has above the median market share of SML corporate loans, and ARG_j is a dummy equal to 1 if the destination is Argentina. I include municipality-time, destination-sector-time, and state-destination fixed effects as well as main interactions. Standard errors are two-way clustered by municipality-sector and time. **Source**: Brazilian Central Bank, SECEX

One way to interpret the estimates is to compare them to estimates of the trade effect of joining a currency union. Rose (2014a) finds that the effect of joining a currency union is to increase exports by around 54%, similar to the estimate of 47% found in an earlier meta study by Rose and Stanley (2005). The ascension to a currency union incorporates much more than simply changing to a common currency. My estimate suggests, that a significant share of the trade benefits from joining a currency union come from eliminating foreign currency risk.

There are at two concerns with this specifications that I aim to alleviate here.[29] The first concern with these results is that differential exposure by financial institutions to the global trade collapse or the global financial crisis may have disproportionately affected export resiliency. The triple difference specification above in some sense controls for this effect by taking exports relative to other South American export destinations. Regardless, I conduct placebo tests where I examine how exports to *other* destinations changed following the launch of the SML system. Specifically, I estimate the following regression

$$\ln y_{msjt} = \alpha + \beta \left(POST_t \times \widetilde{SML\_Share}_m \times Dest_j \right) + \mathbf{X}_{msjt}\Gamma + \varepsilon_{msjt} \tag{2.3}$$

This equation is similar to Equation (2.2). I run the regression multiple times, replacing $Dest_j$ with replace specific destinations to compare the inspect of the SML system on relative exports to different destinations. For example, setting $Dest_j = ARG_j$ means that $Dest_j$ is a dummy equal to 1 if the destination is Argentina. Setting $Dest_j = COL_j$ means that $Dest_j$ is a dummy equal to 1 if the destination if Colombia. When Argentina is not the destination, I drop exports to Argentina. The controls, $\mathbf{X}_{msjt}$, are as in Equation (2.1).

In columns (2)-(5), of Table (2.4), I drop all exports to Argentina and test the effect of the SML system on relative exports to other South American destinations: Colombia, Chile, Paraguay, and Uruguay. In all other columns, the coefficient on the triple interaction is insignificant and close to zero, suggesting there is little effect of the SML system on exports to other destinations.

A second concern with these results is that individual financial institutions may also have opted into the SML system due to persuasion efforts by their clients. Individual firms who wish to select into the SML system may have lobbied their financial institutions to do so. This would create correlation between the endogenous selection problem of firms and the endogenous selection problem of banks.

To account for this concern, I rely on the fact that the decision to opt into the SML system was a national one from the perspective of banks. I assume that the pressure exerted

[29]In the appendix, I discuss two other robustness checks. First, I show that results are robust to balancing the sample after aggregating up to the municipality-time level, and I show that results are robust when conditioning on municipality-sectors with some pre-period observations. Second, I explore using the continuous measure SML_Share_m as opposed to the binary measure. The SML system was more costly to large banks than it was to small banks due to returns to scale in spot exchange rate markets. After assuming that the two largest banks in Brazil were not SML banks, I show that under this assumptions, results are similar when using the continuous measure.

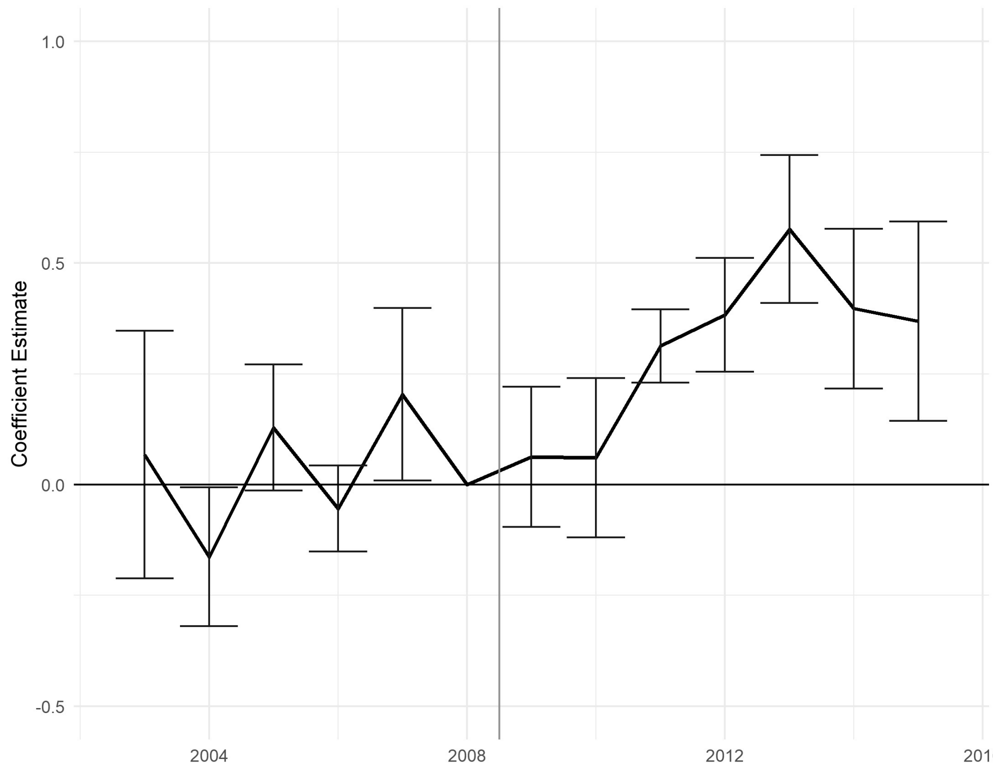
Table 2.4: Placebo Tests

	Dependent variable:				
	Log Value				
	(1)	(2)	(3)	(4)	(5)
Dest x SML	-0.362^{***}	-0.033	0.045	-0.166	-0.091
	(0.090)	(0.151)	(0.169)	(0.163)	(0.164)
Post x Dest x SML	0.222^{**}	-0.023	0.011	0.025	0.0004
	(0.090)	(0.163)	(0.039)	(0.133)	(0.126)
Country	Arg	Col	Chl	Par	Ury
Muni-Time FE	Y	Y	Y	Y	Y
HS2-Cou-Time FE	Y	Y	Y	Y	Y
State-Cou FE	Y	Y	Y	Y	Y
Observations	406,847	343,898	343,898	343,898	343,898
R^2	0.344	0.343	0.343	0.343	0.343
Adjusted R^2	0.297	0.292	0.292	0.292	0.292
Residual Std. Error	2.664	2.625	2.625	2.625	2.625

This table presents results from estimating the triple-difference specification given by Equation (2.3) $\ln y_{msjt} = \alpha + \beta \left(POST_t \times \widetilde{SML\_Share}_m \times Dest_j \right) + \mathbf{X}_{msjt}\Gamma + \varepsilon_{msjt}$. Controls include municipality-time fixed effects, state-destination fixed effects, HS2-destination-time fixed effects, and main interactions. Each column of this table replaces the term $Dest_j$ with a dummy variable equal to one if the destination j is equal to the country denoted in the Country row. With the exception of Column (1), all exports to Argentina are dropped from the sample. **Source**: SECEX

$* \; p < 0.1$; $** \; p < 0.05$; $*** \; p < 0.01$. Standard errors are two-way clustered by municipality-sector and time.

Figure 2.7: Dropping Municipalities with Greater Than 2% Market Share



This figure presents results from estimating the triple difference specification given by equation (2.1): $y_{msjt} = \alpha + \beta_t \times \left(\widetilde{SML\_Share}_m \times ARG_j\right) + \mathbf{X}_{msjt}\Gamma + \varepsilon_{mst}$, where the outcome y_{msjt} is log exports in sector s from municipality m in year t to destination j, $\widetilde{SML\_Share}_m$ is a dummy variable equal to 1 if a municipality has above the median market share of SML corporate loans, and ARG_j is a dummy equal to 1 if the destination is Argentina. I include municipality-time, destination-sector-time, and state-destination fixed effects as well as main interactions. Municipalities with at least 2% market share of any SML institution are dropped. Standard errors are two-way clustered by municipality-sector and time. **Source**: Brazilian Central Bank, SECEX

by a municipality for a bank to join the SML system is proportional to its portfolio share. For example, a municipality that accounts for 10% of a bank's loan portfolio is likely to be influence a bank's national decision compared with a municipality that accounts for only 1% of a bank's loan portfolio.

I drop municipalities for which the share of total loans by the financial institution is equal to or above 2%. These municipalities are mainly large municipalities such as Sao Paulo. Figure (2.7) shows the results. While the initial effect is attenuated, the significant rise in exports remains in the years following the introduction of the SML system.

2.5 Micro Evidence of the SML System

I complement the municipal analysis in Section 2.4 with a detailed look at the evolution of export behavior by individual firms using confidential customs data. The analysis proceeds in two parts. First, I show how BRL-invoicing behavior proxies for SML takeup, and use this measure to understand the heterogeneity across firm takeup of the SML system. Second, I estimate the effect of the SML system on export volumes and prices using detailed fixed effect regression designs.

Time-Varying Invoicing Decision

I construct a time-varying measure of participation in the SML system at the transaction level. I define SML take-up by Brazilian exporters to Argentina as whether or not the export is invoiced in BRL. Recall that SML statistics are recorded by the Central Bank, while I only observe customs data that includes only the currency of invoicing. However, a requirement of using the SML system is that the export transaction must be invoiced in BRL, so BRL usage can be thought of as an upper bound on SML takeup. In Figure 2.8, I compare the value share of exports to Argentina invoiced in BRL from the customs data to the value share of SML usage reported by the Central Bank. Both track each other similarly, giving confidence that BRL invoicing represents SML usage.[30]

I define BRL-invoicing at the transaction level as a dummy variable equal to one if if the percentage of exports invoiced in BRL within a given period is at least 50%. Formally, policy take-up, denoted by ι_{isjt} for firm i in sector s exporting to destination j at over time period t, is given by

$$\iota_{isjt} = \mathbf{1}\left(\frac{V_{ijst}^{BRL}}{V_{ijst}} > 0.50\right)$$

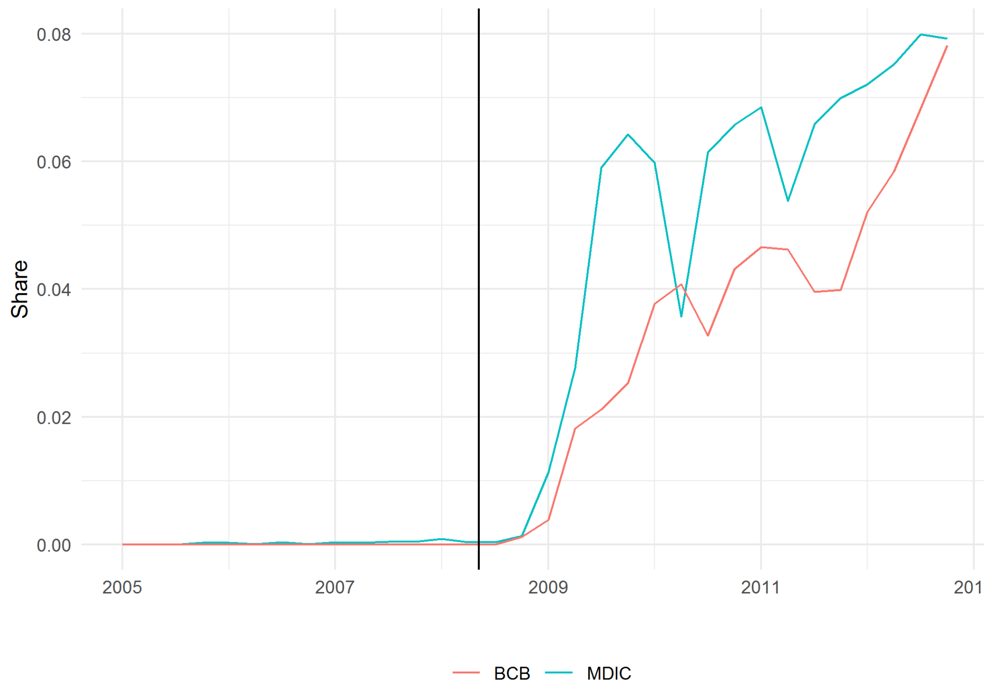
where V_{isjt}^{BRL} denotes the total dollar value of the transaction that is invoiced in BRL and V_{ijst} is the total dollar value of exports in any currency.[31] Figure 2.9 plots the distribution of value shares over the entire sample, with a vertical line at 10%. As can be seen, the vast majority of 4-digit commodity codes at the firm-destination-quarter level are either 0% or 100%.[32]

[30]That both do not line up exactly is likely the result of two factors. First, the BCB reports SML usage in BRL, while export data is denominated in USD. The exchange rate adjustment I use may be imprecise. Second, it is possible that some firms invoice in BRL when exporting to Argentina while not using the SML system. Additionally, as one of the goals of the SML system was to reduce reliance on the USD in export transactions, it may be that some firms that initially used the SML system switched to simply invoicing in BRL. Unfortunately, without detailed data on SML usage, I cannot examine this possibility.

[31]Recall that the data is constructed by aggregating monthly data by 8-digit sector to quarterly data by 4-digit sector. I therefore choose a threshold greater than 0 to ensure it ensure that no small month or 8-digit sector drives results.

[32]I experimented with setting the cutoff to be 0% and 10%, and the results are unchanged. t

Figure 2.8: BRL Invoicing and SML Usage



This figure compares the share of exports invoiced in BRL from the customs data with the share of total exports reported as SML exports via the Central Bank of Brazil. **Source**: Brazilian Central Bank, SECEX

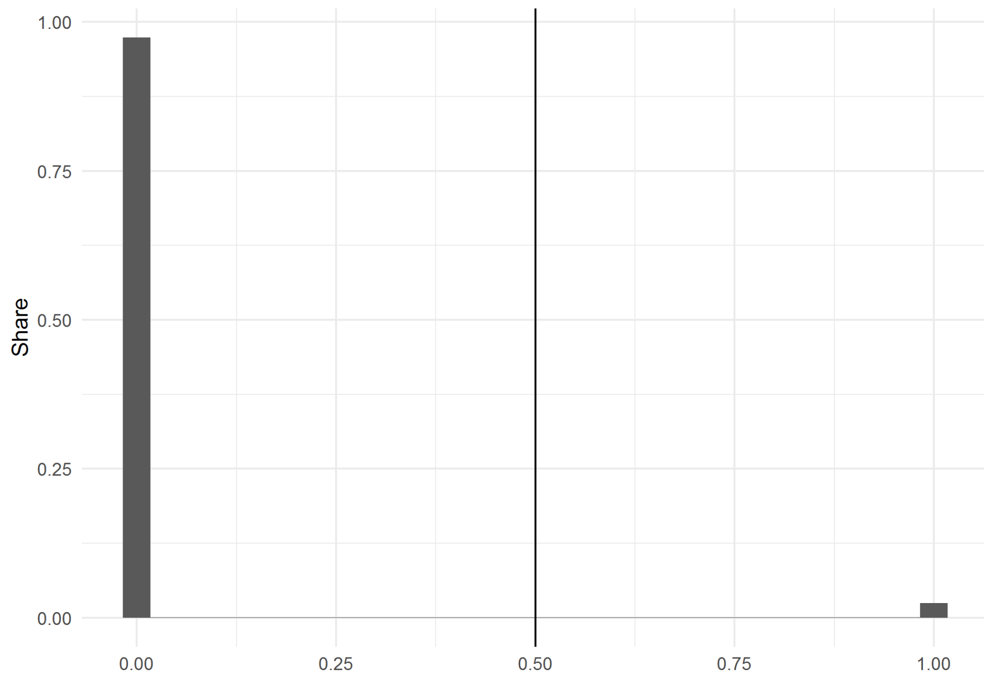
Figure 2.10 plots the quarterly average value of the measure of policy takeup, ι_{isjt}, for Argentina ($j = ARG$) over the sample period, with 2-standard deviations error bands. While ι_{isARGt} is effectively zero before the implementation of the SML system, there is a steady increase to approximately 8% by 2012. This rise is even larger when looking at the share of *firms*, as opposed to firm-sectors, that switch to BRL invoicing. Figure 2.11 shows that the ι share across firms rises to nearly 10%.[33]

It may seem strange that not all eligible firms take up the SML system. There are at least three reasons why they may not. First, firms that rely on imported intermediate goods from other locations may benefit from the natural hedge that dollar export revenues provide.[34] Similarly, firms with borrowing denominated or indexed to USD borrowing rates may also

[33]For comparison, Figure A.9 plots the evolution of ι_{isjt} for Colombia. The share of transactions invoiced in BRL is orders of magnitude smaller, and there is no break following the introduction of the SML system. Figure A.10 plots the increase in ι_{isjt} relative to other Latin American countries, showing that the rise is unique to Argentina.

[34]Unfortunately, invoicing data on imports is not available from SECEX, but rather by the Receita Federal. Because Argentine exporters and Brazilian importers barely used the SML system, it is reasonable to conclude that the currency of invoicing for imports, and hence exchange rate exposure through marginal costs, did not change significantly.

Figure 2.9: Distribution of BRL Shares of Transactions



Histogram of BRL-invoicing for exports, V^{BRL}_{ijst}/V_{ijst}. V_{ijst} is the dollar value of total exports to destination j by firm i in 4-digit sector s in quarter t. V^{BRL}_{ijst} is the dollar value of BRL-invoiced exports. (Values strictly greater than 0 or strictly less than 1 are available in Figure (A.5)) **Source**: SECEX

prefer the natural hedge from foreign currency export revenues. Second, given that the SML system requires opting in from *both* the importer and the exporter, some Argentine importers may not have wished to use the SML system. The Argentine exchange rate has historically been very volatile, and holding deposits and operating in USD is common in Argentina. Finally, the system was announced with little fanfare, so a number of firms may have taken time to learn more about the system. Caldentey et al. (2013) argue that even by 2014, many firms may have been unaware of the system's existence.

I next decompose ι_{isARGt} along three dimensions: sector, size, and number of destinations. As mentioned in Section 2.3, small exporters typically invoice in their home currency. Additionally, it is traditionally assumed that because most commodities are priced in dollars on commodity exchanges, then it is only natural for commodity exports to be invoiced in USD. It may be tempting to assume, then, that only small exporters in non-commodity sectors used the SML system.

I find that all manner of exporters, including large and multi-destination exporters, switched took up the SML system. Figure 2.12 decomposes the ι_{isARGt} along three different dimensions. The top-left panel decomposes ι_{isARGt} by size as measured by total 2007 exports. While the entering and small firms took up the SML system at higher ates, even

Figure 2.10: BRL-Invoiced Exports to Argentina

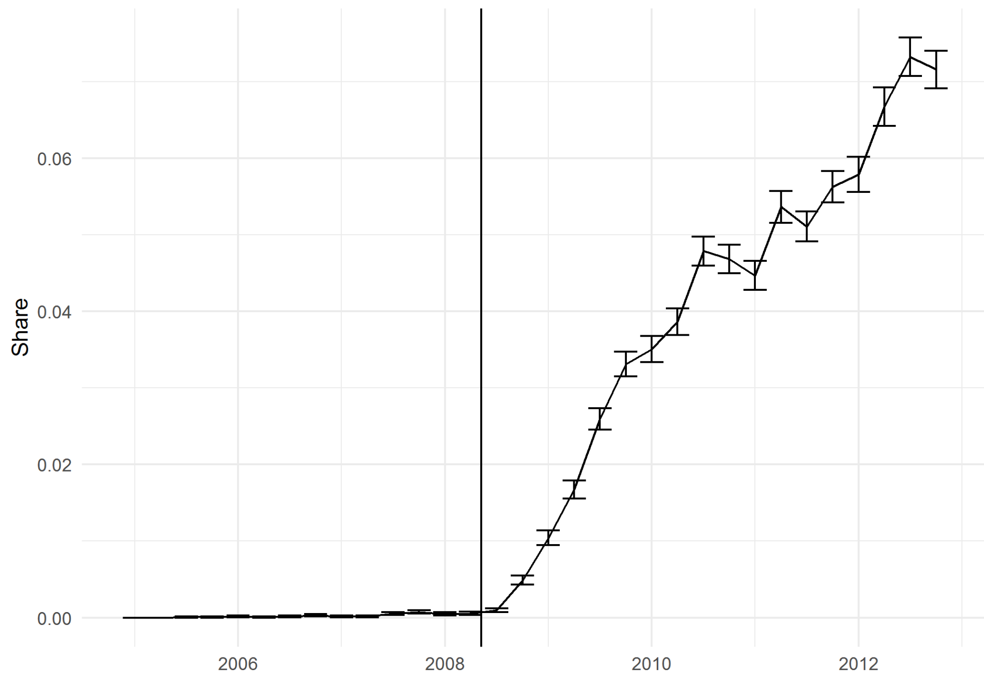


Figure shows the mean and standard deviation bands of the value of ι_{iARGst}. ι_{iARGst} is a dummy variable equal to one if at least 50% of the value of exports to Argentina by firm i in 4-digit sector s in quarter t are invoiced in BRL. **Source**: SECEX

firms in the top half of the size distribution switched to BRL invoicing through the SML system.

The top-right panel decomposes the rise in ι_{isARGt} by the number of export destinations. Again, while firms that only exported to Argentina saw the highest takeup, a non-trivial share of exporters with at least ten different export destinations also had high SML takeup.

Finally, the last panel shows the change in invoicing split between commodity and non-commodity exports.[35] Both sectors experienced similar increases in BRL-invoicing. This coarse classification, however, masks considerable heterogeneity. For example, within the HS Section "Live Animals", dairy produce (HS04) experienced a sharp rise in BRL invoicing whereas meat did not (HS02).[36]

These simple decompositions suggest that the invoicing change was not due to sector-specific or time-specific events. To understand better the sources of variation across policy takeup, I perform a variance decomposition of the change in invoicing currency on varying sets of fixed effects. The R^2 of this regression provides insight into where most of the variation

[35] Commodity exports are defined as in Boz et al. (2018). See the appendix for more details.

[36] In the appendix, I show the rise in ι_{isARGt} across all two-digit HS sectors. The heterogeneity across HS2 sectors does not appear to have any obvious pattern.

Figure 2.11: Argentina ι Shares by Firm

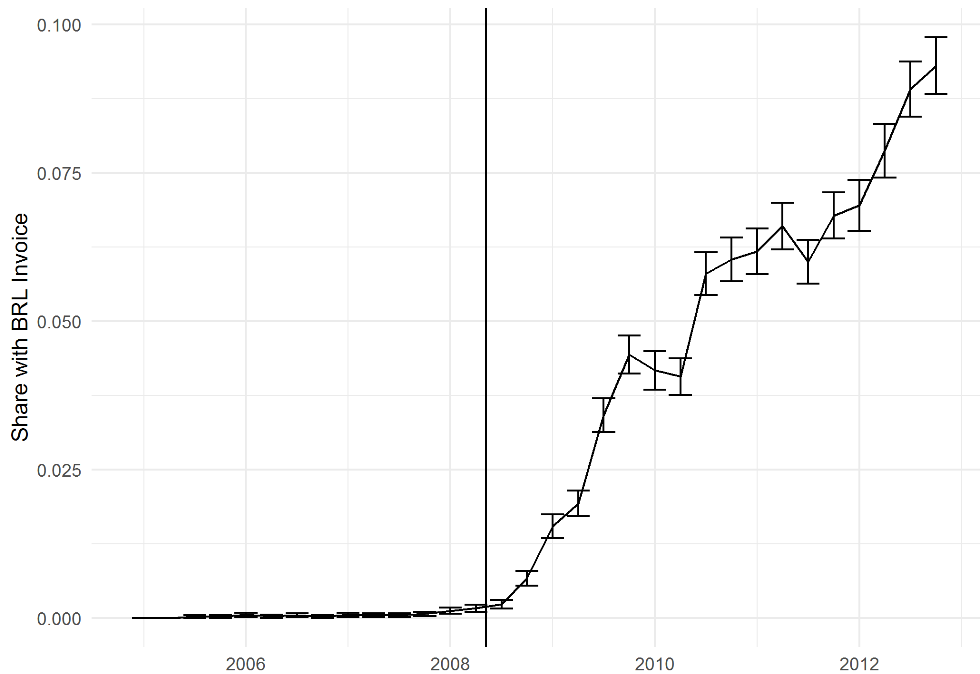


Figure shows the mean and standard deviation bands of the value of ι_{iARGt}. ι_{iARGt} is a dummy variable equal to one if for at least one sector exported by for i to Argentina in quarter t has at least 50% of the value of exports invoiced in BRL. **Source**: SECEX

in currency choice is coming from. Formally, I run the following regression

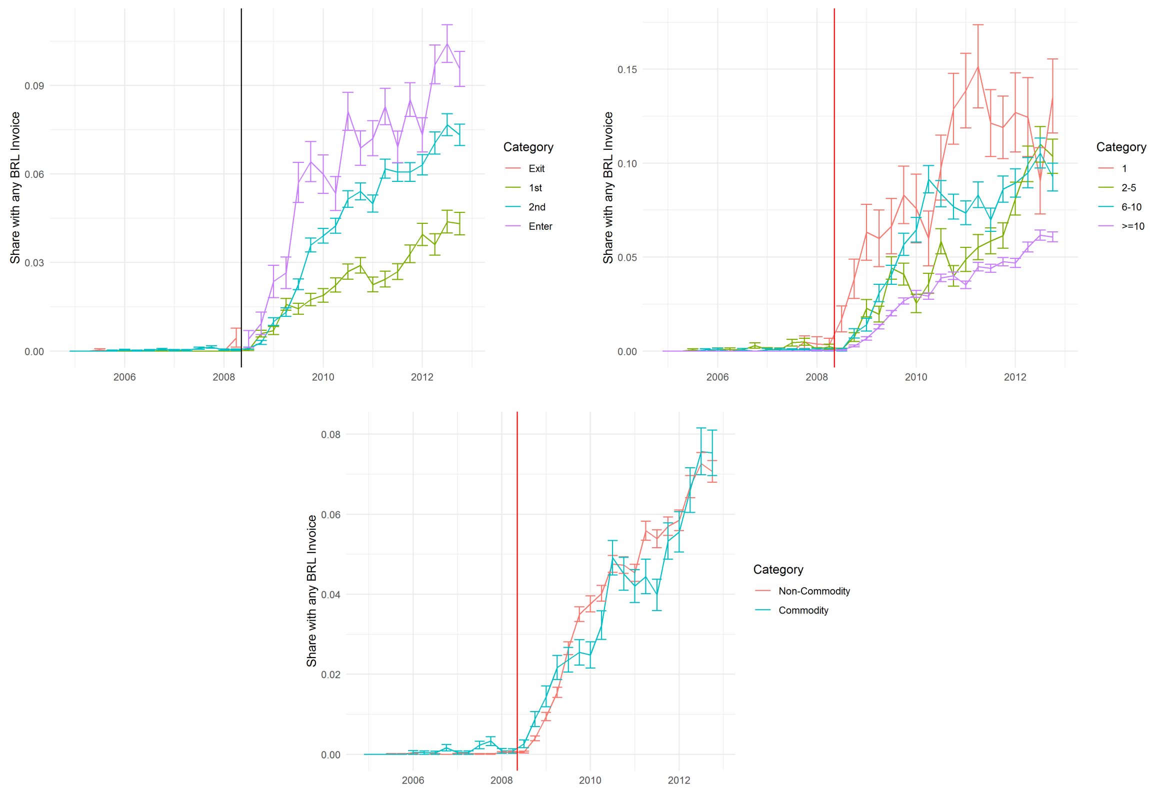
$$\Delta\iota_{iARGst} = \alpha + \varepsilon_{iARGst}$$

The left-hand side variable, $\Delta\iota_{isARGt}$, is equal to one in the first quarter where BRL-invoicing occurs for firm i exporting in sector s to Argentina, assuming that the firm had invoiced at least once in non-BRL previously. $\Delta\iota_{isARGt}$ is therefore the moment a firm switches from USD invoicing to BRL invoicing.[37] α represents different sets of fixed effects to determine whether or not the change in invoicing behavior changes along a specific dimension. This is a similar exercise as in Amiti et al. (2018), although given the time-variation in my data I look at the *change* rather than the level of invoice currency.

The method of calculation of $\Delta\iota_{isARGt}$ only looks at changes from one non-zero export period to the next. One benefit of this specification is that it avoids issues related to imputing invoicing decisions for missing data for firms that may not export every quarter. One

[37] Note that in a small number of cases, the value of this variable is -1. This means that a firm switched from BRL invoicing to USD invoicing. However, in the sample of firms that export to Argentina, this happens only five times.

Figure 2.12: Decomposition of BRL-Invoiced Exports to Argentina



Figures show the mean and standard deviation bands of the value of ι_{iARGst}. ι_{iARGst} is a dummy variable equal to one if at least 50% of the value of exports to Argentina by firm i in 4-digit sector s in quarter t are invoiced in BRL. In the first panel, the categories are determined by size as measure by total exports between 2007Q1 and 2008Q2 (below and above the median), where "exit" and "enter" denote not having or having exports after 2008Q2, respectively. In the second panel, the categories are determined by the total number of export destination by the firm including Argentina. In the last panel, the categories are whether the industry is a commodity as measured by Boz et al. (2018). **Source**: SECEX

downside is that it does not capture effects along the extensive margin. This is because firms that enter and immediately export in BRL are set to zero in this regression. In this sense, the R^2 should be interpreted only as an intensive margin effect. Still, it is informative to see where most variation in changes in invoicing comes from.

Table 2.5, shows that more than 50% of the variation in $\Delta\iota_{iARGst}$ occurs across firm-time cells. This suggests that firms typically adjust their currency of invoicing across all sectors at the same time, rather than one at a time. There is very little variation explained by sector or even sector-time fixed effects. This is because not only is there a wide set of industries that switch to the SML system, but within each industry firms stagger their adoption.

Table 2.5: Variance Decomposition

R^2	0.022	0.002	0.023	0.512	0.031	0.048
Firm	X		X			X
Time		X	X			
Firm-Time				X		
HS4-Time					X	X

This table presents regression results from estimating $\Delta\iota_{isARGt} = \alpha + \varepsilon_{ijARGt}$. ι_{isARGt} is a dummy equal to one if at least 10% by value of a shipment by firm i in sector s to Argentina at time t in in BRL. $\Delta\iota_{isARGt}$ denotes the medium-run change in invoicing behavior, as I take the difference between periods where positive values are observed. α denotes the different levels of fixed effects. **Source**: SECEX.

Methodology

I leverage time-variation in invoicing currency to estimate how changes in invoicing behavior affect the size of export volumes and their prices. The preceding analysis defined the treatment group to be those firm-sector-destinations that invoice in BRL within a given quarter. This BRL-invoicing is meant to capture takeup of the SML system. Firms of a variety of different sizes across different sectors switched to BRL invoicing for Argentina shipments following the introduction of the SML system at staggered times.

I utilize the rise in BRL-invoicing due to the introduction of the SML system to compare otherwise similar transactions that are invoiced in different currencies. This specification is akin to an event study specification whereby once a unit is treated, it remains treated for the remainder of the sample. The regression specification relates the log value of exports and the log price to the time-varying invoicing decision at the shipment level. Formally, the regression to be estimated is

$$y_{isjt} = \alpha + \beta\iota_{isjt} + \gamma\left(\iota_{isjt} \times ARG_{jt}\right) + \mathbf{X}_{ijst}\Gamma + \varepsilon_{isjt} \tag{2.4}$$

where y denotes either the log values of exports or the log price of firm i to Argentina in HS4 sector s during quarter t. $\mathbf{X}_{ijst}$ denotes controls that vary possibly at the firm-destination-sector-time level. I include firm-HS4, HS2-time, and destination-time fixed effects. These fixed effects control for time-invariant transaction-specific effects and aggregate changes in sector-specific or destination-specific conditions, such as demand or supply shocks. In this sense, I compare BRL-invoiced transactions to Argentina with a counterfactual transaction by the same firm in the same sector, holding constant aggregate economic conditions.

I estimate Equation (2.4) for two samples. First, I estimate this equation only for the sample of exports to Argentina (by dropping the interaction term and destination-time dummies). In this specification, I recover estimates of β that using time-variation within a given firm-sector. In the second specification, I include exports to all South American destinations. This specification leverages both time-variation within a given firm-sector but also variation across destinations within a given firm-sector.

The main coefficients of interest are β and γ. Specifically, β represents the marginal effect of BRL invoicing across all export destinations, while γ represents the relative effect of BRL invoicing on exports to Argentina. Because $\iota_{isjt} = 0$ for essentially all exports to Argentina prior to the introduction of the SML system, and the rise in BRL-invoicing closely matches the share of exports through the SML system reported by the Central Bank, the coefficient γ can be interpreted as the effect of the SML system itself relative to non-SML transactions.

I am interested in interpreting γ (or, when using only exports to Argentina, β) as the effect of the SML system. For OLS estimates of γ to have a causal interpretation, it must be the case that the switch to BRL-invoicing is exogenous with respect to other determinants of trade values. In other words, the evolution of export volumes and prices should have been the same for SML and non-SML exports to Argentina in the absence of the SML system.

The main threat to identification involves endogenous selection into the SML system. These selection effects can bias the estimate of the SML system upwards or downwards. If firms are more likely to select into the SML system because sales are falling and they believe the SML system may improve their prospects, then the SML system will be biased downwards. If instead firms select into the SML system because they are growing, and expect the SML system to continue improving their export prospects, then the effect will be biased upwards.

In Section 2.4, I leveraged municipality variation in access to the SML system that is orthogonal to such selection effects and find similar results as in this section. The results of that analysis found that the introduction of the SML system significantly increased exports to Argentina in municipalities more likely to take up the SML system. The methodology in this section, on the other hand, is focused on understanding the extent to which individual firms increased their exports.

I emphasize that for endogenous selection to bias estimates of the SML system, it must be that the reason for taking up the SML system is either time-varying at the firm-sector level. Time-invariant determinants of selection are controlled for by firm-sector fixed effects. However, if firms choose to use the SML system because of, for example, foreign currency borrowing, such reasons would not be controlled for. In an alternative specification, I directly include firm-time controls. These controls account for time-varying characteristics at the firm-level, such as import intensity or foreign currency borrowing. However, given that must variation in changes in invoicing comes from the firm-time dimension, including such controls is likely to result in lower powered estimates.

I do stress that other threats due to omitted variable bias can be directly controlled for. First, there is the effect of the Global Recession.[38] International trade peaked in 2008Q3, which is precisely the same quarter as the introduction of the SML system. (Eaton et al., 2016) It is unlikely this trade collapse is biasing my results. First, as a practical measure, the inclusion of sector-time fixed effects should control for aggregate changes in economic activity,

[38]Note that domestic economic conditions are less likely to be concerned in the case of trade between Argentina and Brazil. This is because, for the most part, economic conditions did not worsen in Latin America until much later.

including the demand and supply effects of the trade collapse. Second, the global collapse in trade during the Great Recession was temporary, and largely recovered by 2011. In Brazil, the value of exports reached its pre-Great Recession peak by the end of 2010. However, BRL-invoicing continued to rise beyond this recovery. Finally, the Global Recession had muted effects on countries in Latin America.

Second, there is the effect of foreign currency credit shocks for firms that may disproportionately rely on foreign currency. Firms may have taken up the SML system may have switched their financing decision. This change in financing decision may drive any subsequent results, and the direct effects of the SML system may be small. The Global Financial Crisis distorted credit markets which had large a large effect on exports, as described for example by Amiti and Weinstein (2011). If USD-invoicing firms were to borrow in USD, and BRL-invoicing firms were to borrow in BRL, then any disproportionate tightening of USD credit would affect trade values similarly.[39] While most Brazilian non-financial corporate firms do not directly borrow in USD for capital control reasons, offshore bond issuance or turmoil in the financial sector as a result of distress in global credit markets could be problematic for identification. Specifically, firms may have chosen to switch away from USD borrowing as a result of the Global Financial Crisis, invoice in BRL. In robustness checks leveraging cross-destination exports, I show that including firm-time fixed effects that control for firm-financing do not change the results.

Results

I begin in the top panel of Table 2.6, which presents results including only firm-HS4 and HS2-time fixed effects. Column (1) of this top panel reports results from estimating Equation (2.4) using the log value of exports as the outcome for *only* the set of export transactions to Argentina. In column (1), the estimate of 0.273 (SE: 0.077) for the coefficient on ι_{ijst} implies that switching to a BRL invoiced shipments results in trade volumes being higher by approximately 27%.

Column (2) repeats this estimation using the log price as the outcome instead of log value. If the SML system worked only to eliminate transaction costs, then if there is some pass-through of costs into prices these transaction costs savings should be reflected in a lower relative price. I instead find that prices do not move much in response to changing invoice currency, with an insignificant point estimate of -0.007 (SE: 0.025). This result is consistent with models of optimal currency choice, such as Gopinath et al. (2010), who argue that in the presence of nominal rigidities, optimal price setting is not a function of currency choice.

Columns (3)-(4) show the results for estimating the full specification of Equation (2.4), including all South American export destinations and including destination-time fixed effects. First, note that the coefficient estimate for the direct effect of ι_{ijst}, β, is -0.035 (SE: 0.089). This is small and statistically insignificant, suggesting that there is not a noticeable change

[39]In practice, firms do not directly borrow in USD in Brazil due to capital controls. Instead, firms borrow at the *cupom cambrial*, which is an interest rate pegged to offshore USD rates. Chamon and Garcia (2016) summarize these synthetic USD markets.

Table 2.6: Shipment-Level Effects of BRL Invoicing on Values

	$\ln V_{ijst}$ (1)	$\ln V_{ijst}$ (2)	$\ln V_{ijst}$ (3)	$\ln V_{ijst}$ (4)	ι_{ijst} (5)	$\ln V_{ijst}$ (6)
ι_{ijst}	0.273***	0.476***	-0.035	-0.128		
	(0.077)	(0.131)	(0.089)	(0.112)		
$\iota_{ijst} \times ARG$			0.437***	0.393***		0.318
			(0.118)	(0.128)		(1.255)
$Z_{m(i)jt}$					0.052***	
					(0.017)	
Firm-HS4 FE	Y	Y	Y	Y	Y	Y
HS2-Time FE	Y	Y	Y	Y	Y	Y
Firm-Time FE		Y		Y	Y	Y
Dest-Time FE				Y	Y	Y
Firm-Dest FE				Y	Y	Y
Sample	Arg	Arg	SA	SA	SA	SA
Obs	364,705	292,729	1,934,081	1,748,786	1,825,422	1,825,422
Adj. $\mathcal{R}^2$	0.841	0.856	0.746	0.792		
First-Stage F					20.8	

The first two columns of the table reports regressions of the form $y_{ijst} = \alpha + \beta\iota_{ijst} + \mathbf{X}_{ijst}\Gamma + \varepsilon_{ijst}$, where y_{ijst} represents the log value of exports (in USD) for establishment i in sector s to Argentina at time t. ι_{ijst} is a dummy variable equal to 1 if at least 10% of exports in sector s by firm i to destination j are invoiced in BRL, and ARG_j is a dummy variable equal to 1 if the destination is Argentina. The second columns of the table reports regressions of the form $y_{ijst} = \alpha + \beta\iota_{ijst} + \gamma\left(\iota_{ijst} \times ARG_j\right) + \mathbf{X}_{ijst}\Gamma + \varepsilon_{ijst}$, where y_{ijst} represents the log value of exports (in USD) for establishment i in sector s to destination j at time t. The last two columns reflect the IV specification of $y_{ijst} = \alpha + \gamma\left(\iota_{ijst} \times ARG_j\right) + \mathbf{X}_{ijst}\Gamma + \varepsilon_{ijst}$. The difference in observations across specifications reflects the dropping of singleton observations. **Source**: SECEX, Brazilian Central Bank

$*\ p < 0.1$; $**\ p < 0.05$; $*{*}*\ p < 0.01$. Standard errors clustered at the sector-establishment level.

in export behavior when a specific firm-sector transaction has different invoicing behavior across locations, conditional on time-varying sector and destination effects. The coefficient estimate of 0.437 (SE: 0.118) for γ suggests that firms raise export volumes in response to the SML system by 44%.

The reason for such large differences between the estimate on the Argentina sample vs. the South American sample can be seen in Panel B. In Panel B, I include firm-time and firm-destination fixed effects, although the changes in coefficients are mainly due to the inclusion of firm-time fixed effects. To the extent that the selection effect of opting into the SML system is due to unobserved firm characteristics that vary only over time, but not across sector, then the inclusion of firm-time fixed effects should control for this endogenous selection effect. I find that this specification significantly raises the effect when conditioning only on exports to Argentina, but the effect when including all of South America remains

	$\ln P_{ijst}$ (1)	$\ln P_{ijst}$ (2)	$\ln P_{ijst}$ (3)	$\ln P_{ijst}$ (4)
ι_{ijst}	-0.007	-0.035	0.007	-0.012
	(0.025)	(0.046)	(0.089)	(0.047)
$\iota_{ijst} \times ARG$			-0.034	-0.017
			(0.035)	(0.053))
$Z_{m(i)jt}$				
Firm-HS4 FE	Y	Y	Y	Y
HS2-Time FE	Y	Y	Y	Y
Firm-Time FE		Y		Y
Dest-Time FE				Y
Firm-Dest FE				Y
First-Stage F				
Sample	Arg	Arg	SA	SA
Obs	350,321	279,361	1,851,077	1,748,786
Adj. $\mathcal{R}^2$	0.856	0.871	0.792	0.861

The first two columns of the table reports regressions of the form $y_{ijst} = \alpha + \beta\iota_{ijst} + \mathbf{X}_{ijst}\Gamma + \varepsilon_{ijst}$, where y_{ijst} represents the log price (in USD) for establishment i in sector s to Argentina at time t. ι_{ijst} is a dummy variable equal to 1 if at least 10% of exports in sector s by firm i to destination j are invoiced in BRL, and ARG_j is a dummy variable equal to 1 if the destination is Argentina. The last two columns of the table reports regressions of the form $y_{ijst} = \alpha + \beta\iota_{ijst} + \gamma\left(\iota_{ijst} \times ARG_j\right) + \mathbf{X}_{ijst}\Gamma + \varepsilon_{ijst}$, where y_{ijst} represents the log price (in USD) for establishment i in sector s to destination j at time t. The difference in observations across specifications reflects the dropping of singleton observations. **Source**: SECEX, Brazilian Central Bank

$* \ p < 0.1$; $** \ p < 0.05$; $*** \ p < 0.01$. Standard errors clustered at the sector-establishment level.

similar, albeit slightly attenuated. Results for prices are relatively unchanged.

Including these fixed effects also naturally allows an instrumental variables specification. I use as an instrument the time-varying presence of the importance of SML institutions within the municipality of the firm, as described in Section (2.4).

$$Z_{m(i)jt} = POST_t \times \widetilde{SML\_Share}_m \times ARG_j$$

Results are displayed in columns (5) and (6) of the bottom panel of Table (2.6). Column (5) shows results of the second state. The first stage is positive and significant. However, the value for the F-test is just below the standard threshold of ten, suggesting that there may be issues customary of weak instruments. Still, the results of the second stage, shown in column (6), shows a point estimate of 0.318 (SE: 1.542). The large standard errors are the result of a weak reduced form. Regressing the log value of exports on the instrument (and including all fixed effects) results in an estimate of 0.017 (SE: 0.081). While the second stage coefficient is insignificant, the point estimate suggests that the selection effect of the

SML system is that growing *firm-sectors* are likely to opt into the program, consistent with intuition. This selection effect is likely to be small, with the caveat that the instrumental variable analysis has high uncertainty.

Finally, I also perform a series of heterogeneity analyses. Specifically, I interact the invoice dummy ι_{ijst} with a number of firm observable characteristics. First, I study the difference in volume effects between commodity and non-commodity exporters, potentially due to being in a less competitive differentiated goods sector. Second, I study whether having a larger share of exports to Argentina affects the benefits of the SML system by calculating the share of exports by firms to Argentina in the six quarters prior to the introduction of the SML system. Finally, I look at heterogeneity along firm size by comparing firms above and below the median total exports in the six quarters prior to the introduction of the SML system.[40]

Results of the heterogeneity analysis are in Table (2.7).[41] In columns (1) and (2), I show that non-commodity exporters that predominantly export to Argentina are the main beneficiaries of the SML system, suggesting that models of imperfect competition (common in differentiated goods sectors) that focus on frictions that rely on only one destination are likely to capture the main benefits of the system. In the last column, I show that both small and large firms benefit from the the SML system.

The evidence within this section suggests that import demand is a function of currency choice. It points to exchange rate risk being an important determinant for import demand. Because prices do not change in response to the invoicing change, this suggests that neither marginal costs nor optimal markups have changed substantially. Still it is useful to examine a specification that includes firm-time fixed effects. These fixed effects not only control for changes in marginal costs at the firm level, but also control for other determinants of policy takeup such as import intensity and foreign currency borrowing.

The Effects of BRL Invoicing on Firm Size

The transaction-level analysis does not capture whether or not firms respond to the SML system by changing the distribution of their sales across countries or by growing overall. If total sales by firms are relatively unchanged but the volume of shipments to Argentina rise, it suggests either low returns to scale in production or high elasticities of substitution across export destinations.

I investigate how the SML system influenced firms' export behavior in terms of their reliance on Argentina as an export destination and in terms of their total size as measured by total exports. Collapsing across destinations, an empirical specification at the firm-sector-time level can estimate how overall sales and exposure to Argentina evolve following the introduction of the SML system. This specification takes a similar form as Equation

[40]For these last two analyses, I drop firms that do not export in the six quarters prior to the introduction of the SML system.

[41]Results including firm-time fixed effects are qualitatively similar. However, when including firm-time fixed effects the effect of being large is bigger and marginally significant. This is likely due to the fact that smaller firms do not have much variation across destination or sectors, so I elect not to report them.

Table 2.7: Heterogeneity Analysis of the Effects of BRL Invoicing

	$\ln V_{ijst}$ (1)	$\ln V_{ijst}$ (2)	$\ln V_{ijst}$ (3)
ι_{ijst}	-0.047*	-0.035	-0.066**
	(0.025)	(0.023)	(0.030)
$\iota_{ijst} \times ARG$	0.048	-0.008	0.258
	(0.176)	(0.083)	(0.190)
$\iota_{ijst} \times ARG \times NonComm$	0.539***		
	(0.094)		
$\iota_{ijst} \times ARG \times ArgShare$		0.454**	
		(0.181)	
$\iota_{ijst} \times ARG \times Large$			0.089
			(0.196)
Firm-HS4 FE	Y	Y	Y
HS2-Time FE	Y	Y	Y
Dest-Time FE	Y	Y	Y
Sample	SA	SA	SA
Obs	1,934,081	1,793,286	1,539,533
$\mathcal{R}^2$	0.744	0.746	0.741

Each column of the table reports regressions of the form $y_{ijst} = \alpha + \beta\iota_{ijst} + \gamma(\iota_{ijst} \times ARG_j) + +\delta(\iota_{ijst} \times ARG_j \times H_{ijst}) + \mathbf{X}_{ijst}\Gamma + \varepsilon_{ijst}$, where y_{ijst} represents the log value of exports (in USD) for establishment i in sector s to destination j at time t, ι_{ijst} is a dummy variable equal to 1 if at least 10% of exports in sector s by firm i to destination j are invoiced in BRL, and ARG_j is a dummy variable equal to 1 if the destination is Argentina, and H_{ijst} represents some level of heterogeneity, either sector, Argentina export share, or size. The difference in observations across specifications reflects the dropping of singleton observations and whether data exists for the heterogeneity analysis.

$*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$. Standard errors clustered at the sector-establishment level.

(2.4)

$$y_{ist} = \alpha + \beta\iota_{ist} + \mathbf{X}_{ist}\Gamma + \varepsilon_{ist} \tag{2.5}$$

where the outcome variable is now either total log value of exports in sector s by firm i at time t to *any* destination, denoted $\ln V_{ist}$, or the share of total exports in sector s by firm i to Argentina, denoted Arg_Share_{ist}. ι_{ist} is a dummy variable equal to one if for any destination j, $\iota_{isjt} = 1$, and ι_{ist}^{ARG} is a dummy equal to one if exports specifically to Argentina are invoiced in BRL. $\mathbf{X}_{ist}$ denotes controls that possibly vary at the firm-sector-time level. I include firm-HS4 and HS2-time fixed effects.

The main coefficient of interest, β, estimates the relative effect any BRL invoicing by the firm has on the total value and Argentine share of exports. As in Equation (2.4), the identifying assumption is that firm-sectors that have any BRL-invoiced shipments would have grown at the same rate as those that did not have any switch. Under this assumption, the treatment effect identified by β is the causal effect on *total* sales from *at least some* BRL invoicing.

Under the assumption that the coefficient β is causal when estimating at the shipment level in Equation (2.4), the results of estimating at the firm level provide evidence simply about the possible reallocation of sales across export destinations. Causality as a result of BRL invoicing due to the SML system follows directly from those firms that switch to BRL invoicing in shipments to Argentina. However, it is not necessarily true that *all* BRL-invoicing can be considered plausibly randomly assigned. In fact, it is almost surely not. For example, Bolivia and Paraguay have non-trivial shares of exports invoiced in BRL almost exclusively by smaller firms. To ensure that the results are not driven by non-Argentina BRL-invoiced exports, I also decompose ι_{ist} into an indicator for only Argentina and another for other export destinations:

$$y_{ist} = \alpha + \beta_1 \iota_{ist}^{ARG} + \beta_2 \iota_{ist}^{OTH} + \mathbf{X}_{ist}\Gamma\varepsilon_{ist} \tag{2.6}$$

where ι_{ist}^{ARG} is an indicator equal to one if $\iota_{isjt} = 1$ for $j = ARG$, while ι_{ist}^{OTH} is an indicator equal to one if $\iota_{isjt} = 1$ for any $j \neq ARG$. The coefficient of interest in this equation is β_1. So long as the effect we find in Equation (2.5) is due to the SML system, then β_1 should have a similar magnitude as β. I include the same fixed effect controls as in Equation (2.5).

Columns (1) of Table (2.8) reports the results of estimating Equation (2.5) using the share of exports to Argentina as an outcome variable. The switch to BRL invoicing by firms results in a statistically significant increase of around 7 percentage points. This suggests that firms that switch to BRL invoicing do so when tilting their sales towards Argentina. Column (2) estimates Equation (2.6) and decomposes the effect into that coming from invoicing in BRL to Argentina and that of elsewhere. Splitting in this way shows that all of the effect on Argentina export shares comes from BRL invoicing to Argentina.

Column (3) changes the outcome variable to log total value of export shipments. The point estimate of 0.211 suggests that total exports rise by approximately 22% for firms switch to BRL invoicing. This point estimate alone suggests that firms do some substitution across destinations, as it is less than the point estimate in Table (2.6). However, Column (4), which decomposes into the Argentine and non-Argentine invoicing dummies, finds again that the whole effect is due to the SML system, which leads to exports being larger by 54%.

Extensive Margin Effects

I explore whether or not the introduction of the SML System had an effect along the extensive margin. To do so, I collapse the data across firm-sectors by taking the average value of ι_{ijst}, and regressing both entry and the number of firms on this variable.

$$N_{sjt} = \alpha_{sj} + \alpha_{st} + \alpha_{jt} + \beta\bar{\iota}_{sjt} + \gamma\left(\bar{\iota}_{sjt} \times ARG_j\right) + \varepsilon_{sjt}$$

In Column (1), we see that industries with a high BRL share have a larger number of firms and a larger number of entrants. This is broadly consistent with the idea that smaller

Table 2.8: Effect of BRL Invoicing on Firm Export Behavior

	Arg_Share_{ist}		$\ln V_{ist}$	
	(1)	(2)	(3)	(4)
ι_{ist}	0.071***		0.211***	
	(0.010)		(0.080)	
ι_{ist}^{ARG}		0.222***		0.541***
		(0.014)		(0.062)
ι_{ist}^{OTH}		-0.003*		0.052
		(0.002)		(0.093)
Firm-Time FE	Y	Y	Y	Y
Firm-HS4 FE	Y	Y	Y	Y
HS2-Time FE	Y	Y	Y	Y
Obs	1,714,524	1,714,524	1,714,524	1,714,524
$\mathcal{R}^2$	0.639	0.638	0.825	0.825

This table reports regressions of the form $y_{ist} = \alpha + \beta\iota_{ist} + \varepsilon_{ist}$, where y_{ist} represents *total* exports in sector s by firm i at time t across all destinations, and ι_{ist} is equal to one if any ι_{ijst} for firm i in sector s at time t is equal to one. **Source**: SECEX

$*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$. Standard errors clustered at the establishment level.

firms disproportionately invoice in BRL. In Column (2), we see that industries exporting to Argentina with high BRL shares have a lower number of firms and entrants. This does not mean that the SML system raises entry costs. This is because the total effect of the SML system is given by the sum of $\beta + \gamma$.

That I do not find strong effects of foreign currency usage is in line with other work studying the effects of credit access on international trade. For example, Paravisini et al. (2015) find a strong effect of a credit crunch in Peru on the intensive margin of exports, but do not find any evidence of an extensive margin effect.

2.6 Discussion

The results thus far suggest that the usage of foreign currency acts a variable cost along the intensive margin of trade. In this section, I discuss both the magnitude of the estimated effects as well as the potential reasons for foreign currency to act as a barrier to trade. Motivated by the results that the effect of the program is largest for firms in non-commodity sectors with a significant share of exports to Argentina, I first outline a stylized import/export model featuring a monopolistically competitive importing and exporting firms exposed to frictions. I then use the model to back out the implied reduction in frictions using the empirical estimates and discuss the sources of these frictions. Finally, I relate my results to the literature on endogenous invoicing decisions.

Simple Import/Export Model

Workhorse models that feature a role for invoicing currency typically assume some sort of nominal rigidity on output prices. [42] These models are able to successfully capture many important features of currency choice and exchange rate pass through. For example, they are able to match the fact that import prices (in local currency) are not sensitive to bilateral exchange rate movements when invoiced in the dollar or that the terms of trade are not very sensitive to exchange rate movements Boz et al. (2018), or that firms with high market share have low exchange rate pass-through Amiti et al. (2014).

At the same time, these models struggle to explain why switching invoice and payment currency through the SML system would have such a large effect on export volumes. This is because, in those models, import demand is only a function of changes in the relative price. Importers observe the exchange-rate adjusted realized local price when making purchasing decisions. Due to price stickiness, fluctuations in the market exchange rate of the local currency vis-a-vis the invoice currency causes relative price changes, which then result in changes in quantity demanded. Switching invoicing currency changes only which exchange rate import prices are sensitive to.

Similarly, while the exporter's invoicing decision is certainly a function of exchange rate volatility (see Engel (2006) or Mukhin (2018)), the optimally chosen price is not a function of the invoicing currency. As in most models of price-setting, the optimally chosen price is set to be a markup over marginal cost. While movements in the exchange rate may temporarily change profits via marginal costs (if intermediate input prices change as in Boz et al. (2018)) or markups (due to strategic complementarities in pricing as in Atkeson and Burstein (2008)), the optimal price is typically unaffected.

That the introduction of the SML system results in higher export volumes without a decline in the relative price suggests some effect on both supply and demand. The heterogeneity analysis also shows that only firms in non-commodity sectors, typically firms with some form of price setting ability facing downward sloping demand curves, captured the benefits of the SML system. It is therefore unlikely that either import demand or export supply curves are perfectly elastic, and so the small effect on price and large effect on volume must shift both curves outwards.

Consider the following stylized model. There exists an exporter, who produces and sells some good to an importer. Assume that the importer's demand for the exporter's good, Q^D, takes the following CES form

$$Q^D = \left(S_\ell^M (1 + \tau_{M\ell}) P^\ell\right)^{-\rho} \mathbf{X} \tag{2.7}$$

Here, the price faced by the importer in invoice currency ℓ is given by $(1 + \tau_{M\ell})P^\ell$, where P^ℓ is the price posted by the exporter in currency ℓ and $\tau_{M\ell}$ is some friction faced by the

[42] The literature on *endogenous* invoicing decisions further assume that exporting firms choose the invoicing currency that maximizes expected profits. Under some assumptions, Mukhin (2018) shows that the choice of invoicing currency reduces to minimizing the variation of the import price around its flexible price benchmark. For example, strategic complementarities in pricing and input-output linkages across firms may cause firms to invoice in home, destination, or vehicle currencies. (Engel, 2006; Gopinath et al., 2010)

importer. S_ℓ^M is the importer's currency price of currency ℓ, such that an increase in S_ℓ^M is a depreciation of the importer's currency. ρ is the elasticity of substitution. Finally, $\mathbf{X}$ is a constant that includes, for example, the relevant price index or total income.

The exporter uses only one input, labor l, with cost W in production according to the technology $Q = l^\alpha$ where $0 < \alpha < 1$. Costs for the exporter are therefore given by $C(Q) = WQ^{\frac{1}{\alpha}}$. The exporter has some market power and chooses the price P^ℓ in the (exogenously given) currency of invoicing ℓ. The exporter takes the importer's demand as given and sets prices by maximizing profits.

$$P^\ell = \arg\max_P \frac{S_\ell^X P}{(1+\tau_{X\ell})} Q - C(Q)$$
$$\text{s.t. } Q = \left(S_\ell^M (1+\tau_{M\ell}) P\right)^{-\rho} \mathbf{X}$$

where S_ℓ^X is the exporter's currency price of currency ℓ, such that an increase in S_ℓ^X is a depreciation of the exporter's currency. $\tau_{X\ell}$ denotes some friction faced by the exporter, such that the amount received per unit is only $P^\ell/(1+\tau_X)$.

Solving for the optimal posted price P^ℓ, then plugging in for demand gives the equilibrium price in currency ℓ as[43]

$$P^\ell = \left[\frac{(1+\tau_{X\ell})/S_X^\ell}{(S_M(1+\tau_{M\ell}))^{\rho\frac{1-\alpha}{\alpha}}} \frac{\rho}{\rho-1} \frac{W}{\alpha} \mathbf{X}^{\frac{1-\alpha}{\alpha}} \right]^{\frac{1}{1+\rho\frac{1-\alpha}{\alpha}}} \tag{2.8}$$

Equilibrium exports are given by

$$Q = \left((1+\tau_{M\ell}) S_M^\ell (1+\tau_{X\ell})/S_X^\ell\right)^{\frac{-\rho}{1+\tilde{\rho}}} \Gamma_2 \tag{2.9}$$

To the extent that the SML system reduced *both* τ_X and τ_M, then we would expect to see P^ℓ stay stable.[44] Intuitively, the reduction of τ_M shifts out the importer's demand curve and the reduction of τ_X shifts out the exporter's supply curve, resulting in higher export volumes.[45]

Implied Reduction in Trade Barriers

Log-differencing Equation (2.9) (and holding constant the non-friction and non-quantity terms) gives the following expression for the effect of reducing frictions on quantity

$$\Delta \ln Q \approx \left(\frac{-\rho}{1+\rho\frac{1-\alpha}{\alpha}} \right) \left(\underbrace{\Delta\tau_{X\ell} + \Delta\tau_{M\ell}}_{\Delta\tau} \right)$$

[43]See Appendix A.3 for a full derivation.

[44]In my data, I only observe unit values rather than posted prices. I assume that the total value of goods is inclusive of these frictions.

[45]Note that under this formulation, changes in τ_M affect quantity exported while changes in τ_X affects (real) revenues. To the extent that I observe (real) revenues, rather than quantity purchased, then both of these effects would be active.

Using this equation, one could back out the implied reduction in total frictions $\Delta\tau$ required to generate the observed change in quantity. One only needs estimates of ρ and α to do so.

The main issue with relating the empirical estimates in this paper to the implied reduction in trade barriers is that two levels of aggregation were used. Column (1) of Table 2.4 shows that, at the HS2-Municipality level, the increase in trade was approximately 20%. Column (3) of Panel A of Table 2.8 shows that, at the HS4-Firm level, the increase in trade was approximately 40%. In particular, the parameter ρ, which represents the demand elasticity, depends crucially on the level of aggregation.

First, when aggregating across firms, ρ is effectively the frequently studied trade elasticity, whose structural interpretation is related to the specific underlying trade model. At the aggregate level, trade elasticity estimates are typically found to be between 4 and 8. (Eaton & Kortum, 2002; Head & Mayer, 2014; Simonovska & Waugh, 2014) At the firm-level, the elasticity again depends on the structural interpretation (e.g., in Melitz (2003)/Chaney (2008) models of trade, the intensive margin elasticity is given by $1 - \rho$.)[46,47] Fitzgerald and Haller (2018) or Fontagné et al. (2018), using firm-level data, find the elasticity using tariffs is approximately 2, which implies a similar aggregate elasticity. I set the benchmark specification to $\rho_{MUN} = 4.0$ and $\rho_{FIRM} = 2.5$.

The parameter α represents diminishing marginal returns in variable inputs to production.[48] A voluminous literature has studied diminishing marginal returns along a number of inputs. Loecker et al. (2016) find returns to scale across exporters in India, another emerging market, to be on the order of 0.8-1.0. I set the benchmark value at the midpoint of this range, $\alpha_{MUN} = \alpha_{FIRM} = 0.9$.

Row (1) of Table 2.9 shows estimates of the implied reduction in total trade costs for both the municipal and firm level. The estimated effect of the program is that trade costs fell by between 7.22 and 12.44 percentage points, depending on the level of aggregation. That the size of the frictions at the firm level is larger than the size of the frictions at the municipality level is due to two reasons: differences in the value of ρ and differences in the estimated effect, with both approximately responsible for half of the difference. To see this, in row (2), I set $\rho = 3.5$ for both levels of aggregation (lowering the firm-level effect), and in row (3) I set the change in export volumes to be the value at the municipality level. In row (4), I lower the estimate of α to 0.85 and show results rise slightly.

The evidence in this subsection is meant to be illustrative of the approximate effect of the reduction in tariffs. As such, I make two comments regarding the interpretation. First, when assuming a zero change in prices, backing out the implied effects on export and import tariffs individually suggests that the vast majority of currency-related trade barrier effects fall on the importer. It is not obvious why this should be the case, given that a key benefit

[46]The estimates in this paper correspond to an intensive margin effect.

[47]This elasticity is also related to the gains from variety parameter. Broda and Weinstein (2006), using sector-destination export data, estimate the median value of this parameter to be between 2-3.

[48]This parameter is closely related to the returns to scale parameter studied by, for example, Basu and Fernald (1997). However, returns to scale may come from the presence of fixed costs in production, so the comparison is not direct.

Table 2.9: Implied Reduction in Frictions

Specification	$\Delta\tau_{MUN}$	$\Delta\tau_{FIRM}$
Baseline	-6.02% (2.72%)	-17.79% (5.79%)
Identical ρ	-6.02% (2.72%)	-11.89% (3.87%)
Municipality Effect	-6.02% (2.72%)	-14.44% (5.79%)
Low α	-8.49% (3.84%)	-22.66% (7.48%)

Implied change in τ applying regression estimates in Column (1) of Table 2.4 (for municipality result) and Column (3) of Panel B of Table 2.6 (for firm results, except for row 3). Baseline specification sets $\alpha_{MUN} = \alpha_{FIRM} = 0.95$, $\rho_{MUN} = 4.0$, and $\rho_{FIRM} = 2.5$. Identical ρ sets $\rho_{MUN} = \rho_{FIRM} = 4.0$. Low α sets $\alpha_{MUN} = \alpha_{FIRM} = 0.85$. Standard errors calculated applying equation (2.9) to standard errors from Column (1) in Table 2.4 and Column (3) of Panel B of Table 2.6, and so do not reflect parameter uncertainty.

of the SML System for the exporter was the elimination of risk. Second, the estimates of the reduction in trade barriers, especially at the firm level, should be interpreted as large, but not unreasonably so. The Uruguay Round of tariff negotiations in 1993 resulted in a significant decline in tariffs, in some cases more than 50 percentage points. Buono and Lalanne (2012)

Discussion of Frictions

Risk aversion: Risk-averse exporting firms may charge higher prices based on the variability of profits. In Appendix A.3, I show that under the formulation of risk aversion as in Mann (1989), that if firms do not have access to hedging markets, then higher levels of risk aversion result in higher prices. Intuitively, exporters must be compensated for bearing some risk in international trade as it pertains to realized revenues.[49] In the context of the SML system, risk aversion is likely to only affect the exporter, as the importer is still exposed to movements in the SML exchange rate. Recall that the SML system is a payments system. Payments through the SML system are therefore still subject to exchange rate risk vis-a-vis changes in the posted SML rate. Thus, in my context, risk aversion only should affect τ_X.[50,51]

Imperfect competition in exchange rate markets: There may also exist transaction costs in the foreign exchange market if financial intermediaries charge firm additional fees relative to global currency markets. The SML rate, compiled based on underlying dollar

[49] One famous result is the separation theorem as proposed by Ethier (1973), which argues that with hedging the spot exchange rate volatility does not affect optimal pricing. Other work by Feenstra and Kendall (1997), Friberg (1998), or Lyonnet et al. (2016), which also feature hedging, also find some form of the separation theorem.

[50] The SML rate and the bilateral market rate as reported by Interactive Data Corporation (ICE) are almost perfectly correlated.

[51] Still, there may be some benefits of reduced risk for importers. To the extent that movements in exchange rates are due to changes in the value of the dollar, then bilateral exchange rates could be more stable. To the extent that firms would benefit by hedging against dollar-specific movements, then the SML could offer some protection. However, it is difficult to think this explanation alone would explain the empirical results in this paper, as importers could have already benefitted from this channel by simply paying in Reals.

reference rates, is updated daily and so reflects market changes in exchange rates. It is important to emphasize that many are not able to make exchange rate transactions at this market rate, and so the SML system can potentially lower costs of foreign exchange transactions by permitting firms to use the market rate. In Appendix A.3, I outline a stylized model of the foreign exchange market, whereby market power allows financial intermediaries to apply an exchange rate that is a markup over the exchange rate faced in global markets, thereby affecting τ_M, although in practice, this could also potentially affect τ_X. Such monopoly power by financial intermediaries may come from legal restrictions, local knowledge of foreign exchange markets, or direct access to central bank liquidity. The SML system, which would direct apply the SML rate, could alleviate this market power by allowing firms to directly trade at the global exchange rate.

Unhedged FX borrowing: Time delays between when importing firms purchase goods (either for domestic retail sales or as an intermediate input) and when sales are realized may affect the real value of loan repayment due to realized movements in foreign currency. More concretely, firms may borrow in foreign currency to buy foreign currency imports in the form of trade credit or trade finance. When firms then repay the foreign currency loan, realized movements in the exchange rate may directly affect profits.[52] In Appendix A.3, I outline a stylized model of foreign currency working capital loans showing how expected depreciations and borrowing rates can affect demand for imported intermediate inputs by producers, motivating τ_M as a friction related to intraperiod borrowing. The SML system works by removing the effect of such exchange rate movements, as the producer may now borrow the required amount in local currency. Note that this time difference is distinct from local currency changes in prices between when the invoiced price is posted and when the realized price is determined by exchange rate movements.

Relation to Endogenous Invoicing Literature

My paper provides empirical support for theories of currency usage that rely on first-order determinants. Broadly speaking, there are three strands of thinking regarding endogenous invoicing in international trade. These models feature network externalities in foreign currency markets, financial determinants related to a currency's role as a unit of account, and nominal rigidities. I discuss each in turn.

First, consider the models of Krugman (1980) or Rey (2001). These models features network externalities in foreign currency markets, whereby foreign currency transactions have increasing returns to scale. This lowers the cost of international payments when one currency is used more frequently due to real costs of transaction services affecting goods prices. The SML System effectively bypasses such transaction costs by "introducing" a fourth currency option. This currency option avoids the usage of potentially costly foreign

[52] This channel is similar to Barbiero (2020), who studies how the currency mismatch of importers and exporters may result in capital gains for manufacturing firms due to movements in the exchange rate.

currency markets. Unlike these models, however, I do not find evidence of price changes, suggesting that importer transaction costs may also be important.

Second, consider the models of Doepke and Schneider (2017) or Drenik et al. (2019). These models features a contracting problem between buyers and sellers over the quantity sold and currency of denomination for a sale. They show that the optimal contract uses the currency with the largest marginal benefit, which incorporates price risk and a premium denoted by the covariance of preferences with prices. In the setting studied in this paper, using the SML System allows for the exporter to eliminate price risk coming from foreign currency movements. However, the SML System does not significantly change importer risk. The only way price risk would be reduced, then, is if this bilateral exchange rate experienced less volatility than the dollar exchange rate, which is unlikely to be the case given the volatility of emerging market currencies. Additionally, monetary policy is unlikely to be significantly altered, so through the lens of the model of Drenik et al. (2019) the covariance term should not be substantially changed.

Finally, there are models that emphasize nominal frictions in price setting, arguing that the pass-through of exchange rates to optimal prices (via e.g. strategic complementarities or imported intermediate inputs) determines the optimal currency choice. (Engel, 2006; Gopinath et al., 2010; Mukhin, 2018) One assumption embedded in the models that incorporate nominal frictions is that without nominal frictions, currency choice is irrelevant. That I find a first-order effect of changing currencies after a year, when prices have typically adjusted, suggests instead that currency choice has first-order effects. This instead is in support of the theories suggesting transaction costs in foreign exchange markets.

2.7 Conclusion

There are at least two important questions left for future research. First, although this paper argues that the SML system worked by increasing import demand due to importer risk, the role of risk by exporters may also be an important factor in understanding how foreign currency risk affects international trade. Research areas such as Lyonnet et al. (2016) or Goldberg and Tille (2008), which focus on how firms manage exchange rate risk via financial derivatives or bargaining, may help explain why emerging markets have relatively lower levels of international trade. Such an issue seems especially important in a world where the dollar plays such an important role in international trade.

Second, understanding how local currency payments systems operate more generally may give clues to why they may be effective. Brazil has since entered into agreements with Uruguay and Paraguay. In addition, a number of countries in Southeast Asia, such as Indonesia, Malaysia, and Thailand have also begun to implement local currency payment systems. Argentina did not take up the SML system at the same scale as Brazil. The reason is likely to be unique to Argentina, as statistics reported by the Central Bank of Brazil note a similar take-up by both Uruguayan and Brazilian exporters as a result of the SML system introduced in 2015. It is likely that the reliance on the dollar both due to historical instability of Argentine monetary policy and as a financing currency contribute to its lack of usage. The link between exchange rate volatility in goods trade and monetary policy, as studied for example in Drenik et al. (2019), is likely to be a fruitful area of research.

Chapter 3

Financial Failures and Depositor Quality: Evidence from Building and Loan Associations

3.1 Introduction

Bank runs, or the failure of individual financial institutions, can have detrimental effects on the real economy. During the financial crisis of 2007-2009, the failure of many high-profile financial institutions contributed to the largest decline in economic output since the Great Depression. Conventional explanations for why financial institutions fail focus on either liquidity management (e.g. Diamond and Dybvig, 1983, Richardson and Troost, 2009) or solvency and asset management (e.g. Calomiris and Kahn, 1991, Calomiris and Mason, 1997). The idea is that the failure comes from the asset side of the balance sheet, either through bad fundamentals, or from mismatching asset and liability liquidity.

Less attention, however, has been given to the idea that the quality of liabilities may be an important determinant of financial failure. To attract counterparties, financial institutions can offer different terms when they borrow. In the context of banking institutions, deposit contracts may differ structurally to attract depositors that are ex-ante more susceptible to liquidity shocks, i.e. more "flighty." For example, institutions may differ in terms of withdrawal penalties and flighty counterparties may choose the low penalty institutions. During a crisis, flighty counterparties may be more likely to experience liquidity shocks and initiate a run on the financial institution. What then looks like an unexpected liquidity shock is actually a function of the institution's predetermined structure; the potential for liquidity shocks at institutions with flightier deposits is significantly larger.

In this paper, I study the role of flightiness using Building and Loan Associations in California during the Great Depression. Building and Loan Associations (B&Ls) were lending institutions that specialized in real estate loans, comprising about one-third of the institutional residential mortgage market at their peak. During the Great Depression, there were failures not only in the commercial banking sector, but there were also a large number of liquidations among B&Ls. I investigate the depositor role in bank failures by taking advantage of differences in organizational structures at B&Ls in California.

B&L's focus on residential mortgage lending meant that the asset structure was homogenous across institutions over the course of their history. B&L's originally started as temporary institutions where a few individuals or members would get together and pool funds. Potential borrowers would then bid on the available loanable funds, and the one willing to pay the highest "premium" would borrow in order to build a home. Lenders would continue saving at a regular rate, being required to put money in according to a certain schedule. Over time, borrowers would pay off their loans, and the association would be dissolved in an orderly and planned fashion.

Over time, B&L's innovated on the liability side of their balance sheet to attract new investors. The planned closure of the institution and the fact that new members had to back-pay earlier savings led to innovation within the industry. The structure of B&L's developed in the late 1800s and early 1900s towards more complex institutions. They first created plans that attracted long-term investors, known eventually as **permanent** plans. These plans no longer forced the B&L to close at a specific date. In addition, they allowed in new members who continued to be bound by forced savings plans and withdrawal penalties, but no longer needed to back-pay earlier intervals. Additional innovation eventually developed plans that did away with the forced saving plan. These new **Dayton** plans (named after the city in Ohio, where they originated), competed for short-term deposits in a similar fashion as commercial banks.[1]

The simultaneous existence of Dayton plans, which emphasized short-term liabilities, and permanent plans, which emphasized regular savings by investors, presents the key source of liability heterogeneity studied in this paper. Due to the gradual nature of B&L innovation, in some states there were periods of overlap in which there existed permanent plans that looked more like "traditional" permanent B&L's and newer Dayton B&L's that were closer in spirit to commercial banks. This paper studies the state of California during the 1920s, which was one of the periods with the most overlap. I leverage variation across these two types of financial institutions to understand how depositor characteristics, which differed across corporate structures, may have lead to different rates of failure during the Great Depression.

B&Ls in the Great Depression offers an exceptional laboratory to study financial distress due to the quality of depositors. First, the proliferation over the past few decades of different types of derivatives and investments has complicated both sides of financial balance sheets, making it difficult to disentangle the relative effects of specific liabilities. B&L's at this time, however, had a structure that was was much less complex. Second, it is difficult to find similarities in the asset side of balance sheet. Whether one looks historically or in the present day, the types of loans made by commercial banks vary based on sector (e.g.

[1] Members of a Building and Loan with a permanent plan (or earlier plans) purchased *equity* contracts, which is why I use the term investors rather than depositors to describe their status. However, as explained in this paper, Dayton plans also used investment certificates, which were more similar to deposit certificates. I also use the term investor to describe these members due to convention.

mortgage, commercial) or maturity. However, B&Ls in California has assets that were almost completely in real estate loans and, due to legal restrictions, very similar across institutions. If anything, the minor differences between the institutions would only bias against finding higher failure rates at Dayton B&L's, which had higher levels of cash as a share of assets. [2]

Previous work on bank failures has excluded flightiness as a potential reason for financial distress due to at least two main factors. First, in most cases financial institutions differ drastically in terms of their assets. The choice of which financial institution to bank at may be confounded by the institution's asset allocation. The risk profile of lending across financial institutions varies significantly. Unobserved determinants of lending may vary substantially across financial institutions, which may play a key role in determining probability of failure. B&L's focused on mortgage lending, meaning the asset side of balance sheets was relatively homogenous across institutions. Second, it is difficult to observe salient information regarding the characteristics of depositors or investors attracted by different institutions. This is because observed liability contracts are typically quite homogenous. B&L's of different types, however, offered investors contracts of different degrees of withdrawal penalties.

B&L investors were largely local individuals looking for reliable returns. During the early decades of the twentieth century, B&L's were seen as attractive places to save. They were perceived as safe financial institutions, lending only for the purposes of home construction. The number of B&L's grew drastically over this time period due in part to high demand for construction loans, but also increased demand for high-return savings. B&L's increased their share of total savings from around 10% to 15% during the 1920s. Bodfish, 1935 This growth in savings was accompanied by new types of investors in B&L's, many of whom viewed these institutions as close substitutes for commercial banks.

I begin by estimating a cross-sectional linear probability model to determine the effect on the probability of failure of being a Dayton plan. I find that of the two types of B&L's, Dayton and Permanent, Dayton plans were much more likely to fail. I find that the effect of being a Dayton plan raises the probability of failure by around 20 percentage points. This higher failure probability holds even when conditioning on conventional measures of failure such as size, liquidity, and geography.

There are two main concerns with the benchmark regression specification that I investigate in a series of robustness checks. First, the definition I use of Dayton plan is a self-reported measure assigned in 1927. Using instead the observed liability structure to define plan type implies quantitatively similar effects. Second, I investigate how including the age

[2] B&L's in this time period are also attractive to study because reverse causality is unlikely to be a major concern. That is, it is unlikely that distress in this sector caused the Great Depression in any meaningful sense. Field (2014) shows that the impacts on the housing market during the Great Depression was comparatively small relative to the Great Recession, but notes that while "[w]e have abundant historical evidence that commercial bank failures can pose a systemic threat to an economy, it is less clear that this would have been so with building and loans." Similarly, White (2014) finds little role for the housing market in the 1920s to impact the financial system at the time. While this paper abstracts from a discussion of aggregate activity, it does show that failures of B&L's, at least in California, were affected by the aggregate economy.

of the B&L affects the coefficient estimate. While including B&L age as a predictor of failure lowers the coefficient on Dayton plan, neither Dayton nor B&L age is significant when both are included simultaneously. I argue this is due to the nature of innovation; younger B&L's were by definition more likely to begin as Dayton plans.

I then investigate the mechanism by which this higher failure occurred and test the role of flightiness as a cause. I find that Dayton B&Ls were less likely to have withdrawal penalties and had lower dues on average. These low access costs were accompanied by low returns on saving relative to permanent plans. The difference across institutions in terms of returns is not driven by any differences in lending rates.

I also compare differences in terms of member characteristics. While Dayton B&L's were larger on average in terms of members and assets, members of permanent B&L's were wealthier as measured by total shares per member and total assets per member. Additionally, I find evidence that fees were heightened for Dayton B&L's during the Great Depression. These fees were primarily a function of withdrawal fees and late dues, providing evidence that Dayton B&L members had higher liquidity needs.

Finally, I examine the real effects of this channel. Real economic data at the local level is difficult to find over this period. I therefore examine changes in real farmland value from the Agricultural Census between 1925 and 1935. I find that counties that had a higher share of Dayton B&L's had larger declines in farmland values during the Great Depression.

It is important to emphasize that Building and Loan Associations did not fail in the conventional sense. While deposits at commercial banks were debt contracts, which banks were required to repay on demand, most building and loan associations issued *equity* contracts. Members of B&L's were therefore investors in the institution, with the value of their investment supposedly linked to the success of the B&Ls. California was no exception, with liquidation requiring the vote of two-thirds of total members. However, this paper is interested in the role of ex-ante differences in liquidity needs by depositors. I argue that the propensity to liquidate was not different across the institutions due to the fact that the share of borrowing members was similar.

Related Literature

This paper falls at the intersection of three literatures. First, there is a literature using the Great Depression to try to understand why banks fail. Calomiris and Mason (1997) study whether commercial bank failures in Chicago and argue that failures were due to asset declines rather than depositor sentiment. Postel-Vinay (2016) studies at commercial bank failures in the Great Depression and finds that bank balance sheets matter, as those banks with a higher portion of Real Estate loans were associated with a higher rate of failure. Along a different margin, Richardson and Troost (2009) look at monetary policy and show that looser monetary policy can help bank survival during financial panics. Closely related to this paper, Blickle et al. (2019) study the Great Depression in Germany and find that well capitalized and more liquid banks were less likely to experience deposit outflows. That paper and this paper are distinct from models in which flightiness can be disciplining behavior, such as in Diamond and Rajan (2001) or Calomiris and Kahn (1991), because the withdrawals are not disciplining the institution.

Second, this paper can help to understand how non-commercial banks operate. While commercial banks have been a heavily studied topic in financial history, relatively little attention has been focused on the "non-traditional" sector. The Building and Loan sector, studied in detail in this sector, has received increased attention in recent years. Work by Snowden (1997), Snowden (2003), Fleitas et al. (2017), and Rose and Snowden (2013) have established the importance of Building and Loan Associations in the institutional mortgage lending market in the United States in the first half of the twentieth century as well as their lasting influence on the structure of the residential mortgage contract. Mitchener and Richardson (2013) look at non-member country banks in the Great Depression and find a large role for financial contagion to cities. More recently, Shin (2009) looks at the case of Northern Rock during the Great Recession and notes its reliance on non-retail funding in the form of short-term lending.

Finally, this paper also contributes to a large literature on how banks' reliance on short-term funding contributes to financial distress. In the domestic context, Ivashina and Scharfstein (2010) find that a run in the short-term lending market contributed to bank distress in the United States. More globally, a run in the US Monetary Market Mutual Fund sector led to a tightening of supply in the US dollar market, with negative effects for European banks reliant on short-term dollar funding. (Ivashina et al., 2015)

3.2 Historical Background

Overview of Buildings and Loans

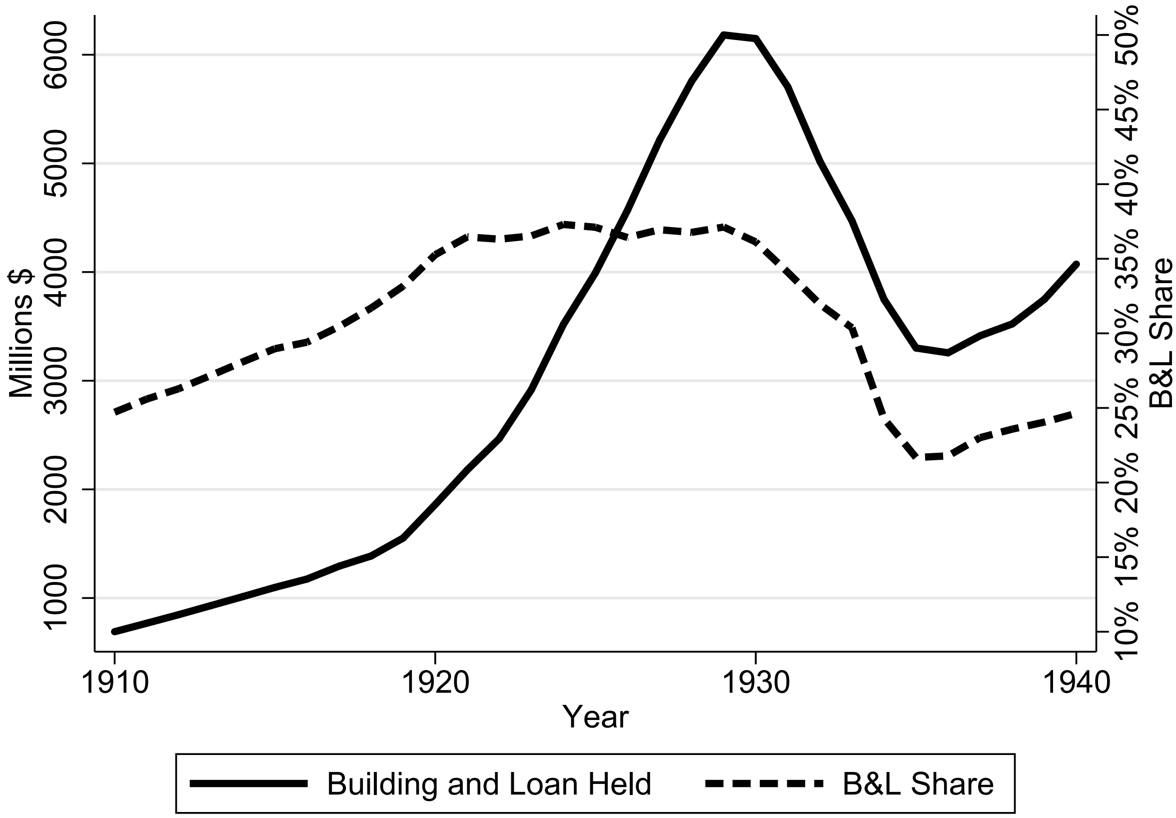
Building and Loan Associations (B&L's) were one of the most important lenders in the United States institutional home mortgage market over the first few decades of the twentieth century.[3] Figure 3.1 plots mortgage debt held by B&L's for all single-family residential structures in both millions of dollars and as a share of the total amount of institutional mortgage debt.[4] B&L growth followed that of the nation as a whole, but took on an increasingly larger share of institutional mortgage debt, peaking at almost just over 33% in the 1920s before collapsing during the Great Depression. The number of B&L's in the country doubled in the 1920's, from 5,869 in 1920 to 11,777 in 1930, with assets per association nearly doubling over that same time period. (Bodfish, 1935)

Throughout this time period, B&L's were seen and marketed as safe vehicles for savings. On January 6th, 1927 the Wall Street Journal Editorial Board wrote "[i]n less than ten years the savings of men and women of small income, in this one form alone, have considerably more than trebled." On August 14, 1927 Gilbert H. Beesemeyer wrote in the Los Angeles

[3]For more in depth historical overviews of B&L institutions, see Clark and Chase (1927), Bodfish (1935), and Snowden (1997), among others.

[4]This data comes from the Historical Statistics of the United States, Series Dc906 and Dc910.

Figure 3.1: Rise and Fall of B&Ls



Source: Historical Statistics of the United States, Series Dc906 and Dc910

Times that "the high regard in which the [B&L's] have come to be regarded by all sections of the general public is strikingly illustrated by their growth." Pieplow (1931) called Building and Loan Associations "the safest, most convenient, and fairest earning institution that we have to aid a person who really desires to save and invest money."[5]

B&L's, along with the rest of the nation's financial system, faced trouble with the onset of the Great Depression. Members with liquidity needs began withdrawing their savings from B&L's. Unemployment forced many borrowers to withdraw funds and cease savings. In addition, B&L's borrowed nearly $400,000,000 from commercial banks in 1930 to meet these withdrawal requests. (Bodfish, 1935) Starting in 1932, building and loan associations were faced with public fears due to the mass failures of commercial banks. As commercial banks failed, they began to call in their advances to building and loan associations, which were believed to have high cash balances. Foreclosure and repossession was common, and by

[5]One caveat is that nearly all surviving descriptions of the B&L industry comes from those who had an incentive to advocate for it. (Snowden, 1997)

the end of 1934 between 15 and 20 percent of building and loan assets were in repossessed properties.

By 1935, the federal government had implemented a number of new laws targeting the B&L industry. The Federal Home Loan Bank System (1932), the Federal Savings and Loan Charter System (1933), and the Federal Savings and Loan Insurance Corporation (1934) were all established in just a few short years. In particular, these policies made it possible for B&L's to "federalize," or join the Federal Home Loan Bank system (in the same manner as commercial banks could become Federal Reserve banks). Snowden (2003) discusses how these laws help create the Savings and Loan industry that would come to persist for the following decades.

Types of Buildings and Loans

B&Ls trace back to at least early nineteenth century England.[6] The first B&L in the United States was the Oxford Provident Association in Frankfort, PA. 1831.[7] This original B&L followed what was known as the *Terminating Plan*. A group of households would get together and put forward the same amount of funds for stock purchases in the association. Loanable funds were then auctioned to these members, and the precedence was given to those who were willing to pay the highest "premium" for funds, which were discounted from the gross amount the household was able to borrow. This mortgage was accompanied by periodic repayments towards interest, amortization, and installment on stock payments. As members saved by paying dues and borrowers repaid, new members would then become borrowers. These payments pre-specified the end date of the last mortgage payment, following which the institution was liquidated.

There were two main problems with this plan. First, the plan was inherently temporary, which ran counter to hopes of long-term savings. Second, new investors had to back-pay to maintain equal standing with other investors. To fix these problems, B&Ls soon took on two related forms called the *Serial Plan* or the *Permanent Plan*.[8] Known colloquially as the "Pennsylvania Plan", this structure allowed for several series of stock to be issued, each maturing at different times. Under this plan, investors were now able to purchase stock without paying prohibitively large back-payments to catch up to earlier members. However, members still had to commit to a long-term savings plan, and these associations frequently had high withdrawal penalties.

Taking this idea to the limit, B&Ls eventually developed into a form known colloquially as the *Dayton Plan*. Institutions using this plan allowed individuals to make payments whenever they so pleased rather than as part of a regular interval. At any point, the investor

[6]This section draws heavily from Riegel and Doubman (1927) and Clark and Chase (1927).

[7]See Chapter III of Bodfish (1935) for a summary of this association.

[8]The Serial plan states that there will be more than one official series (with a defined start point) that individuals can buy into. The Permanent Plan states that individuals can buy in at any time, thus start their own unofficial series. The differences between these two are not important for the analysis in this paper as I focus more on the long-term commitment rather than the initial cost.

Table 3.1: Distribution of Plans in the United States in 1923

Plan	Number	Per Cent
Terminating	96	0.92%
Serial/Permanent	9,121	87.04%
Dayton	1,186	11.32%
Other	76	0.73%
Total	10,479	100%

Serial/Permanent plans calculated as the sum of "All Permanent" and "Regular Permanent." Totals may not add up to 100% due to rounding. **Source**: Page 61 of Clark and Chase (1927), Authors Calculations.

could choose to withdraw his money (Pieplow, 1931). In the extreme, these institutions accepted deposits, which led to observation that the B&L were "open to the charge of being savings banks, a term frequently applied as a stigma." (Clark & Chase, 1927) Dayton plans were most common in Ohio (hence the name Dayton) and a few other states in the country, such as California.

The difference in the structure of assets across the plans were minor. All institutions, being building and loans, specialized heavily in mortgage loans. All institutions required their borrowers to be members of the association. The only difference of note between the Dayton plan and previous plans was the elimination of the premium on loans. When funds were available, potential borrowers bid in order to access them. The highest bid, known as the premium, was added to the mortgage loan. (Clark & Chase, 1927) These premiums were eliminated for Dayton plans.

There were therefore two broad classes of B&L's operating during the 1920s. The Dayton plans, which were essentially commercial banks and had characteristics that catered to short-term investors and looked much more like a commercial bank, and permanent plans that required more of a regular commitment by borrowers. Both plans invested heavily in local real estate by permitting only its members to borrow.

Table 3.1, taken from Clark and Chase (1927), shows the distribution in 1923 for the United States as a whole. Terminating plans were almost completely eliminated, accounting for less than 1% of the total. Serial or permanent plans accounted for 87%, while Dayton plans accounted for a little over 11%. Because this includes all states, it includes some places, such as New Jersey, where the only plans in operation were permanent plans. This study analyzes the effect of depositor quality by looking at how these liability structures contributed to B&L distress in California, which had a mix of Dayton and permanent plans, in the Great Depression. To do so, I next provide an in-depth look at California B&L's.

California Building and Loans

The history of B&L's in California traces back to at least 1893, when the first annual report of the Office of the Board of Commissioners of the Building and Loan Associations was issued.[9] Figure 3.2 shows the development of total assets and the total number of associations from 1920-1934.[10] As elsewhere in the country, during the 1920s the number of associations and the total number of assets were on the rise. The number of associations peaked in 1929 with 233 associations, whereas the total value of assets peaked in 1930 with $513,110,594.58.[11]

The first annual report in 1893 listed 146 active B&L's (of which, 137 reported balance sheets). Discussion of the type of plan used was important enough to be included in this report as well. As B&L's were growing in size, at this stage the report was mostly interested in inequality between borrowers and non-borrowers. This report does not mention Dayton plans or Permanent/Serial plans. Instead, this report defines B&L's based on their scope of operation (Local/National), and whether or not members plan to eventually become borrowers (Type of Premium). The latter distinguishes between the types of shares issued by B&L's: free shares (non-borrower) vs. pledged shares (borrower). Importantly, this "premium" is not a characteristic of Dayton plans, confirming that all B&L's in California still operated as Permanent/Serial plans. By the third annual report in 1895, the Dayton plan began to be used by two institutions in California. By 1900, the seventh annual report, California was well aware of the transition from Permanent/Serial to Dayton. "...the old Terminating association was succeeded by the Serial and is now fast being succeeded by the Dayton."' By 1905, this number jumped to 24 officially listed. California B&L's also began the process of federalization around the same time as the rest of the country.[12] In California, the Bakersfield Mutual Building and Loan was the first to federalize in 1934, and by the end of 1936 there were 36 federalized S&L's in California.[13]

The oldest B&L in the state was the Germania Building and Loan Association of Sacramento, incorporated in 1872. There were 34,169 members (out of a total California population of around 1.2 million) with 8,972 borrowers. Although this first annual report was issued in the midst of the 1890s long economic malaise, the report emphasizes that B&L's in California were largely unaffected by the panic. In fact, with the exception of three B&L's, all B&L's that submitted balance sheets reported a net profit.

[9]Prior to 1893, some B&L's were incorporated under various legislative environments in California. In 1893, all B&L's were consolidated under the same California law. This law also created the state's Building and Loan Commission, which started the series of annual reports used extensively in this paper.

[10]Not all associations reported each year, although the annual reports do not mention why. In the analysis section, I can only work with the number of reporting associations. Usually only 10 or so associations do not report. This is mostly because they are new associations active for less than a year.

[11]Prior to 1931 these numbers represent the June 30th statements at the latest for each year. B&L's were free to choose their own fiscal year, so these numbers aggregate together all the statements sent in. Starting in 1931, the state standardized returns, and they are then all of December 31 of each year.

[12]Federalization refers to converting B&L's from being under state control to being under federal control. See Snowden, 2003 for more about this process.

[13]This number comes from the 1937 Annual Report of the Building and Loan Commissioner.

In the 1929 Annual Report to the Governor of California, California State Commissioner George Walker wrote that while "Building and loan laws have been materially strengthened during the last few years, ... additional power should be granted your commissioner. Because of the advertised profitableness of the building and loan business, promoters and others are endeavoring to organize new associations in every part of the state regardless of the fact that in many sections the business is already overdone." (Annual Report, 1929) Although B&Ls continued to increase in size through 1930, new commissioner Charles Whitmore wrote to the Governor that "Loan commitments by associations showed a decline for the year of 38 per cent." (Annual Report, 1930) However, he did not see any cause for concern, writing that "conditions in many parts of the state show signs of returning normality, and more and better loans are now being offered for association investment." The following few years would be hard for B&L's, as the total number operating in California declined from 233 total associations in 1929 to 178 associations in 1934.[14]

In the 1931 annual report, new commissioner Friend W. Richardson, worte that he "hardly think[s] it would be proper for [him] to comment upon the history of building and loan associations ... as [he] did not take office until January 1, 1932." (Annual Report, 1932) In the 1932 annual report, Richardson then wrote that "[t]he year 1932 was the most critical in the history of building and loan associations." (Annual Report, 1933) Through 1934, the number of associations and the total amount of assets was on steady decline, as was common throughout the country. I stop my analysis in 1935, as the passage of numerous laws during the Great Depression to assist the housing crisis related to federalization is sufficiently disruptive to the prior environment, however the trend is similar.[15]

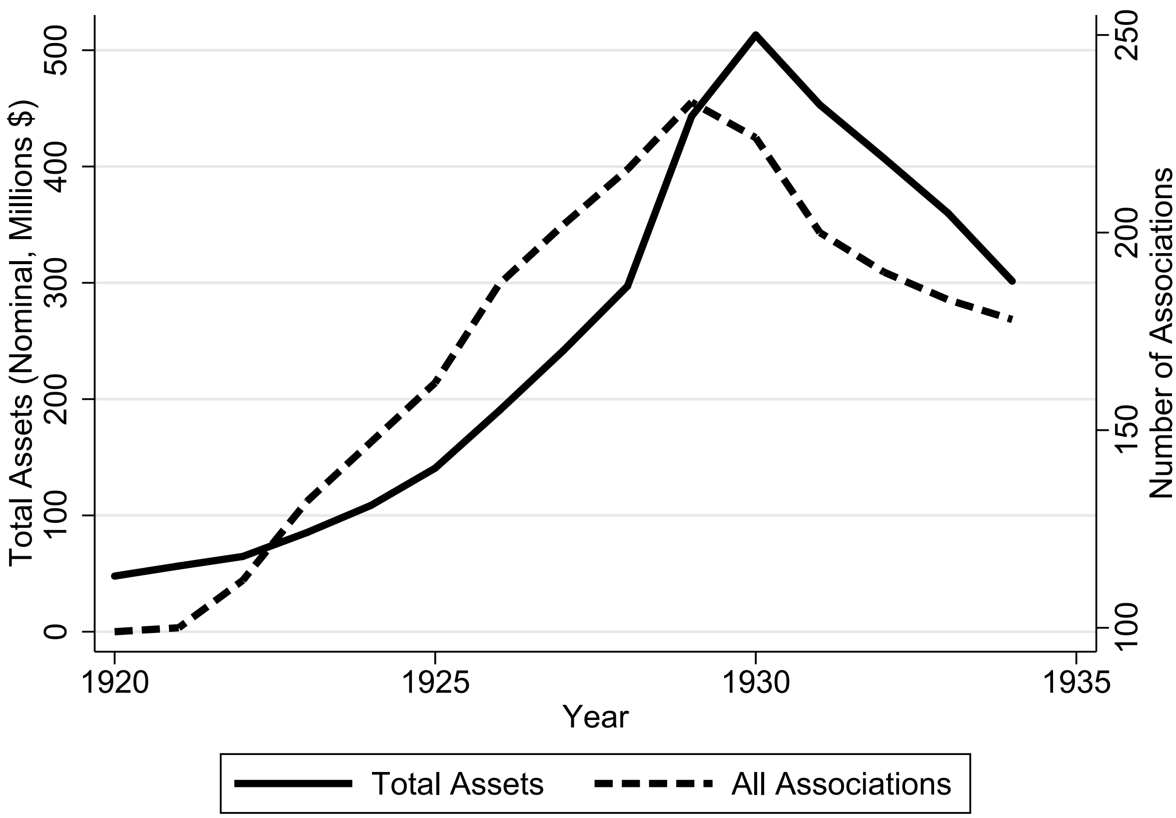
Figure 3.2 maps the location of B&L's in California in 1927, the base year of my analysis. Most of the map shows zero B&L's. However these places had relatively few people, and so there were less demand for B&L's. Second, most cities did not have more than one Building and Loan Association. Conditional on having at least one B&L, two-thirds of cities have exactly one. However, assuming B&L's competed with commercial banks for savings, then the number of banking institutions per city is likely much higher. An important assumption in this paper is that B&L's of a specific type are not "the only game in town" for savings. Finally, the big cities, Los Angeles, Oakland, San Jose, and San Francisco, account for a large percentage of Building and Loans. Together they account for roughly one-third of all building and loan associations in the state.

In California, as in most states, the only loans B&L's were allowed to make were for real estate. Title XVI of the California Civil Code in 1929 required loans to be secured by a first mortgage with appraised value equal to 25% of the size of the loan (with some exceptions). If a borrower couldn't pay his or her debts, the Building and Loan Association could, after a period of 6 months, issue him a notice of default in writing. If the borrower didn't repay

[14]Following this year, it is difficult to track the total number. B&L's were allowed to federalize, and the state commissioner did not compile statistics on Federalized B&L's due to newly passed legislation.

[15]Note that this differs substantially from the analysis in Fleitas et al. (2017). In New Jersey, they find that most voluntary liquidations occurred during 1934-1940. I find the opposite, as by 1935 many voluntary liquidations had already occurred.

Figure 3.2: California B&L's in the Great Depression



Source: Annual Report of Savings and Loan Commisioner (Various Years)

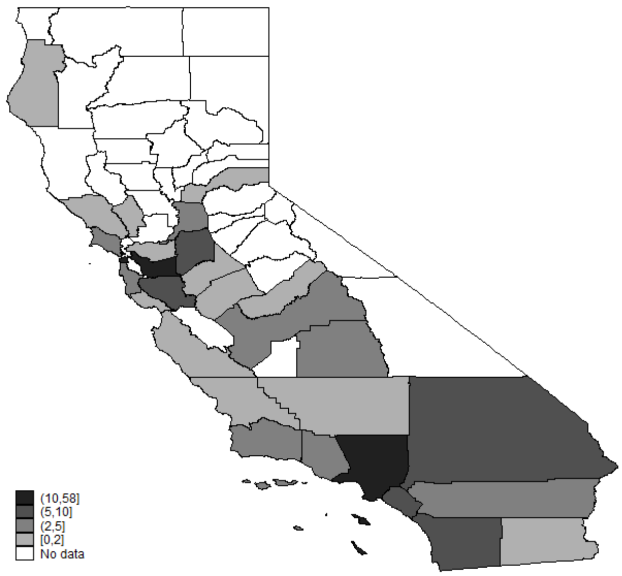
his or her debts within 2 months, he or she is in default, and the association may, by law, purchase the property.

One key difference between Dayton plans in California and most others around the country is that California B&L's issued investment certificates. Investment certificates were guaranteed by either guarantee stock or some sort of guarantee fund held by the B&L. Clark and Chase (1927) view these certificates as comparable with certificates of deposit, as they make it "possible to withdraw money quickly and take it elsewhere." By law, these investment certificates also created a sinking fund type of investment for individuals. The law stated that the repayment schedule could either be forced, as in the case of the permanent plans, or up to the investor.

Withdrawal penalties in California were much more lax than in other parts of the country. Clark and Chase (1927) note that California is one of only two states that does not permit forfeiture of principal when investors withdraw either installment shares or investment certificates. Instead, entrance fees or withdrawal fees are charged.[16] As early as 1891,

[16]Clark and Chase (1927) note that these fees may be high enough to effective reduce the principal if an

Figure 3.3: County Distribution of California Building and Loan Associations (1927)



This figure plots the number of Building and Loan Associations in each County. **Source**: Building and Loan Commissioner of the State of California (1927)

withdrawals of *stock* were subject to thirty days notice. By 1927, with the introduction of investment certificates, all types of liabilities were subject to thirty-day withdrawal notice, a time period similar to savings banks.

Do B&L's Fail?

B&L's were fundamentally different institutions than commercial banks. Commercial banks' main source of liabilities were depositors that owned debt contracts in the commercial bank. In the event that a bank couldn't pay out depositors, then the bank could be forced to close. However, for B&L's around the country, most contracts were in fact *equity* contracts. Households held stock in the institution, and the institution was not always required to repay shares on demand. Instead, B&L's were liquidated.

The liquidation option has been studied in the New Jersey context by Fleitas et al. (2017). In New Jersey, voluntary liquidation occurred when two-thirds of members, either borrowing or non-borrowing, voted to liquidate. They find that the probability of liquidation rose when there was a higher share of non-borrowers. The 1891 California B&L act dictated that "dissolution" can also be either involuntary (if the association commits a crime) or voluntary. In the case that dissolution was voluntary, two-thirds of stockholders or members must agree to dissolve the corporation. So, unlike banks, institutions did not fail because of a conventional run.

investor withdraws too early.

Fleitas et al. (2017) compare this liquidation option to two other scenarios available to B&L's. First, B&L's in New Jersey could freeze and postpone withdrawals indefinitely. A similar law was in place in California. Thirty days notice was required by investors to sell back shares or redeem investment certificates. However, even with thirty days notice for withdrawals, reorganization and liquidation of many institutions became necessary. In extreme cases, this could be done by the state commissioner. According to the 1930 annual report, the commissioner was given power to "revoke an association's licence [due to] insolvency of the association." While the B&L's were frozen, they would wait for borrowers to pay back enough of their loans that withdrawals could be paid out.

Second, members who held stock in the association could sell their shares in informal secondary markets. Rose (2014b) finds that in New Jersey, these secondary markets were common throughout the country. There is evidence that these markets existed in California. He finds that as late as 1934, share prices in San Francisco were 50 cents on the dollar.

In this paper, I assume that the primary goal for members was to access liquidity. The decision to close the B&L is thus assumed to be the way for individuals to access this liquidity. If instead, members of permanent plans chose to sell in the secondary markets while members of Dayton plans chose to voluntarily close their institutions, then this assumption would be violated.[17] While it is true that there was no bank run at B&L's in the conventional sense, there still existed points in time when members of B&Ls desired access to funds and were willing to go to the length of closing the institution in order to access it. Understanding this flightiness motive by different types of members is the key goal of the paper.

3.3 Data

I draw on historical data on B&Ls in California. I focus on California for a number of reasons. First, California has a non-trivial share of permanent and Dayton plans, unlike almost every other state in the country. Second, the amount of money invested, in terms of assets per member, was higher relative to the United States as a whole. In 1923, assets per member in California were $1,014.22 compared with $486.96 for the United States as a whole. (Clark & Chase, 1927). Members in California thus presumably relied more heavily on B&Ls as a source of investment, making the B&L choice salient. Third, data availability makes California B&L's attractive to study. Annual balance sheet and profit and loss data is available from the Annual Report of the State Building and Loan Commissioner. Additionally, select underlying data from the annual reports has survived over the last century to provide additional insight into how B&L's operated during the Great Depression. Finally, California's Building and Loan League was active in preventing so-called "National" B&L's, or B&L's headquartered outside of the state of California, from entering. Thus, nearly every B&L operated almost exclusively in California, limiting the effect of external factors in determining closure rates.

[17]For the 1938-1939 period, Rose (2014b) finds little volatility in share prices in New Jersey. He traces the earliest such secondary market to 1933, well after the time period I consider.

Public Annual Reports Data

I use the appendix to the 1927 annual report to construct a cross-section of B&L's balance sheets in California for 1927.[18] I focus on 1927 for three reasons. First, the 1927 annual reports explicitly stated when the institution was a Dayton plan. Second, data availability in 1927 was at its highest. The breakdown of liabilities distinguishes between withdrawable shares (a characteristic of permanent plans) vs. investment certificates (a characteristic of Dayton plans). Along with balance sheets, which were available every year, there is also data on dues, withdrawal value, and dividends. Finally, 1927 allows me to condition on pre-period characteristics observed prior to the onset of the Great Depression, avoiding any effects from depressed aggregate economic conditions. I therefore assume that the onset of the Great Depression was sufficiently unexpected that the decision to start and operate a B&L by 1927 was independent of this aggregate shock.

An example of a balance sheet for a Dayton B&L is displayed in Figure 3.4. At the top of the figure, there is demographic information about the B&L, such as the number of members/investors and shares. In the middle of the page is the balance sheet data. On the asset side, the large reliance on real estate loans is clearly visible. On the liability side, investment certificates are listed as the third item.Finally, at the bottom of the page, one can see clearly that the association is labeled "Dayton Plan." There is also additional data on dues and withdrawal value. For an example of a permanent plan, see Figure 3.5. The key difference is the reliance on shares, rather than investment certificates, and the listing of individual series at the bottom of the page.

[18]For one exercise, I use the cash-flow statement from the 1930 annual report. These cash flow statements are nearly identical to those in 1927.

Figure 3.4: Balance Sheet: Dayton Plan

220 *Thirty-fourth Annual Report of*

No. 136—SAN FRANCISCO.

METROPOLITAN GUARANTEE BUILDING-LOAN ASSOCIATION.

P. O. address, 915 Mission Street, San Francisco.

Incorporated December 18, 1924. Fiscal year ended December 31, 1926.

Officers—Harvey M. Toy, President; J. H. McCallum and C. M. Wooster, Vice Presidents; P. F. Fratessa, Attorney; D. C. Watson, Secretary.

Directors—Harvey M. Toy, C. M. Wooster, D. C. Watson, T. M. Gardiner, Ben W. Reed, W. E. Bouton, H. S. Thompson, S. C. Symon, B. Grant Taylor, C. I. Dennis, J. H. McCallum, W. E. McDonough, Geo. K. Rogers, W. G. Metson, J. H. Roberts.

No. of series, none. No. of members and investors, 469. No. of shares, 815.

Assets.		Liabilities.	
Loans on real estate, shares, etc.	$302,161 92	Guarantee stock, capital	$81,495 07
Arrearages on dues, interest, etc.	3,128 93	Guarantee stock, surplus reserve	2,578 72
Cash in office and bank	8,013 14	Investment certificates, principal	188,053 13
Furniture and fixtures	2,726 21	Investment certificates, interest unpaid	1,327 60
Advances, ledger accounts	559 12	Overdrafts and bills payable	25,000 00
		Reserve and undivided profits	1,600 78
		Loans due and incomplete	16,514 00
		Sundry ledger accounts	20 00
Total assets	$316,589 32	Total liabilities	$316,589 32

Receipts for Fiscal Year.		Disbursements for Fiscal Year.	
Balance from last report	$11,733 53	Overdrafts and bills payable	$30,000 00
Guarantee stock	26,991 19	Loans on real estate, shares, etc.	267,535 81
Guarantee stock premium	7,583 87	Interest paid	504 05
Investment certificates	214,995 87	Dividends on guarantee stock	6,715 22
Interest	16,755 61	Investment certificates, principal	71,446 35
Fines	50 46	Investment certificates, interest	7,331 90
Fees	4,851 21	Advances, ledger accounts	559 12
Loans repaid	86,371 43	Salaries	3,211 86
Overdrafts and bills payable	40,000 00	Taxes	391 79
		Other expenses	12,308 00
		All other disbursements	326 93
		Balance, cash in office and bank	8,013 14
Total receipts	$408,343 17	Total disbursements	$408,343 17

Installment Shares and Certificates. Age, Value, and Withdrawal Value.

Dayton plan.
Dues 50 cents per certificate per month.
Dividend, last fiscal year, 6 per cent.
Book value, dues plus dividend.
Withdrawal value, full book value.

Source: Building and Loan Commissioner of the State of California (1927)

Figure 3.5: Balance Sheet: Dayton Plan

230 *Thirty-fourth Annual Report of*

No. 146—SAN FRANCISCO.

WESTERN LOAN ASSOCIATION.

P. O. address, 1150 Divisadero Street, San Francisco.

Incorporated November 12, 1886. Fiscal year ended March 21, 1927.

Officers—F. R. Dann, President; P. N. Williams, Vice President and Manager; Leon E. Morris, Secretary and Attorney.

Directors—F. R. Dann, P. N. Williams, H. Dederky, Jr., J. A. Wallacher, E. H. Rixford, Samuel Rhine, S. E. Wallacher, M. M. Williams, Leon E. Morris.

No. of series, 20. No. of members and investors, 124. No. of shares, 680.

Assets.		Liabilities.	
Loans on real estate, shares, etc.	$132,042 98	Guarantee stock, capital	$30,000 00
Arrearages on dues, interest, etc.	2,516 35	Installment shares, dues	46,712 00
Cash in office and bank	23,450 20	Installment shares, profits	17,325 63
Other real estate owned	550 00	Paid-up and matured shares, principal	31,710 47
Advances, ledger accounts	3,803 43	Advance payments	71 25
		Reserve and undivided profits	13,787 87
		All other liabilities, real estate suspense	22,755 74
Total assets	$162,362 96	Total liabilities	$162,362 96

Receipts for Fiscal Year.		Disbursements for Fiscal Year.	
Balance from last report	$14,878 91	Overdrafts and bills payable	$2,000 00
Installment shares, dues	6,956 00	Loans on real estate, shares, etc.	5,207 44
Paid-up and prepaid shares, dues	5,000 00	Interest paid	2,225 87
Interest	14,101 01	Dues repaid, installment shares	7,923 00
Premiums	484 40	Profits repaid, installment shares	3,685 51
Fees	90	Paid-up and prepaid shares, capital	12,857 77
Loans repaid	19,859 70	Advances, ledger accounts	1,732 41
Overdrafts and bills payable	2,000 00	Salaries	3,600 00
Advances, ledger accounts	4,863 57	Taxes	59 79
		Other expenses	1,364 50
		All other disbursements	4,048 00
		Balance, cash in office and bank	23,450 20
Total receipts	$68,144 49	Total disbursements	$68,1 4 49

Installment Shares and Certificates. Age, Value, and Withdrawal Value.

Serial No.	Age in months	Total dues per share	Book value per share	Withdrawal value
31	124	$124 00	$180 76	$167 57
32	112	112 00	157 66	146 24
33	100	100 00	135 95	125 16
34	88	88 00	116 82	106 73
35	76	76 00	96 18	88 09
36	72	72 00	89 89	81 83
37	60	60 00	72 08	66 04
38	48	48 00	55 47	51 73
39	36	36 00	39 99	37 99
40	24	24 00	25 68	24 84
41	12	12 00	12 39	12 19

Source: Building and Loan Commissioner of the State of California (1927)

204 B&L's submitted annual reports to the commissioner in 1927. Of these, I drop twelve institutions that began business less than a year prior and did not have complete information. I also drop two national B&L's from Salt Lake City, Utah. Of the remaining 190 B&L's, six were federalized by 1936, and one has incomplete data. I drop these in the baseline specifications, but check the sensitivity of the results to their inclusion in a robustness check. Finally, I drop twenty B&L's that closed in 1927 and 1928 prior to the onset of the Great Depression.[19] The final sample has 163 B&L's.

[19]Ideally, one would drop B&L's that closed in 1929 but prior to October 29, 1929 (Black Tuesday). Unfortunately, the appendix to the annual reports to not give the exact dates of closure, so I assume all 1929

Table 3.2: California Building and Loan Associations by Type (1927)

City	Dayton	Permanent
All	126	37
City	40	19
Los Angeles	23	1
Oakland	5	1
San Francisco	12	7

This includes only the 163 non-federalized, non-national California B&Ls in operation at least one year that failed as early as 1929. City includes Los Angeles, Oakland, and San Francisco. **Source**: Building and Loan Commissioner of the State of California (1927)

Table 3.2 breaks down the B&L's in the sample by type. In California in 1927, there were significantly more Dayton Plans than non-Dayton Plans.[20] This is true not only in the state as a whole, but also in the urban areas. I use the city listed by the B&L statement to divide B&L's into different cities. This is a rather narrow definition of cities. For example, Los Angeles does not include Anaheim or Santa Ana, which both have permanent plans. With this definition, Los Angeles is particularly lopsided, with all but one association operating under the Dayton Plan. Oakland has a similar pattern. For San Francisco, around 50% of each type are operating. Of the approximately sixty cities with only one B&L, one third had a permanent type and two-thirds had a Dayton type. This is the a similar distribution to the total, making the "only game in town" hypothesis unlikely to drive any results.

From the 1927 annual report, I record the balance sheet of each B&L. The main variables on the asset size of the balance sheet include the reliance on real estate loans and cash. From the liabilities side of the balance sheet, I record all sources of liabilities, including the value of outstanding withdrawable shares and investment certificates. I also record the total number of members, the total number of certificates and shares, and the dues per certificate or share.[21] I also record how the B&L calculates withdrawal value.

To track what happened to the B&L over time, I use the 1935 annual report. From the 1935 annual report I record the history of changes in the operating status of B&L's. The changes are classified as one of the following: Absorbed, Changed, Closed, Consolidated, Federalized, Open, Removed, and Transferred. As mentioned previously, in all main specifications I drop those that are Federalized by the new federal housing legislation, as these are no longer monitored by the state commissioner. I count as failures those listed as "Absorbed", "Closed", "Consolidated", and "Transferred".[22] If the business is listed

closures are due to the Great Depression. I show in a robustness check that the results are quantitatively similar when dropping all 1929 closures.

[20]For the nineteen Federalized B&L's, there are eleven Dayton B&L's that federalized by 1936 compared with eight permanent plans.

[21]While dues per share could theoretically differ across different series for non-Dayton plans, in practice they did not.

[22]Absorbed occurs when a B&L is bought by another B&L. Consolidation occurs when small institutions

as "Changed" or "Removed", I list them as open, as these represent relocations or name changes. In total, there were 71 failures from the 1927 listing of B&L's.[23]

Summary statistics for the 171 B&L's are reported in Table 3.3. Some key differences stand out. First, permanent B&L's were older. This is expected due to the historical development of B&L's explained earlier. Second, while Dayton and permanent B&L's were of roughly similar sizes in terms of assets, Dayton B&L's had more members on average. As previously mentioned, we can see that Dayton plans offered overwhelmingly more investment certificates, while permanent plans relied more heavily on shares. Both make up more than half of their liabilities on average.[24] If anything, Dayton B&L's had more liquidity available in terms of cash ratios. One might then expect that higher liquidity would lead to lower failure rates, all else equal. This difference therefore biases the main hypothesis of finding higher failure rates at Dayton B&L's. In the last row, I show that the simple difference in mean failure rates suggests that Dayton B&L's failed at higher rates that permanent B&L's.

Archival Data

I also use surviving archival data available from the California State Archives (CSA) in Sacramento, California. The CSA has records divided by office: either San Francisco or Los Angeles. The San Francisco office consists of records only since 1968, far too late to be useful. The Los Angeles office, on the other hand, has some records dating back as early as the 1900s.

I use raw copies of the detailed balance sheet data submitted by the B&L's. These balance sheet form the basis of the reported balance sheets in the annual reports. Along with the publicly available information, they also include additional statistics such as lending rates and member returns. An example is given in Figure 3.6. The row labeled "Interest on Loans" is assumed to represent the average interest on mortgage lending. The rows labeled "Investment Certificates" are assumed the represent the average rate of interest on the various types of investment certificates offered. This institution is a Dayton institution, which is also evident from the "None" listed next to the returns on membership shares. In the analysis that follows, I take simple averages of the rates by type (loan, investment certificates, and shares) because there is no information on the distribution of lending rates by institution.

get bought by large institutions. I treat this as a failure as, according to the 1910 Annual Report, "The larger volume of assets, coupled with a good reserve, attracts the attention of the public, commands respect, and attracts more and better business." I interpret this statement as saying the institutions would have failed if not consolidated with a larger enterprise. Closed is either due to involuntary and voluntary liquidation, as discussed earlier. Transferred occurs when one institution closes and simply moves its business to another B&L. In practice, Absorbed, Consolidated, and Transferred are very similar. In my data, I have only one absorption, zero consolidations, and fifteen transfers. None of the main results in the paper depend on including these as failures.

[23]New B&Ls were opening during the Great Depression, which is why the net difference between the number of B&L's in 1927 and 1935 is less than 72.

[24]Note that total liabilities includes the guarantee stock, as this was listed as a liability in the balance sheets at that time.

Table 3.3: Balance Table (1927 Values)

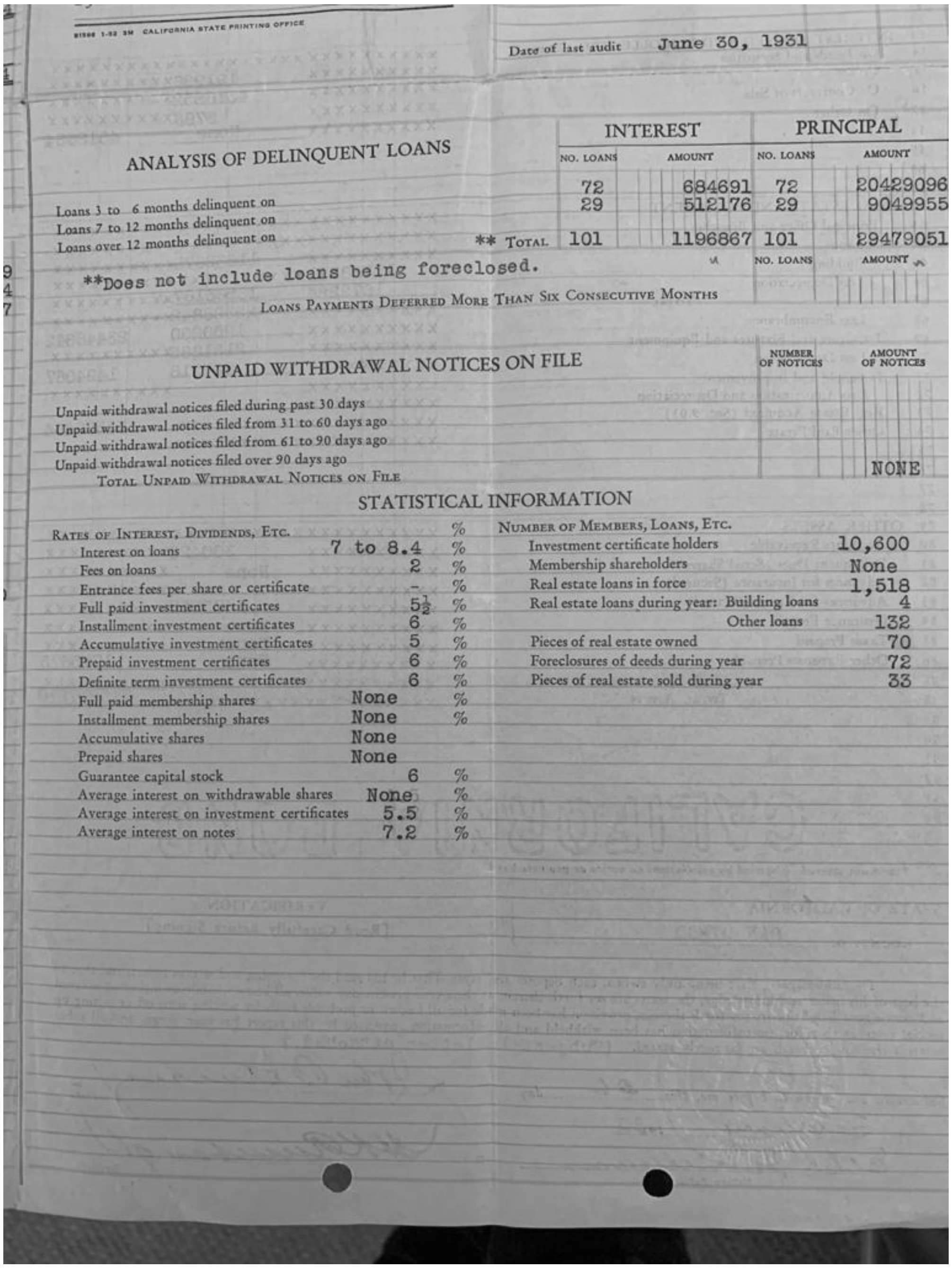
Variable	(1) Permanent	(2) Dayton	(3) Diff
Age	31.75	12.01	-19.74
	(12.03)	(14.25)	(2.61)***
Log Assets	12.92	13.29	0.37
	(1.11)	(1.40)	(0.25)
Log Members	5.99	6.58	0.59
	(1.08)	(1.38)	(0.25)**
Investment Cert. (% Liabilities)	22.43	53.21	30.78
	(29.35)	(28.67)	(5.39)***
Shares (% Liabilities)	61.42	21.27	-40.16
	(29.76)	(31.38)	(5.80)***
Cash (% Assets)	2.99	5.02	2.03
	(3.10)	(4.75)	(0.83)**
Loans (% Assets)	95.50	89.90	-5.60
	(3.56)	(8.36)	(1.41)***
Failure	0.14	0.33	0.20
	(0.35)	(0.47)	(0.08)**
Observations	37	126	163

Robust Standard Errors in Parentheses. Age calculated as number of years open as of 1927. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. **Source**: Building and Loan Commissioner of the State of California (1927,1935)

These reports are only available at 5-year intervals for a limited number of B&L's. I focus on the 1931 annual reports, which is the earliest year for which a substantial number of B&L's have surviving balance sheet data.[25] I hand match this data to the 1927 balance sheet data. I am only able to match 104 B&L's with at least some usable data, which is a little more than half of the sample. For the remainder, the B&L's most likely either failed before 1931, changed names or moved (making them difficult to match). Table 3.4 compares summary statistics for B&L's inside and outside of the sample. While observable characteristics are mostly similar, the sample of B&L's with micro data have significantly lower failure rates.

[25] There is some data going back to 1926 for an extremely limited number of B&L's. There is also balance sheet information on surviving 1936 annual reports, however I do not use it. First, the number of failed B&L's cuts the number of reporting institutions by at least a third. Second, any institutions that were "federalized" no longer report balance sheets to the state regulators.

Figure 3.6: Balance Sheet: Raw Archives



CALIFORNIA STATE PRINTING OFFICE

Date of last audit June 30, 1931

ANALYSIS OF DELINQUENT LOANS	INTEREST NO. LOANS	INTEREST AMOUNT	PRINCIPAL NO. LOANS	PRINCIPAL AMOUNT
Loans 3 to 6 months delinquent on	72	684691	72	20429096
Loans 7 to 12 months delinquent on	29	512176	29	9049955
Loans over 12 months delinquent on				
** Total	101	1196867	101	29479051

**Does not include loans being foreclosed.

Loans Payments Deferred More Than Six Consecutive Months — No. Loans / Amount

UNPAID WITHDRAWAL NOTICES ON FILE	NUMBER OF NOTICES	AMOUNT OF NOTICES
Unpaid withdrawal notices filed during past 30 days		
Unpaid withdrawal notices filed from 31 to 60 days ago		
Unpaid withdrawal notices filed from 61 to 90 days ago		
Unpaid withdrawal notices filed over 90 days ago		
Total Unpaid Withdrawal Notices on File		NONE

STATISTICAL INFORMATION

Rates of Interest, Dividends, Etc.	%
Interest on loans	7 to 8.4 %
Fees on loans	2 %
Entrance fees per share or certificate	- %
Full paid investment certificates	5½ %
Installment investment certificates	6 %
Accumulative investment certificates	5 %
Prepaid investment certificates	6 %
Definite term investment certificates	6 %
Full paid membership shares	None %
Installment membership shares	None %
Accumulative shares	None
Prepaid shares	None
Guarantee capital stock	6 %
Average interest on withdrawable shares	None %
Average interest on investment certificates	5.5 %
Average interest on notes	7.2 %

Number of Members, Loans, Etc.	
Investment certificate holders	10,600
Membership shareholders	None
Real estate loans in force	1,518
Real estate loans during year: Building loans	4
Other loans	132
Pieces of real estate owned	70
Foreclosures of deeds during year	72
Pieces of real estate sold during year	33

Source: Inventory of the Dept. of Savings and Loan Records. Records of the Los Angeles Office. F3739:425-450. California State Archives

Table 3.4: Balance Table for Existing Micro Data

Variable	(1) Missing	(2) Micro Data	(3) Diff
Dayton Plan	0.78	0.77	-0.01
	(0.42)	(0.42)	(0.07)
Age	17.59	15.57	-2.02
	(15.97)	(16.10)	(2.57)
Log Assets	13.39	13.08	-0.32
	(1.48)	(1.23)	(0.21)
Log Members	6.67	6.29	-0.37
	(1.33)	(1.32)	(0.21)*
Investment Cert. (% Liabilities)	45.99	46.38	0.39
	(33.01)	(30.60)	(5.03)
Shares (% Liabilities)	32.34	29.02	-3.32
	(37.06)	(34.03)	(5.62)
Cash (% Assets)	4.64	4.50	-0.15
	(4.22)	(4.70)	(0.72)
Loans (% Assets)	90.55	91.60	1.05
	(7.42)	(8.21)	(1.26)
Failure	0.51	0.14	-0.37
	(0.50)	(0.34)	(0.07)***
Observations	67	96	163

Age calculated as number of years open as of 1927. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. **Source**: Building and Loan Commissioner of the State of California (1927,1935)

$* \ p < 0.1$ $** \ p < 0.05$ $*** \ p < 0.01$. Robust Standard Errors in Parentheses.

3.4 Failure Rates

I begin by analyzing the probability of failure of Dayton B&Ls compared with permanent B&Ls. I estimate the following linear probability model[26]

$$Failure_i = \alpha + \beta Dayton_i + \Gamma X_i + \varepsilon_i \tag{3.1}$$

where $Failure_i$ is a dummy variable equal to one if an institution fails at any time between 1929 and 1935. The main regressor of interest, $Dayton_i$, is a dummy variable equal to one if an institution is reported as a Dayton plan in the 1927 annual report. The coefficient of interest, β, is hypothesized to be positive, indicating that Dayton B&Ls were more likely to fail compared with permanent B&Ls. X_i represent other controls at the institution level.

[26]In the appendix, I show results are robust to using a logit specification.

These include a dummy variable for being in a city, log assets, log members, and the share of assets in cash.

In a causal sense, the identifying assumption in this model is that the decision of whether or not to have a Dayton plan is uncorrelated with other determinants of failure that would be included in the error term ε_i. There are some threats to this assumption that are observable and hence can be directly controlled for. First, the size of B&L's in terms of the number of members may be a source of distress. If the probability of failure is based on the raw number of individuals who withdraw, rather than the share of individuals, then this could bias the results for β upwards since Dayton B&L's had more members on average. To account for this possibility, I include log members and log assets as controls in X_i.

A second threat to identification is local economic conditions, such as commercial bank competition or the state of the housing market. For example, local banking competition may push B&L's to take the Dayton plan. This competition may also result in higher failure rates if banking panics spread locally. This would bias the estimate upwards towards finding an effect. To account for this issue, I include city controls, such as city fixed effects.

Table 3.5 reports the results of estimating equation 3.1. The first column reports results from the univariate regression of failure on just the Dayton dummy without any controls. The point estimate of 0.225 implies that Dayton institutions had higher failure rates on the order of around 22.5 percentage points. This result support the hypothesis that Dayton plans did fail at higher rates.

The second and third columns control for two different measures of size, either log members or log assets. Larger institutions are more likely to fail, and also are more likely to be Dayton plans. As expected, the coefficient rises slightly compared with the first column. The fourth column controls for a measure of liquidity, the percentage of assets in cash, which also has only a small effect on the estimated coefficient. Finally, in the last column, including all the controls at once does not change the point estimate or confidence interval substantially.

The analysis so far compares across B&L's cities. The fifth column includes city fixed effects. Cities with only one B&L drop out, leaving only 127 B&Ls. The coefficient falls and becomes insignificant, but even with this highly restrictive sample Dayton plans are more likely to fail. The sixth column pools B&L's outside of a big city, defined as Los Angeles, Oakland, and San Francisco, and includes fixed effects for these three plus the remaining B&Ls. The logic is that because larger cities are likely to have more competition for deposits, this might have induced more B&L's to fail.

This sixth column is my preferred specification and implies that Dayton B&L failure rates were 16.7 percentage points higher than permanent institutions. To put this number into context, the unconditional probability of failure of all types was ≈ 0.42 with a standard deviation of ≈ 0.5. Having a Dayton plan as the organizational structure therefore raised the probability of failure by around one-third of a standard deviation.

Alternatively, I can also condition only on one location that has a mix of Dayton and non-Dayton plans. From Table 3.2, only San Francisco has a sufficient number of both Dayton and non-Dayton plans for such a test. The last column conditions only on San Francisco. The coefficient on Dayton plan remains of a similar magnitude when only looking at the

Table 3.5: Failure Rates: Linear Probability Model

	Failure	Failure	Failure	Failure	Failure	Failure	Failure
Dayton Plan	0.198***	0.202***	0.199***	0.185**	0.128	0.172**	0.150
	(0.0706)	(0.0718)	(0.0738)	(0.0719)	(0.131)	(0.0769)	(0.263)
Log Assets		-0.00995				-0.0116	-0.429***
		(0.0270)				(0.0581)	(0.0771)
Log Members			-0.000525			0.0116	0.225**
			(0.0282)			(0.0623)	(0.0995)
Share Cash				0.00670		0.00565	-0.0117
				(0.00811)		(0.00867)	(0.0206)
Constant	0.135**	0.264	0.138	0.115*	0.251**	0.208	4.355***
	(0.0566)	(0.353)	(0.175)	(0.0623)	(0.112)	(0.463)	(0.825)
N	163	163	163	163	115	163	19
R-Squared	0.03	0.03	0.03	0.04	0.22	0.06	0.41
City FE	N	N	N	N	Y	N	N
Large City FE	N	N	N	N	N	Y	N

This table presents results from estimating Equation (3.1): $Failure_i = \alpha + \beta Dayton_i + \Gamma X_i + \varepsilon_i$. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. City includes Los Angeles, Oakland, and San Francisco. **Source**: Building and Loan Commissioner of the State of California (1927,1935)

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$. Robust Standard Errors in Parentheses.

twenty non-Federalized B&L's in San Francisco. The small number of B&L's, unfortunately, limits power.[27]

One may be concerned about the difference in lending terms across plan types. If Dayton plans had loan terms that were "looser", then even though the value of their assets is similar to permanent plans, lower quality of assets may induce failure at higher rates under a monitoring hypothesis. There are two pieces of evidence that suggest this is not the case. First, discussed in the next section, lending rates for the two institutions were very similar. Second, the structural differences in lending would pertain mostly to repayment rates, as Dayton plans were less restrictive in how often the members had to repay.[28] If anything, the less restrictive repayment plans should mean that Dayton plans have more cushion during the Depression

[27] In this regression, dropping the only significant covariate, log assets, raises the coefficient on Dayton but does not improve power.

[28] In this paper, I focus on how the plans implied that *all members* could save at whatever rate they like. In contrast, Price and Walter (2019) study how for Dayton plans in Ohio in the 1880s, *borrowers* could repay at any rate they'd like.

to stay open. This is because individuals can simply delay their savings until the economy recovers. That Dayton plans still fail at higher rates suggest the effect is a lower bound with respect to this concern.

Another worry is that the share of borrowers was higher at Dayton plans than at permanent plans. Fleitas et al. (2017) find that in New Jersey, when the share of non-borrowers rose to around two-thirds (the cutoff for voluntary liquidation), then the probability of liquidation was much higher. The archival data has information on the total number of loans relative to the total number of investors. The average number of loans as a share of total members is around 50% for both Dayton and permanent plans, suggesting that this channel is unlikely to be the main driver of the differences in failure rates across institutions in my setting.[29]

Finally, one may wonder whether some permanent plans that would have failed simply chose to federalize instead. Due to the distress faced by B&L's during the Great Depression, United States federal policy in the 1930s allowed B&Ls to federalize and join the Federal Home Loan Bank system, created in 1932. As shown in the appendix, including federalization as a potential outcome in a multinomial logit specification does not change the main result, that Dayton plans failed at significantly higher rates, mainly because Federalized California B&Ls were significantly less likely to be Dayton plans.

The results in this section strongly support the hypothesis that Dayton plans had failure rates that were significantly higher, both statistically and economically. In the next section, I dig deeper into the mechanism driving this result by comparing the quality of assets (via lending rates) and the incentives to use the institution (via returns and withdrawal penalties). Before proceeding, in the remainder of this section I present a series of robustness checks of this result by considering alternative definitions of Dayton plans using observable shares of investment certificates and withdrawable shares, investigating the timing of failure, and finally by including Federalization as a potential outcome. The result that Dayton plans fail at significantly rates is robust to each of these tests.

Robustness Checks

Alternative Measure of Dayton Plan

The fact that many institutions over time issued both withdrawable shares and investment certificates hints that the definition used may have been more fluid than in the nineteenth century. It is possible that many Dayton plans by 1927 were more likely to be categorized as permanent plans if the state board did not accurately update its categorization. might bias the estimate of β in equation 3.1 if the commissioner's office was more likely to review and update vulnerable institutions.

It is therefore useful to consider a more general definition of a Dayton plan by considering only the observed liability structure of the balance sheet. Because B&L's reported withdraw-

[29] The number of loans comes from the archival data from the CSA. The ratio is relative to the total number of members available from the publicly available annual reports.

Table 3.6: Comparison of Dayton Measures

Original \ Liabilities	Permanent	Dayton	Total
Permanent	24	13	37
Dayton	31	95	126
Total	55	108	163

"Original" refers to the classification of Dayton plan according to the annual reports. "Liabilities" measure refers to classifying institutions based on whether shares or investment certificates comprised a larger share of liabilities. **Source**: Building and Loan Commissioner of the State of California (1927)

able shares separately from investment certificates, it is possible to use this information only to code the plan type. I calculate a dummy variable $Dayton_i^L$ equal to one if the outstanding value of investment certificates is larger than the outstanding value of withdrawable shares. I call this the "liabilities" measure of the Dayton plan.

Table 3.6 compares this measure to the 1927 balance sheet indicators, and finds that while they are correlated, there are some differences. 24 plans that were originally coded as Permanent plans are also coded as permanent plans in this new measure. Similarly, 95 plans that were originally coded as Dayton plans remain Dayton plans.

There are some differences. thirteen plans that were originally permanent plans now become Dayton plans. Additionally, 31 plans that were originally Dayton plans now become Permanent plans. For example, the Railway Mutual Building and Loan Association in Los Angeles is officially listed as a Dayton plan. Its liabilities only include installment shares and paid-up and prepaid shares. The 1929 California civil code states that installment shares must be issued as either "serial" or "permanent" in form. One reason for this discrepancy is that being a Dayton plan is self reported. Permanent institutions that are, or wish to, transition from permanent to Dayton may have stated so prior to implementing such a change in the structure of their liabilities. Similarly, some Dayton plans may be listed as permanent plans if they have a sufficient number of outstanding series following their shift. Their annual report would not list them as a Dayton plan but would list the value of the outstanding series.

Table 3.7 replicates Table 3.5 using this alternative measure of Dayton plan. Column (1), which shows the univariate results, presents a point estimate of approximately 0.15. While lower, this point estimate continues to stay stable across the same specifications as earlier. In fact, in my preferred specification, the point estimate is extremely similar when using this alternative measure. While this measure predicts a slightly lower probability of default, the overall pattern is consistent with the hypothesis that plans that relied more heavily on investment certificates, and likely to be Dayton plans, were more likely to fail. Investment certificates, as discussed earlier, were more likely to be associated with Dayton plans and were less likely to have withdrawal penalties.

Table 3.7: Probability of Failure using Alternative Measure of Dayton Plan

	Failure	Failure	Failure	Failure	Failure	Failure
Dayton (Liabilities)	0.188***	0.195***	0.189***	0.178**	0.144	0.177**
	(0.0682)	(0.0714)	(0.0710)	(0.0692)	(0.117)	(0.0721)
Log Assets		-0.0143				-0.0213
		(0.0276)				(0.0591)
Log Members			-0.00107			0.0167
			(0.0281)			(0.0613)
Share Cash				0.00715		0.00539
				(0.00785)		(0.00846)
Constant	0.164***	0.348	0.170	0.138**	0.250***	0.320
	(0.0502)	(0.353)	(0.175)	(0.0568)	(0.0945)	(0.468)
N	163	163	163	163	115	163
R-Squared	0.04	0.04	0.04	0.04	0.23	0.07
City FE	N	N	N	N	Y	N
Large City FE	N	N	N	N	N	Y

This table presents results from estimating Equation (3.1): $Failure_i = \alpha + \beta Dayton_i + \Gamma X_i + \varepsilon_i$. The Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. City includes Los Angeles, Oakland, and San Francisco. Dayton is a dummy equal to one if the Building and Loan Association has mostly investment certificates. **Source**: Building and Loan Commissioner of the State of California (1927,1935)

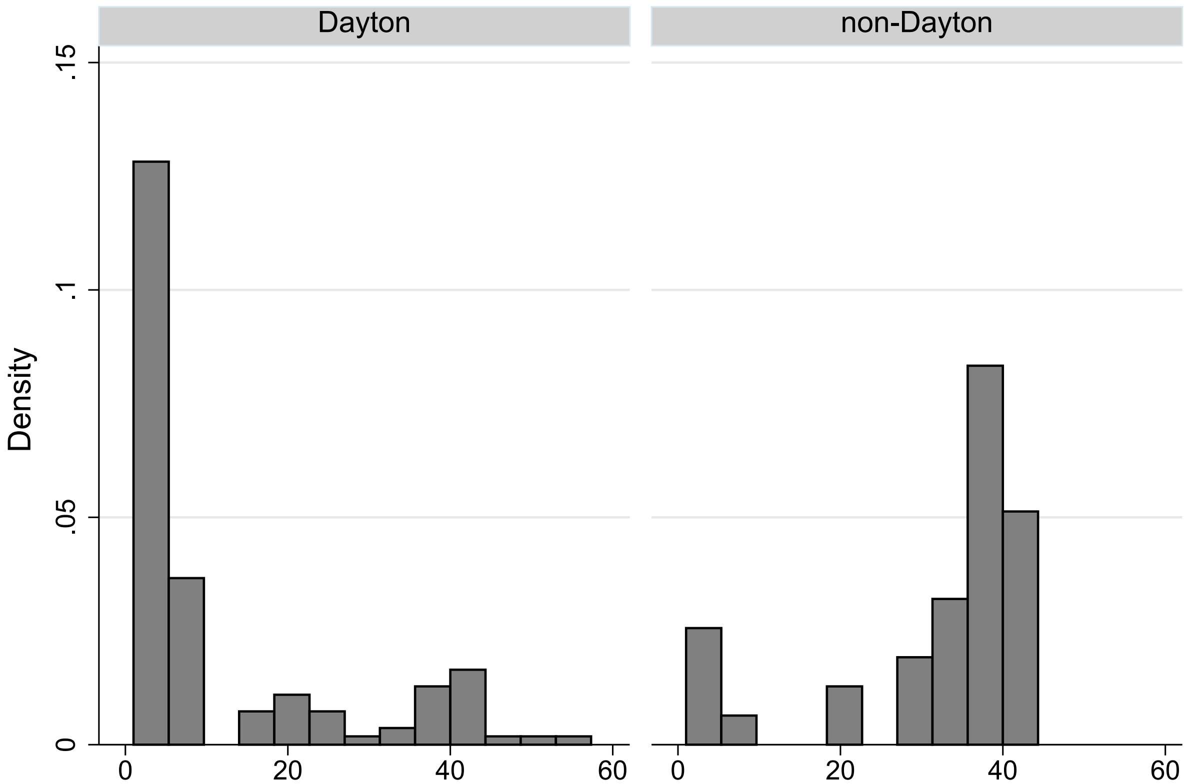
$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$. Robust Standard Errors in Parentheses.

Age as a Predictor of Failure

In this subsection I investigate how including B&L age as a predictor of failure affects the benchmark estimates. The balance table in the main text suggests that the most important determinant of plan type is Age. Figure 3.7 graphs a histogram of the age of B&L's by type. As can be seen, many more Dayton plans were younger. This is not surprising, as the concept of the Dayton B&L was not invented until the late 1900s.

I include B&L age as a control in the benchmark regression, with results presented in Table 3.8. The first column of this regression table repeats results from Table 3.5 that Dayton plans had higher failure rates on the order of 22.5 percentage points. The second column repeats this regression including only age as a predictor. The statistically significant and negative coefficient suggests that, for example, a twenty year old financial institution (the average value) had lower failure rates on the order of 10 percentage points compared with a newborn. Including both Age and Dayton in column three results in both coefficients falling magnitude and both losing their significance. This is the result of the limited number

Figure 3.7: Age Distribution by Type



Source: Building and Loan Commissioner of the State of California

of observations and the high correlation between the two limiting precision. However, the coefficient estimate remains within the one standard deviation confidence interval.

3.5 Costs, Returns, and Lending Rates

The previous section argued that Dayton B&L's failed at a higher rate during the Great Depression. In this section, I investigate why these high failure rates occurred. I compare withdrawal penalties, costs, returns, and lending rates between Dayton and permanent B&L's in California. The picture that emerges is one in which permanent B&L's used higher withdrawal penalties to attract less flighty members, or those who ex-ante were less likely to need to access their funds. They were able to attract such members by offering slightly higher returns on average. Importantly, there is no significant difference in lending rates across the two types.

I begin by comparing withdrawal penalties for shares and securities across B&L types.

Table 3.8: Probability of Failure including Age as a Predictor

	Failure	Failure	Failure
Dayton Plan	0.172**		0.111
	(0.0769)		(0.104)
Age		-0.00469*	-0.00288
		(0.00263)	(0.00337)
Log Assets	-0.0116	0.0198	0.00947
	(0.0581)	(0.0618)	(0.0610)
Log Members	0.0116	0.00355	0.00199
	(0.0623)	(0.0632)	(0.0623)
Share Cash	0.00565	0.00492	0.00444
	(0.00867)	(0.00894)	(0.00885)
Constant	0.208	0.0594	0.0925
	(0.463)	(0.477)	(0.473)
N	163	162	162
R-Squared	0.06	0.06	0.07

This table presents results from estimating Equation (3.1): $Failure_i = \alpha + \beta Dayton_i + \Gamma X_i + \varepsilon_i$. Age calculated as number of years open as of 1927. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. City includes Los Angeles, Oakland, and San Francisco. Source: Building and Loan Commissioner of the State of California (1927,1935)

$*\ p < 0.1$, $**\ p < 0.05$, $***\ p < 0.01$. Robust Standard Errors in Parentheses.

Clark and Chase (1927) discuss withdrawal fees across the types of institutions noting that "[a]ssociations using the Dayton plan ... customarily repay to withdrawing members the full book value of their investment." On the other hand, permanent plans can utilize one of many different version of withdrawal penalties. Clark and Chase (1927) discuss how fees varied substantially from state to state. Some states reduced earnings upon withdrawal. Others retained a portion of the principal as a fee.

The 1927 annual report from the Building and Loan Commissioner of the State of California specifically mentions withdrawals in the first page. It begins by noting that "many associations in the past have advertised that money might be withdrawn at will by the investor, and the public has come to expect it." This suggests that the public has a right to withdraw on demand, with little to no penalty. The commissioner also noted that agents who sold stock and certificates in B&L's "do not properly explain the withdrawal fee to the investors." As complaints rose in number, the commission issued an order that upon sale of certificates or stock to a new member, the association must "inform the investor of the

Table 3.9: Percentage of Associations Penalizing Withdrawals

Type	Percentage	N
Dayton	56%	126
Permanent	92%	37
Total	64%	163

Source: Building and Loan Commissioner of the State of California (1927)

nature of his contract, and specifically call his attention to the fact that the membership fee is not returnable." That withdrawal fees were important enough to be mentioned in the first page of the 1927 annual report suggests that these fees were salient for this time period.

I am interested in comparing the withdrawal penalties across types in California. Unfortunately, withdrawal penalties were not explicitly listed in the 1927 balance sheets, making it difficult to compare fees at Dayton with those at permanent institutions. Under some assumptions I can estimate withdrawal penalties. Permanent plans explicitly listed the withdrawal value for each share series.[30] If this withdrawal value was less than the listed book value, then I consider this a withdrawal penalty under the definition by Clark and Chase (1927) [31,32] In no case is the total withdrawal value less than dues, so the penalty is on the returns rather than the principal itself. For Dayton plans, withdrawal values were either "Full Book Value" or "Dues plus Profits". The 1891 Annual Report in California found that the average amount of profits paid out was only 50% of the total accrued.[33] I treat Dues plus Profits as a withdrawal penalty.

Table 3.9 shows the breakdown of withdrawal penalties under this categorization scheme. Only 57% of Dayton Plans penalized withdrawals according to the 1927 balance sheets, compared with 94% of permanent plans. To emphasize again, withdrawal penalties here refer to withdrawal value being less than book value. Fees almost surely varied across institutions. However, the unconditional probability of getting book value for withdrawals was higher at Dayton plans.

I next analyze costs by comparing dues by type. Dues are what is owed at each meeting for forced savings plans, and so represent the timing of forced savings when B&L's have such a policy in force. The traditional Dayton plan would not have any dues listed, but

[30]Figure 3.5 shows an example. In the last column on the bottom we see the withdrawal value.

[31]The appendix to Clark and Chase (1927) describes withdrawal fees as "Deductions from book value when shares are withdrawn before maturity."

[32]In five cases, withdrawal values for permanent plans were listed as dues plus profits. I consider this a penalty because, relative to book value, profits were more variable and were paid out only on specific dates. Thus, one would have to hold their savings in the institution until at least those dates to make a return. This contrasts with book value, which would not be subject to dividend dates.

[33]One concern is whether individuals could simply sell their shares at book value on a secondary market to capture more of the withdrawal value of profits. Rose (2014b) presents evidence from New Jersey that while secondary markets for B&L shares existed, shares sold for steep losses.

Table 3.10: The Costs of Membership by Type

	Dues per Share	Shares per Member	Cost
Dayton	0.62	7.76	4.83
Permanent	0.89	16.11	12.94
Observations	163	163	163
Difference	-0.28	-8.34	-8.12
t-stat	-7.30	-2.77	-4.93

Robust t-stats tests significance of the coefficient β in the regression $y_i = \alpha_i + \beta DAYTON_i + \varepsilon_i$. **Source**: Building and Loan Commissioner of the State of California (1927)

in California it was possible to have forced savings plans be in force even for investment certificates. For my purposes, I am interested in whether these dues were different between Dayton and permanent B&L's. If the dues structure is different across type, then this means that the forced savings plan for Dayton plans was less restrictive, and had a lower cost.

Dayton plans listed the dues per share per month, and this appeared to be the same amount for all members.[34] Permanent plans listed dues per share for each series.[35] I compare dues directly in the first column of Table 3.10. Dues per share for Dayton plans were on average 60 cents compared with around 90 cents for permanent plans. This difference of around 30 cents is statistically significant.

Comparing only dues per share leaves out the fact that members of permanent B&L's may hold fewer shares in total. This would mean that the *total* amount of dues paid could be the same across institutions. To account for this possibility, I examine the number of shares per member, under the assumption that all shares had a forced savings plan. Column (2) of Table 3.10 shows that permanent B&L's had shares per member that were nearly twice as high as for Dayton B&L's. Again, this difference is statistically significant. In column (3), I show that the total cost, measured as dues per member (or the product of columns 1 and 2) is around three times higher for permanent institutions compared with Dayton institutions.[36] Using this definition of cost, it is the case that the amount paid per member is higher at permanent plans compared with Dayton plans.

Having presented evidence that access costs were higher at permanent B&L's, I next consider returns to members. Specifically, for both shares and certificates, which type of institution provided a better average return on investment? The detailed statements in the archives provide information unavailable in the publicly available reports that help to answer this question. In particular, I use the simple average of reported returns to investment certificates and shares across the two types in 1931.

[34] When I use the term "per share," I am referring to the amount per share or per certificate, depending on what the association uses.

[35] See Figure 3.5 for an example. For these permanent plans, each series could theoretically have different costs. In practice this was not the case, according to the balance sheets.

[36] These results are robust to the inclusion of the same controls as in Table 3.5.

The first two columns of Table 3.11 shows regression results of the observed investor and lending rates on a dummy variable equal to one if an institution is listed as a Dayton plan. The first column shows the univariate result for investor returns. Dayton plans were associated with payout rates that were lower by around 38 basis points. This result is robust to including a dummy for whether a B&L is located in a "big city" (Los Angeles, San Francisco, or Oakland) and previous measures of size and liquidity. One may wonder whether or not the difference is economically meaningful. A difference in borrowing rates of around 40 basis points may not seem large. However, interest rates around this time were around 6 percent with a standard deviation of 60 basis points, so the 40 basis point difference is around two-thirds of a standard deviation.

One concern with higher average lending rates representing offering higher returns to account for withdrawal penalties may be if the quality of loans at permanent institutions is worse. It could be the case that such high returns signify monitoring by members who understand that the loan portfolio of the B&L is riskier. While Dayton and permanent B&L's had similar shares of loans as a percentage of total assets, the quality could differ. To test this, columns 3 and 4 examine the differences in lending rates. There is no statistically significant difference across institutions. If anything, lending rates were higher at Dayton institutions.[37] I take this as evidence that permanent plans used higher returns to justify higher withdrawal fees rather than to account for riskiness of the loan portfolio.

It is important to reiterate that all lending rates and returns are as of 1931 due to data availability. The regulatory landscape during the Great Depression significantly changed due to the passage of the Building and Loan Act in 1931, which made data collection a priority. Using 1931 excludes B&L's that failed in the late 1920s, many of which were Dayton plans. One concern is that the Dayton plans that failed had offered high interest rates on investment certificates that they were unable to pay out, and thus failed. Unfortunately, given the data restrictions, I cannot exclude this as a possible explanation.

Finally, I present evidence zooming in on the potential liquidity needs of members. In 1930, the profit and loss accounts on the annual statements included various measures of fees. I define as fees any line item that uses the words "fines" or "fees." I then calculate the sum of all fees and divide by total receipts. Figure 3.8 plots the distribution of fee shares for Dayton and Permanent institutions. As can be seen, permanent B&Ls were less likely to have receipts coming from fees compared with Dayton B&Ls.[38] I view this as evidence that households who were members of Dayton institutions were more likely to be willing to pay the fees required in order to withdraw early.

[37] Dayton plans eliminated the premium associated with borrowing. This premium meant that the gross value of the loan was reduced by the amount the borrower was willing to bid. The point estimate in Columns 3 and 4 show that lending rates were (if anything) *higher* for Dayton plans. I interpret this result as potential borrowers (and institutions) internalizing the premium, reflecting it in the lending rate rather than the net amount borrowed.

[38] A regression of the fees share on the Dayton dummy yields a point estimate of around .01 with a t-stat of around 4 that is robust to the inclusion of the same controls as above.

Table 3.11: Rate Regressions

	Return	Return	Lending	Lending
Dayton Plan	-0.378***	-0.370***	0.396	0.483
	(0.107)	(0.116)	(0.318)	(0.311)
City		-0.180		-0.113
		(0.139)		(0.172)
Log Assets		0.0391		-0.187***
		(0.0627)		(0.0625)
Share Cash		-0.00902		-0.0197
		(0.00681)		(0.0158)
Constant	6.241***	5.817***	7.683***	10.18***
	(0.0757)	(0.806)	(0.309)	(1.021)
N	95	95	96	96
R-Squared	0.06	0.10	0.03	0.09

This table presents results from estimating the equation $y_i = \alpha + \beta Dayton_i + \Gamma X_i + \varepsilon_i$. **Source**: Building and Loan Commissioner of the State of California (1927), Inventory of the Dept. of Savings and Loan Records. Records of the Los Angeles Office. F3739:425-450. California State Archives

$* \ p < 0.1$ $** \ p < 0.05$ $*** \ p < 0.01$. Robust standard errors in parentheses

3.6 Real Effects

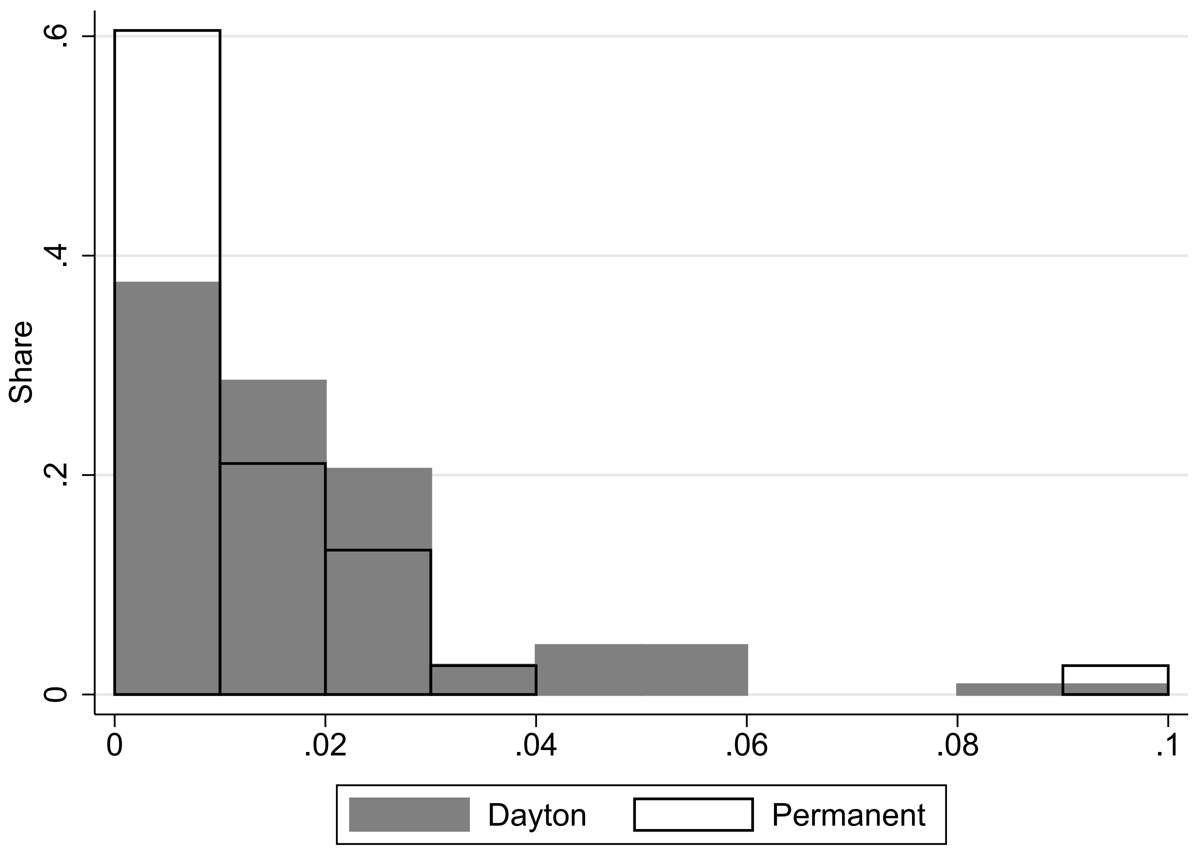
I have argued in this paper that the structure of Building and Loan institutions was an important determinant of their failure. The B&L associations in California with more "flighty" liabilities, the Dayton plans, were more likely to fail compared with those with more restrictive liability structures, the Permanent plans.

Did having higher rates of failure due to having more Dayton plans have effects on the real economy? One difficulty with answering this question is the dearth of data on real economic outcomes at a granular level in California during this time period. To make progress, I use the Agricultural Census, which at a quinquennial frequency reports important statistics related to farm production and sales at the county level. My variable of interest to represent the real economy is the percentage change in the real average (per farm) value of farmland.[39]

I am interested in the effect of the presence of Dayton B&L associations on the average real value of farmland. One may be concerned that relating the change in the value of farmland to failures themselves may suffer from reverse causality if B&L associations were more likely to fail in areas with worse larger local economic crises. I therefore construct the

[39] I deflate the 1935 value using the Historical Statistics of the United States.

Figure 3.8: Distribution of Fees by Type in 1930



Data is for 1930. Drops all institutions for whom fees are above 10% of total receipts. All dropped institutions are Dayton plans. **Source**: Building and Loan Commissioner of the State of California (1930)

share of Dayton B&L associations. A key identifying assumption in this paper is that the decision to be a Dayton plan is orthogonal to economic conditions. Relating the percentage change in the value of farmland to the share of Dayton plans should therefore alleviate any reverse causality concerns.

Figure 3.9 plots, at the county level, the share of Dayton building and loan associations against the real change in the average value of farmland.[40] There is a clear negative relationship between having more Dayton plans and having a larger decline in farmland value.

Column (1) of Table 3.12 presents the coefficient estimate from regressing the log change in the average value of farmland on the share of Dayton B&L associations. The coefficient

[40] I combine San Francisco with Alameda county. San Francisco had only 17 farms in 1925 and 138 farms in 1935

Table 3.12: Farmland Values and Dayton B&L Presence

	Dependent variable:		
	Log Change in Average Farmland Value		
	(1)	(2)	(3)
Dayton Share	−0.071	−0.180*	−0.074
	(0.073)	(0.096)	(0.067)
Constant	−0.087	0.020	−0.131**
	(0.055)	(0.065)	(0.051)
Sample	All	>1	All
Weights	None	None	Farms
Observations	25	18	25
R^2	0.040	0.156	0.048
Adjusted R^2	−0.001	0.103	0.007
Residual Std. Error	0.140	0.137	7.925

This table presents regression results from estimating the effect of changes in real average farmland value on the share of Dayton plan building and loan associations at the county level. $\Delta y_c = \alpha + \beta Dayton\_Share_c + \varepsilon_c$. Δy_c represents the percentage change in real average farmland value for county c. $Dayton\_Share_c$ represents the share of Dayton building and loan associations for county c.

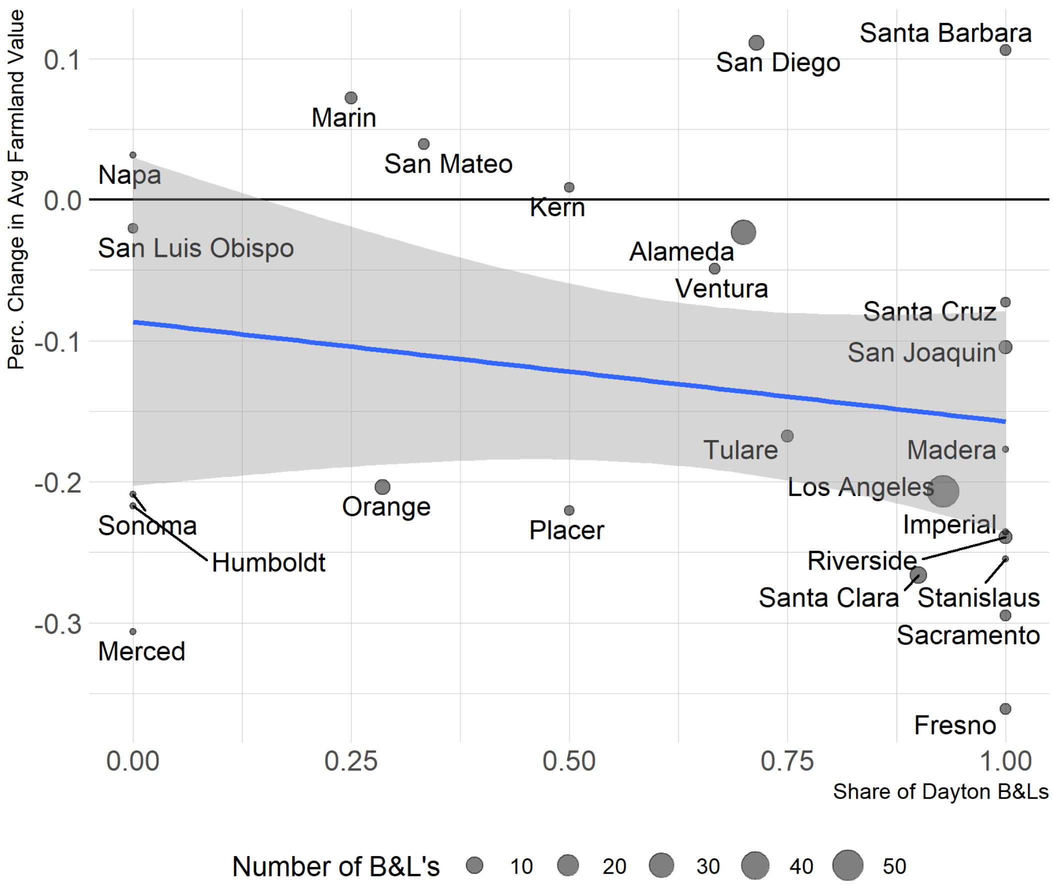
estimate of approximately −0.07 (SE: 0.07) suggests that going from no Dayton B&L's to *all* Dayton B&L's results in average farmland values declining by an addition 7%. Column (2) focuses on the 18 counties for which there were more than 1 B&L and finds the coefficient rise to approximately 0.18 (SE: 0.10). Column (3) weights by the number of farms, and the coefficient estimate is little changed from that in Column (1).

The evidence in this section suggests real effects, in additional to financial instability, of having more Dayton B&L's. The evidence in this section is also consistent with Rajan and Ramcharan (2015), who find that areas with a higher credit boom in the 1920s saw a larger decline in land prices during the Great Depression.

3.7 Conclusion

In this paper, I study the role that depositor quality plays in causing a financial institution's propensity to experience distress and fail. To do so, I leverage institutional differences across Building and Loan Associations in California during the Great Depression. These institutions offer a unique laboratory to investigate depositor quality because, due to legal restrictions

Figure 3.9: Farmland Values and Dayton B&L Presence



This figure presents the relationship between the share of Dayton building and loan associations and the percentage change in average farmland value. **Source**: Agricultural Census, Building and Loan Commissioner of the State of California (1935) and cultural history, their assets were almost completely in real estate loans with institutions offering similar lending rates. Their liabilities on the other hand differed. Dayton plans had low withdrawal penalties and less rigorous savings plans. Permanent plans had high returns with more regular forced savings plans.

To summarize, there are three related and important results. First, Dayton plans had a probability of failure during the Great Depression around 20 percentage points higher than permanent plans. Second, Dayton plans were significantly less likely to penalize withdrawals compared with permanent plans. Finally, Permanent plans had lower returns to members compared with permanent plans.

Taken together, these three results imply that the access costs and returns of permanent B&L's was chosen (by the B&L) to higher than that of Dayton B&L's. This was done to attract higher quality and less flighty depositors, who were less likely to need liquidity in the event of a bad shock. Since both types had similar asset structures and lending rates, it is unlikely that B&L's were chosen based on the risk profile of the potential member. Instead, these higher withdrawal penalties were justified by offering higher returns to potential members.

These results motivate a model with the following characteristics. First, there is heterogeneity across households in the flightiness of individual members. This flightiness refers specifically to the probability of needing their saved wealth in the event of a bad shock. Second, there exist two types of savings institutions. The difference between the two institutions is that one that offers high lending rates at the cost of having high penalties for early withdrawal. A conventional banking model in the spirit of Diamond and Dybvig (1983), which only one type of institution, cannot capture this mechanism. Future work will explore this idea more formally.

Chapter 4

Unemployment Effects of Stay-at-Home Orders: Evidence from High Frequency Claims Data[1]

Joint with ChaeWon Baek, Peter McCrory, and Preston Mui

[1]Except for the first paragraph and a part of the conclusion, this chapter is a reprint (with permission) of "Unemployment Effects of Stay-at-Home Orders: Evidence from High Frequency Claims Data," coauthored with ChaeWon Baek, Peter McCrory, and Preston Mui. It has been accepted for publication in the Review of Economics and Statistics, which retains first publication credit, ©2020 by the President and Fellows of Harvard College and the Massachusetts Institute of Technology.

4.1 Introduction

The previous two chapters of this dissertation are focused on the relationship between financial institutions and the real economy. This chapter uses the empirical tools of the previous chapters to focus on the economic effects of public health policy in the early stages of the COVID-19 pandemic. This chapter also uses some of the theoretical intuition of previous chapters as a foundation for building a theoretical model to study the economic effects of COVID-19.

To limit the spread and severity of the COVID-19 pandemic, officials around the globe turned to non-pharmaceutical interventions (NPIs), such as shutting down schools, restricting economic activities to those deemed essential, and requiring people to remain at home whenever possible. In mid-March 2020, Ferguson et al. (2020) issued a report projecting that, in the absence of the effective implementation of NPI mitigation strategies, more than 2 million Americans were potentially at risk of death from the COVID-19 respiratory disease, with many more facing uncertain medical complications in the near- and long-run.

Soon after, state and local officials in the United States began announcing Stay-at-Home (SAH) orders, which restricted residents from leaving their homes except for essential activities. The earliest SAH order was implemented in the Bay Area, California on March 16th, 2020. Three days later, the governor of California issued a state-wide SAH order. By March 24th, more than 50% of the U.S. population was under a SAH order (see Figure 4.1). By April 4th, 95% of the U.S. population was under a state or local SAH order, likely substantially reducing the supply of and demand for locally produced goods and services.

At the same time, there was mounting evidence of substantial disruption to labor markets in the United States. For the week ending March 21st, 2020, the Department of Labor (DOL) reported that more than 3.3 million individuals filed for unemployment benefits.[2] In the subsequent weeks ending March 28th and April 4th, initial claims for unemployment once again hit unprecedented highs of more than 6.9 million claims and 6.7 million claims, respectively. Taken together, total unemployment insurance (UI) claims over this three week period was almost 17 million.

How much of the initially observed increase in UI claims was attributable to the newly implemented SAH orders? This is not a straightforward question to answer since the increase in unemployment claims could plausibly be attributed to a multitude of factors other than SAH orders that occurred at the same time. For example, consumer and business sentiment both declined and economic uncertainty rose as the pandemic worsened. One stark example of this economic uncertainty was the swift drop in the value of the S&P 500 stock market index, which lost roughly 30% of its value between February 20 and March 16, the first day a SAH order was announced in the United States.

In this paper, we disentangle the local effects of SAH orders from the broader economic disruption brought on by the COVID-19 pandemic and other factors affecting all states

[2]For comparison, in this week one year prior, there were just over 200 thousand initial claims for unemployment insurance. This was also the first time since the DOL began issuing these reports that the flow into unemployment insurance exceeded the number of individuals with continuing claims.

equally. We do so by providing evidence of a direct causal link between the implementation of SAH orders and the observed increase in UI claims. To the best of our knowledge, this paper is the first systematic study of the causal link between SAH orders and UI claims in the United States. This is our main contribution.

We show that the decentralized implementation of SAH orders across the U.S. induced high-frequency regional variation as to when and to what degree local economies were subject to such orders. We leverage the cross-sectional variation in the length of time that states were exposed to such orders to estimate its effect on UI claims.[3,4]

We find that an additional week of exposure to SAH orders increased UI claims by approximately 1.9% of a state's employment level, relative to unexposed states. The effect is precisely estimated and robust to the inclusion of a battery of controls one might suspect are correlated with both local labor market disruption and SAH implementation, lending it a causal interpretation. The set of controls we consider include the severity of the local exposure to the coronavirus pandemic, state-level political economy factors, and each state's industry composition.

We use our cross-sectional estimate to calculate the implied aggregate effect of SAH orders on the number of new unemployment claims. This exercise yields an estimate of approximately 4 million UI claims attributable to SAH orders through April 4, comprising roughly 24% of total claims over the time period. We refer to this calculation as the relative-implied aggregate estimate of employment losses from SAH orders.

It is well known that cross-sectional research designs, such as the one employed in our paper, hold constant general equilibrium effects as well as other aggregate factors. Simply scaling up our cross-sectional estimate may therefore give a biased impression of the aggregate effect of SAH orders on UI claims in the United States.

To understand the nature of these general equilibrium forces, we present a simplified currency union model to provide conditions under which the relative-implied estimate represents an upper or lower bound on aggregate employment losses. When the SAH shock is viewed primarily as a technology shock—and in the empirically relevant case with flexible prices—our estimate represents an *upper bound* on the aggregate effect. However, when SAH orders are treated as a local demand shock, the interpretation is a bit more subtle and depends upon the persistence of the shock and degree of price flexibility. Across all combinations of price rigidity, persistence and nature of the SAH shock, we find that our back-of-the-envelope estimate, at most, understates aggregate employment losses by a factor of approximately two. With sticky prices and a zero-persistence shock, the relative-implied estimate associated with the SAH-induced local demand shock understates aggregate employment losses by 12%.

[3] Our variable of interest pertains to the *government* implementation of SAH orders. Our design does not aim to capture the effects of, for example, social distancing behaviors that may have taken place in the absence of a government order.

[4] In this paper, we principally focus on UI claims for three reasons: (1) UI claims are among the highest frequency indicators of real economic activity—especially as it relates to the labor market; (2) These data are consistently reported at a subnational level; (3) The data are publicly and readily available.

Taken together, the model results then imply a (non-binding) *upper bound* on UI claims from SAH orders through April 4, 2020 of approximately 8 million. Thus, relative to the total rise of around 16.5 million, at most around 50% of the total rise in UI claims over this period can be attributed to SAH orders.

Finally, we document the robustness of our empirical results by considering an alternative research design relying upon county-level data. Specifically, we estimate county-level specifications which allow us to control for unobserved state-level factors, such as each state's ability to respond to and process unprecedented numbers of unemployment claims. We find similar results in this case. Appendix C.1 documents the robustness of our headline result to alternative research designs and empirical specifications.

Related Literature

Our paper relates most obviously to the rapidly growing economic literature studying the COVID-19 pandemic, its economic implications, and the policies used to address the simultaneous public health and economic crises. The epidemiology literature has focused on the health effects of NPIs. In a notable study, Hsiang et al. (2020) estimate that, in six major countries, NPI interventions prevented or delayed over 62 million COVID-19 cases.[5] Our focus is, instead, on the macroeconomic effects of the coronavirus pandemic. Broadly speaking, the macroeconomic literature on COVID-19 has split into two distinct yet highly related strands. Here we provide a representative, albeit not exhaustive, review.

The first strand of research focuses on the relationship between macroeconomic activity, policy, and the unfolding pandemic. Gourinchas (2020) and Atkeson (2020) are early summaries of how the public health crisis and associated policy interventions interact with the economy. Both emphasize the trade-off between flattening the pandemic curve while steepening the recession curve. Similarly, Faria–e–Castro (2020) studies the effect of a pandemic-like event in a quantitative DSGE model in order to assess the economic damage associated with the pandemic along with the fiscal interventions employed in the U.S. to attempt to flatten the recession curve. Eichenbaum et al. (2020) derive an extension of the standard Susceptible-Infected-Recovered (SIR) epidemiological model to incorporate macroeconomic effects, formalizing the relationship between the flattening the pandemic curve and amplifying the recession curve. We view our paper as providing causally identified, empirical support for the claim that flattening the pandemic curve requires steepening the recession curve.

The second strand of research uses high-frequency data to understand the economic fallout wrought by the COVID-19 pandemic. Our paper aligns more closely with this strand of the literature. Baker et al. (2020) show that economic uncertainty measured by stock market volatility, newspaper-based economic uncertainty, and subjective uncertainty in business expectation surveys rose sharply as the pandemic worsened. Lewis et al. (2020) derive a weekly national economic activity index and show that the COVID-19 outbreak had already

[5] The six countries are China, South Korea, Italy, Iran, France, and the United States.

had a substantial negative effect on the United States economy in the early weeks of the crisis. Hassan et al. (2020) use firm earnings calls to quantify the risks to firms as a result of the COVID-19 crisis. Coibion et al. (2020b) examine how the pandemic affected the labor market in general. Using a repeated large-scale household survey, they show that by April 6th, 2020, 20 millions jobs were lost and the labor market participation rate had fallen sharply.

Our paper also relates to empirical work studying the effect of lockdown policies more specifically. For example, Hartl et al. (2020) study the effect of lockdowns in Germany on the spread of the COVID-19. In contrast to these papers, we use geographic variation to understand the effect of COVID-19 on economic activity. In that respect, our paper can be thought of a high frequency version of Correia et al. (2020), who find that over the long term, NPI policies implemented in response to the 1918 Influenza Pandemic ultimately resulted in faster growth during the recovery following the pandemic.

Other papers employing geographic variation in NPI implementation to understand their contribution to the economic fallout associated with COVID-19 pandemic include the following: Kong and Prinz (2020) use high-frequency Google search data as a proxy for UI claim activity to study the labor market effects of various NPIs; Coibion et al. (2020a) study the effect of lockdowns on employment and macroeconomic expectations; Kahn et al. (2020) document broad declines job market openings in mid-March prior to implementation of SAH orders; Kudlyak and Wolcott (2020) provide evidence that the bulk of UI claims over this period were classified as temporary, suggesting that the long-run costs of lockdowns may be mitigated, so long as worker-firm matches persist until the recovery; and, Sauvagnat et al. (2020) document regional lockdowns depressed the market value of affected firms.

A closely related paper is Friedson et al. (2020), which uses the state-wide SAH order implementation in California along with high frequency data on confirmed COVID-19 cases and deaths to estimate the effect of this policy on flattening the pandemic curve. Unlike our approach, however, the authors in this paper use a synthetic control research design to identify the causal effects on this policy. The authors argue that the SAH order in California reduced the number of cases by 150K over three weeks; the authors perform a back-of-the-envelope calculation to calculate roughly 2-4 jobs lost over a three week period in California per case saved. In contrast to Friedson et al. (2020), we are able to directly estimate the causal effect of SAH orders on UI claims. Taking their benchmark number of cases saved over three weeks, we find that a SAH order implemented over three weeks in California would increase UI claims by 6.4 per case saved.

4.2 Data

State-Level Stay-at-Home Exposure

We construct a county-level dataset of SAH order implementation based on reporting by the *New York Times*. On March 24th, 2020, the *New York Times* began tracking all cities, counties, and states in the United States that had issued SAH orders and the dates that those orders became effective.[6]

We calculate the number of weeks that each county c in the U.S. had been under a SAH order between day $t-k$ and day t (and counting the day that the policy became effective).[7] We denote this variable with $SAH_{c,s,t,t-k}$, where s indicates the state in which the county is located. Except when explicitly stated, we drop the $t-k$ subscript and set k to be large enough so that this variable records the total number of weeks of SAH implementation in county c through time t.

As an example, consider Alameda County, California. Alameda County was among the first counties to be under a SAH order when one was issued on March 16th, 2020. Here, $SAH_{Alameda,CA,Mar.28} = 13/7$, as Alameda County had been under Stay-at-Home policies for thirteen days. Los Angeles County, California, on the other hand, did not issue a SAH order before the State of California did so. We therefore set $SAH_{LosAngeles,CA,Mar.28} = 10/7$ since the state-wide order was issued in California on March 19th, 2020.

The previous two examples illustrate how, in some instances, county officials took action before the state in which they were located did. Unfortunately, however, our main outcome of interest, new unemployment claims, is available to us only at the state-level.[8]

To aggregate county-level SAH orders to the state level, we construct a state-level measure of the duration of exposure to SAH orders by taking an employment-weighted average across counties in a given state. Formally, we calculate:

$$SAH_{s,t} \equiv \sum_{c \in s} \frac{Emp_{c,s}}{Emp_s} \times SAH_{c,s,t} \tag{4.1}$$

Employment for each county is the average level of employment in 2018 as reported by the BLS in the Quarterly Census of Employment and Wages (QCEW).[9] One can think of $SAH_{s,t}$ as the average number of weeks a worker in state s was subject to SAH orders by time t.

Figure 4.2 reports $SAH_{s,Apr.4}$ for each state in the U.S. and the District of Columbia. California had the highest exposure to SAH orders at 2.5, indicating that Californian workers

[6]The most recent version of this page is available at https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html. In a few instances, states implemented the closure of non-essential businesses prior to broader SAH orders that affected businesses and households alike. We show that our results are qualitatively and quantitatively robust to accounting for this occasional discrepancy in timing in Appendix C.1. We choose to rely upon the *New York Times* reporting since it provides sub-state variation. Over time, the *New York Times* stopped separately reporting sub-state orders when a state-wide SAH order was issued. We used the *Internet Archive* to verify the timing and location of SAH orders as reported in the *New York Times*.

[7]When a city implements a SAH order, we assign that date to all counties in which that city is located—unless of course the county had already issued a SAH order.

[8]While we lack sufficient data to estimate county-level effects on UI claims, in Section 4.6 we consider county-level regressions in which we estimate the March to April change in log employment and the unemployment rate using data published by the Bureau of Labor Statistics. We find quantitatively similar results even after conditioning on state-level fixed effects. In Appendix C.1 we use this county-level variation to study the impact of SAH orders on retail and workplace mobility, as measured by the Google mobility index.

[9]The annual averages by county in 2019 were, at the time of writing, not yet publicly available.

were on average subject to SAH orders for two and a half weeks. Conversely, five states (Arkansas, Iowa, Nebraska, Northa Dakota, and South Dakota) had no counties under SAH orders by April 4. The average value across all states of $SAH_{s,Apr.4}$ is 1.2.

Main Outcome Variable: State Initial Claims for Unemployment Insurance

Our main outcome of interest is initial unemployment insurance claims. Initial UI claims is among the highest-frequency real economic activity indicators available. As discussed in the introduction, initial claims for unemployment insurance for the week ending March 21st, 2020 were unprecedented, with more than 3 million workers claiming benefits. By the end of that week, very few states or counties had issued SAH orders. Figure 4.1 shows that by March 21st, only around 20% of the U.S. population was under such directives. This suggests that a substantial portion of the initial economic disruption associated with the COVID-19 crisis may have occurred in the absence of SAH orders.

Let $UI_{s,t}$ indicate new unemployment insurance claims for state s at time t and UI_{s,t_0,t_1} denote cumulative unemployment claims for state s from time t_0 to t_1. In our baseline specification, we consider the effect of SAH orders on cumulative weekly unemployment insurance claims by state from March 14th, 2020 to April 4th, 2020:

$$UI_{s,Mar.21,Apr.4} = UI_{s,Mar.21} + UI_{s,Mar.28} + UI_{s,Apr.4} \tag{4.2}$$

We then normalize this variable by employment for each state, as reported in the 2018 QCEW, to construct our outcome variable of interest:

$$\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} \tag{4.3}$$

Our choice of April 4th, 2020 as the end date for this regressions is driven by the observation that, by April 4th, 2020, approximately 95% of the U.S. population was under a SAH order. In Section 4.6, we consider 2-week and 4-week horizon specifications and find quantitatively similar results.

4.3 Empirical Specification

We now turn to our research design. Our main design is a state-level, cross-sectional regression:

$$\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s \tag{4.4}$$

where α is a constant, β_C is the coefficient on state-level exposure to SAH orders, X_s is a vector of controls with associated vector of coefficients Γ, and ϵ_s represents the error term in this equation.

To illustrate the motivation for our empirical design, in Figure 4.3 we compare the evolution of UI claims to state employment of "early adopters," defined as those states being in the top quartile of SAH exposure through April 4, 2020, to that of "late adopters," defined as those states being in the bottom quartile.[10] This figure provides *prima facie* graphical evidence of the main result of our paper: in the first few weeks, early adopters initially had a higher rise in unemployment claims relative to late adopters. By the week ending April 4th, 2020, the relative effect of adopting SAH orders early largely disappears, reflecting the fact that by this point approximately 95% of the U.S. population was under a SAH order, with most having been under the order for the full week ending April 4th.

This figure also suggests that SAH orders alone likely do not account for all of the rise in unemployment claims.[11] In the early weeks, late adopters also experienced historically unprecedented levels of UI claims even though early adopters had higher claims on average. For example, consider the week ending March 28. Here the difference between the median value of the two groups was approximately 1% of state employment; in that week, the median value of initial claims to employment for late adopters was roughly 3%, despite close to zero SAH exposure by this point. By April 4th, this difference almost completely disappears. Late adopters, who were under SAH orders for a much shorter period of time (or not at all, in some cases), converged to similar levels of unemployment claims relative to employment.

Confounding Factors

In order for our estimate $\hat{\beta}_C$ to have a causal interpretation, it must be the case that the timing of SAH orders implemented at the state and sub-state-level be orthogonal with unobserved factors affecting reported state-level UI claims.[12]

We provide further support for our causal interpretation by testing the magnitude and significance of the estimate $\hat{\beta}_C$ against the inclusion of three sets of important controls. The first set of controls considers the impact that the COVID-19 outbreak itself had on local labor markets. States that chose to implement SAH orders earlier may have done so simply because of the intensity, perceived or otherwise, of the local outbreak. In most macro-SIR

[10]The upper and lower edges of the boxes denote the interquartile range of each group, with the horizontal line denoting the median. As is standard, the "whiskers" denote the value representing 1.5 times the interquartile range boundaries.

[11]We thank an anonymous referee for pointing out that this could have the alternative interpretation that local SAH order implementation had substantial negative spillover effects on the rest of the country. See Section 4.5 for a model-driven discussion of such potential spillover effects between states.

[12]An additional reason for preferring April 4th is that over longer horizons, there is greater risk of omitted variable bias (i.e. $\mathbb{C}ov[\epsilon_s SAH_{s,Apr.4}] \neq 0$). A salient example is the rollout of the Paycheck Protection Program (PPP) on April 3rd. (The PPP was a central component of the CARES Act, a two trillion fiscal relief package signed into law on March 27, 2020. The PPP authorized $350 billion dollars in potentially forgivable SBA guaranteed loans.) This program provided forgivable loans to small businesses affected by the economic fallout of the pandemic, so long as those loans were used to retain workers. On the margin, PPP incentivizes firms to not lay off their workers, which would tend to lower UI claims for the week after April 4th. Depending upon how this interacts with the differential timing of SAH implementation, the bias could go in either direction.

models, a larger real outbreak would directly result in a larger drop in consumption due to a higher risk of contracting the virus associated with consumption activity (e.g. Eichenbaum et al., 2020). To account for this concern, we control for the number of excess deaths, as reported by the Centers for Disease Control and Prevention (CDC), relative to population. We also include the share of the population over 60, as this demographic was more at risk of serious health complications arising from contracting COVID-19.

Additionally, one may be concerned that consumers' perceptions of the outbreak differed from its actual severity. During this time period, the reported number of new confirmed cases was an important statistic reported by the media. This statistic, which suffers from differential testing capability and definitions across states, differs from the measure of excess deaths as it focuses on how local labor markets may have interpreted the severity of the outbreak.[13] We therefore also include the total confirmed cases relative to population.[14] Note that the severity of the outbreak would lead to an upward bias in our estimate $\hat{\beta}_C$ if states were more likely to enact SAH orders when the local outbreak was worse or perceived to have been worse, which may itself have led to labor market disruptions.[15]

The second set of controls we consider relates to the political economy of the state government. Some states may have had more generous social safety nets that led workers to separate from firms earlier than in states with less generous policies. Moreover, states with generous policies may also have been more likely to respond earlier to the pandemic, thereby generating bias. To account for this concern, we consider two political economy controls. First, we include the average UI replacement rate in 2019, as reported by the Department of Labor's Employment and Training Administration.[16] Second, we include the Republican vote share in the 2016 presidential election.[17] The first measure is designed to capture the generosity of the social safety net, while the latter is meant to capture political constraints on state and local officials to implement various public health NPIs.

Finally, our last set of controls is intended to address the concern that the timing of SAH implementation may be related to the sectoral composition within each state, and therefore the magnitude of job losses experienced by that state irrespective of SAH orders. To address this concern, we use a measure of predicted state-level UI claims as determined by industry composition within each state and the monthly change in jobs as reported in the national

[13]Evidence from Fetzer et al. (2020) suggests that the arrival of confirmed COVID-19 cases leads to a sharp rise in measures of economic anxiety, which would have an effect on real economic activity through the change in household and firm beliefs about the future state of the economy.

[14]We rely upon confirmed COVID-19 cases as compiled at the county-by-day frequency by USAFacts. USAFacts is a non-profit organization that compiles these data from publicly available sources, typically from daily reports issued by state and local officials. See https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/ for more details.

[15]Our controls for excess deaths and confirmed cases are taken as cumulative sums as of the end of the sample period, which is April 4th in the benchmark analysis. We experimented with using lagged values of these measures as pre-period controls, and they had no effect on the magnitude or significance of our coefficient of interest. These results are available upon request.

[16]See https://oui.doleta.gov/unemploy/ui_replacement_rates.asp for more details.

[17]As reported by the *New York Times* at https://www.nytimes.com/elections/2016/results/president.

jobs report in March by the BLS. These numbers are based on a survey reference period that concluded on March 14th, 2020—fortuitously for us, two days before any SAH order was announced. Specifically we construct a Bartik-style control:

$$B_s = \sum_i \Delta \ln Emp_{i,March} \times \omega_{i,s} \tag{4.5}$$

where $\Delta \ln Emp_{i,March}$ is the monthly percentage change in employment in industry i (3-digit NAICs) for the month of March. $\omega_{i,s}$ is the share of employment in industry i in the state, as reported in the QCEW for 2018.

We also control for the extent of work-at-home capacity at the state-level. Dingel and Neiman (2020) construct an index denoting the share of jobs that can be done at home by cities, industries, and countries. We construct a state-level index by taking an state employment-weighted average of the Dingel and Neiman (2020) industry-level (2-digit NAICS) work-at-home index. It may be the case that states with a higher capacity to work from home may have been willing to implement SAH orders earlier if the labor market disruption of such policies was perceived to be lower when more workers are able to work from home. If this index is correlated with the number of initial UI claims received by the state in the absence of implementing SAH orders, then failing to include this control would introduce bias.[18]

Causal interpretations aside, the cross-sectional framework is nevertheless constrained in only answering the following question: By how much did UI claims increase in a state that implemented SAH orders *relative* to a state that did not? The constant term absorbs, for example, the general equilibrium effects of stay-at-home orders which would affect all states within the U.S.—not just those implementing SAH orders. To the extent that other states' labor markets were affected in any way by the local imposition of SAH orders, then $\hat{\beta}_C$ will fail to capture the *entire* effect of such policies. We postpone discussion of the mapping between the relative effect of SAH orders and their aggregate effect until after presenting our cross-sectional results.

4.4 Results

Effects of SAH Orders on State-Level UI Claims

In Table 4.1, we present results from estimating Equation (4.4). Column (1) shows the univariate specification, with no controls. The point estimate of approximately 1.9% (SE: 0.67%) implies that a one-week increase in exposure to SAH orders raises the number of claims as a share of state employment by 1.9% relative to states that did not implement SAH orders. Figure 4.4 displays this result graphically. The bubbles are shaded according

[18] In unreported regressions, we study whether the effect of SAH orders differentially depends upon the value of the work-at-home index; we find no evidence that this is the case.

to the intensity of the confirmed COVID-19 cases per thousand people and the size of the bubbles are proportional to state population.

In Column (2), we control for the number of confirmed COVID-19 cases per one thousand people, excess deaths by state, and the share of state population over the age of 60. As discussed, these are intended to control for factors related to the pandemic that might simultaneously affect both the timing of SAH implementation and the severity of state labor market disruptions. The change in the coefficient is immaterial—economically and statistically. In Column (3) we control for political economy factors: the state's UI replacement rate in 2019 and the 2016 Trump vote share. Our estimate $\hat{\beta}_C$ falls only slightly to 1.8%. In Column (4) we include controls for each state's sectoral composition (and in turn its sensitivity to both the pandemic-induced crisis and timing of SAH implementation). Our point estimate is again largely unchanged.

Finally, in column (5), we select a parsimonious specification that captures dimensions of each set of controls. We control for confirmed cases, excess deaths, the UI replacement rate, and the WAH index (the only significant variable). In this specification, which is our preferred specification, the estimate of β_C is still 1.9%.[19,20]

Our results support the idea that policies that work to flatten the pandemic curve also imply a steepening of the recession curve (Gourinchas, 2020). To quantify this steepning of the recession curve, we use our point estimate of the relative effect on state-level UI claims of SAH orders to calculate a back-of-the-envelope estimate of the total implied number of UI claims between March 14 and April 4 attributable to SAH orders. We calculate the relative-implied estimate as follows:[21]

$$\text{Relative-Implied-Aggregate-Claims} = \sum_s \hat{\beta}_C \times SAH_{s,Apr.4} \times Emp_s \tag{4.6}$$

where s indexes a particular state. This is a back-of-the-envelope calculation as it simply scales up the cross-sectional coefficient $\hat{\beta}_C$ according to each state's SAH exposure through April 4, 2020 and each state's level of employment.

This back-of-the-envelope calculation yields an estimate of 4 million UI claims due to SAH orders through April 4. Ignoring cross-regional spillovers, this relative-implied estimate

[19] In the appendix, we consider three additional robustness exercises at the state-level. We alternate the horizon over which the model is estimated (2 and 4 weeks), estimate the model by weighted least squares, and re-estimate the model dropping one state at a time. The results are quantitatively and qualitatively similar.

[20] In unreported regressions, we find that, when including all regressors, $\hat{\beta}_C$ is somewhat attenuated—albeit statistically indistinguishable from our baseline estimate; however, this attenuation is largely driven by the parametric assumption of linearity on the share of votes for Trump in 2016, which places substantial leverage on Wyoming and West Virginia. Dropping these states from the full specification with all control variables yields a point estimate of 1.8% (SE: 0.75%). These regressions are available upon request.

[21] We use the terminology "relative-implied" because in the cross-section we are only able to identify effects of SAH orders relative to states not implementing SAH orders. We discuss this issue at greater length in Section 4.5.

suggests that approximately 24% of total claims through April 4, 2020 were attributable to such orders.

This calculation does not incorporate general equilibrium effects or spillovers that may have arisen as a result of local SAH implementation. As we discuss in Section 4.5, when the SAH order is interpreted as a local productivity shock, this represents an upper bound on aggregate employment losses; when, however, the SAH implementation is treated as a local demand shock, the analysis is a bit subtler. Yet, even in this case, we find that at most the relative-implied aggregate multiplier understates true employment aggregate employment losses by a factor of 2. Through the lens of the model, this provides an upper bound on total employment losses attributable SAH orders: 8 million UI claims through April 4, or approximately half of the overall spike in claims during the initial weeks of the economic crisis induced by the COVID-19 pandemic.

An alternative back-of-the-envelope calculation to assess the magnitude of our estimate is to instead focus the relative contribution of SAH orders in terms of typical cross-sectional variation in UI claims in our sample. Our estimates imply that a state which implemented SAH orders one week earlier saw an increase in UI claims by 1.9% of its 2018 employment level relative to a state one week later, which is slightly less than 50% of the cross-sectional standard deviation of employment-normalized claims between weeks ending March 21 and April 4.[22]

4.5 Aggregate Versus Relative Effects

Our empirical strategy relies on cross-sectional variation in the timing and location of SAH orders to identify the relative effect such policies had on labor markets during the initial weeks of the COVID-19 outbreak in the United States. In this section, we discuss in greater detail the sorts of spillovers that are likely to be relevant and the conditions under which the relative-implied aggregate estimate (see equation (4.6)) represents a lower or upper bound on the aggregate effects of SAH orders on UI claims. This is important for how one should interpret our back-of-the-envelope calculation that in the early period of the crisis, approximately only 24% of UI claims through April 4, 2020 were related to SAH orders.

To the extent that there are cross-regional (either positive or negative) spillovers of SAH orders, our estimate will not capture the *aggregate* effect of SAH orders. This limitation is related to the stable unit value (SUTVA) assumption in the causal inference literature, which requires that potential outcomes be independent of the treatment status of other observational units. Because of considerable trade between U.S. states, SUTVA is likely to be violated in our setting.[23]

To guide our discussion, we use a benchmark currency-union model to study the effects of SAH orders on the local economy, the rest of the currency union, and the entire economy

[22]We thank an anonymous referee for this particular recommendation.

[23]SUTVA violations are likely to be more salient in the cross-section when the model is estimated over longer horizons. This is, in part, why we choose as our baseline the 3-week horizon specification.

Table 4.1: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment for Weeks Ending March 21 thru April 4, 2020

	(1) Bivariate	(2) Covid	(3) Pol. Econ.	(4) Sectoral	(5) All
SAH Exposure thru Apr. 4	0.0194^{***}	0.0192^{**}	0.0178^{**}	0.0209^{***}	0.0187^{**}
	(0.00664)	(0.00742)	(0.00818)	(0.00637)	(0.00714)
COVID-19 Cases per 1K		-0.00213			0.00194
		(0.00621)			(0.00676)
Excess Deaths per 1K		0.0446			0.0480
		(0.109)			(0.113)
Share Age 60+		0.237			
		(0.281)			
Avg. UI Replacement Rate			0.0719		0.0726
			(0.0794)		(0.0787)
2016 Trump Vote Share			-0.0225		
			(0.0508)		
Work at Home Index				-0.331^{+}	-0.388^{+}
				(0.192)	(0.229)
Bartik-Predicted Job Loss				-2.401	
				(7.528)	
Constant	0.0815^{***}	0.0357	0.0621	0.181^{**}	0.182^{**}
	(0.00848)	(0.0543)	(0.0481)	(0.0742)	(0.0821)
Adj. R-Square	0.0829	0.0434	0.0618	0.0966	0.0763
No. Obs.	51	51	51	51	51

This table reports results from estimating equation (4.4): $\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s$, where each column considers a different set of controls X_s. Column (5)—a parsimonious model controlling for pandemic severity, political economy factors, and state sectoral composition—is our benchmark specification. The dependent variable in all columns is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (4.3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

Robust Standard Errors in Parentheses

$+\ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

as a whole. We present results for an economy characterized either by sticky prices or flexible prices, with SAH orders modeled as either a pure local demand shock or a pure local productivity/supply shock; the evidence from Appendix C.1 suggests that both channels were operative.[24] We then briefly summarize other important cross-regional spillovers not well-captured by the currency model we study. The most salient of these spillovers relate to the *informational* effect of early SAH implementation in some parts of the country.

Currency Union Model: Supply and Demand Shock Implications of SAH Orders

In this section, we consider the implications of local demand or supply shocks in a benchmark currency union model under either sticky or flexible prices. The model we consider is a simpler version of the baseline, separable utility, complete markets model presented in Nakamura and Steinsson (2014), modified to incorporate productivity shocks and discount rate shocks (to model negative local supply and demand shocks, respectively).[25] We follow Nakamura and Steinsson (2014) in calibrating the model to the U.S. setting. The full model specification is relegated to the Appendix; here we present only those aspects of the model modified to study the effects of SAH orders.

Modeling SAH Orders

Our first model experiment is to treat the implementation of SAH orders as a pure local demand shock. To incorporate this into the model, we introduce a consumption preference shock, δ_t. This preference shock causes home region households to prefer, all else equal, delaying consumption into the future. This may be a reasonable way to model the SAH shock for a variety of reasons. First, to the extent that the drop in retail mobility, as shown in Appendix C.1, represents a decline in goods consumption, households may simply be delaying such purchases until temporarily closed stores reopen. Second, the inability to purchase locally furnished goods and services may lead households to temporarily save more than they might otherwise choose to do, which would be observationally equivalent to a discount rate shock only to consumption.

Households in the home region maximize the present discounted value of expected utility over current and future consumption C_t and labor supply N_t.

[24] Additionally, as is discussed in Brinca et al. (2020), it is appropriate to view the COVID-19 pandemic (and associated policy responses) as some combination of demand and supply shocks. We consider pure demand and supply shocks to illustrate the economic implications of each in isolation.

[25] Implications from a model with different preference structures (e.g. Greenwood et al., 1988 preference) and with incomplete market are qualitatively the same. Unlike the original focus of Nakamura and Steinsson (2014), the model we consider does not incorporate government spending shocks, as that is not our focus in this paper.

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\delta_t \frac{(C_t)^{1-\sigma}}{1-\sigma} - \chi \frac{(N_t)^{1+\psi}}{1+\psi} \right],$$

where β is the rate of time discounting, σ is the inverse intertemporal elasticity of substitution, ψ is the inverse Frisch elasticity of labor supply, and χ is the weight on labor supply. The discount rate shock process follows

$$\log \delta_t = \rho^\delta \log \delta_{t-1} + \epsilon_t^\delta. \tag{4.7}$$

We close the household side of the model by assuming preferences for varieties are constant elasticity of substitution (CES), which gives rise to the standard CES demand curve via cost minimization.

Alternatively, the SAH orders may be modeled as a local productivity shock. Even if demand for locally produced goods is unchanged, firms may be constrained in supplying the goods and services demanded by local households or by the rest of the currency union. We model this interpretation as a region-level productivity shock for intermediate-goods-producing firms. A firm i in the home region faces the following production function

$$y_{h,t}(i) = A_t N_{h,t}(i)^\alpha,$$

where $y_{h,t}(i)$ is the output of a firm i, $N_{h,t}(i)$ is the amount of labor input hired by the firm, and A_t is region-wide technology in the home region. α is the returns to scale parameter on labor. The aggregate supply shock A_t evolves according to the following process:

$$\log A_t = \rho^A \log A_{t-1} + \epsilon_t^A. \tag{4.8}$$

Firms maximize profits subject to demand by households. Nominal rigidities are specified à la Calvo (1983) with associated price-reset parameter θ.

Finally, we close the model by assuming bond markets are complete, labor markets are perfectly competitive, and, when prices are sticky, the monetary authority follows a union-wide Taylor rule. A full derivation is available in the Appendix.

Model Results: Modeling SAH Order Shocks under Flexible and Sticky Prices

We model the implementation of SAH orders as a one-time negative shock with either $\epsilon_t^\delta = -1$ (for local demand shocks) or $\epsilon_t^A = -1$ (for local supply shocks). We choose zero decay parameters on the shock series to illustrate the dynamics of the model in settings in which the shock induced by the SAH order is temporary. Specifically, we set $\rho^A = \rho^\delta = 0$. For the purposes of mapping the relative-implied employment losses to aggregate employment losses, this is without loss for the results for the technology shock but not without loss with respect to the demand shock with sticky prices. Below, we discuss what happens when the demand shock exhibits some persistence.

We calibrate the remaining parameter values according to Nakamura and Steinsson (2014) (see their Section III.D.). When working with the sticky price model, we set the Calvo parameter $\theta = 0.75$. In the flexible price model, we set $\theta = 0$.

We consider each of the two types of shocks in isolation under either sticky prices or fully flexible prices. In each of the four scenarios, we calculate the on-impact responses of home region employment, foreign region employment, and aggregate employment to the local shock. Because the model is calibrated to a quarterly frequency and because our empirical design estimates the relative effect over a short horizon (3-weeks), the relevant horizon for mapping the model to the cross-section is the *on-impact* relative effect between employment in the shocked home region and the non-shocked foreign region.

The results from these exercises are reported in Figure 4.5 and Table 4.2. Figure 4.5 shows the *on-impact* responses of employment in a home region (blue circles) and a foreign region (red crosses), and aggregate employment (black squares) under the four different scenarios. Table 4.2 then compares the relative-implied aggregate employment calculated from the differences between the responses of home and foreign employment and the responses of aggregate employment under different scenarios.[26]

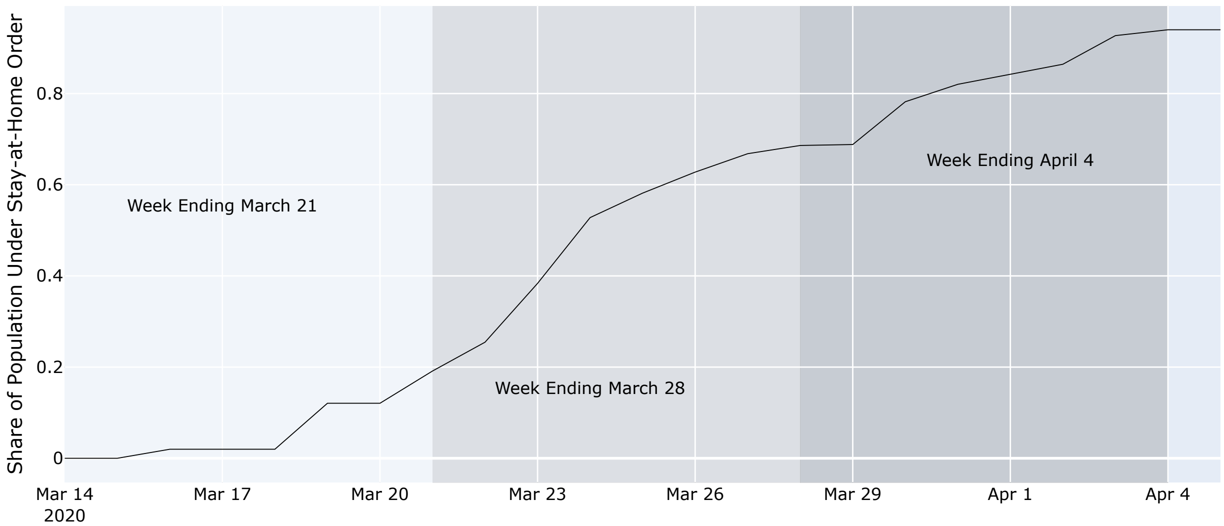
In the model, only three of the four stylized scenarios we consider produce relative effects of SAH orders that are consistent with the positive coefficient we estimate in the data. When the SAH orders are modeled as local productivity shocks, only the flexible price equilibrium produces an immediate, relative decline in employment in the home region subject to the shock. When the SAH orders are instead modeled as local demand shocks, both the sticky price and flexible price economies produce a steeper decline in the shocked home region's employment relative to the rest of the economy, as suggested by the cross-sectional evidence presented above.

When SAH orders are modeled as negative productivity shocks with fully flexible prices, the immediate, relative effect of SAH orders is an *upper bound* on the aggregate employment effect over the same horizon. This is because the decline in local employment arising from the SAH order is offset by an increase in employment in the rest of the economy. The mechanism is that in the flexible price case, the negative productivity shock in the home region translates into an improvement in the foreign region's terms of trade. This, in turn, increases labor demand in the foreign region, which increases employment in the foreign region.

In contrast, when prices are fully flexible in response to an SAH-induced home-region demand shock, the relative-implied estimate represents a *lower* bound on aggregate employment losses. This is because employment in both the home and foreign regions fall in response to the shock. With prices being fully flexible, the negative preference shock in the home region leads to a decline in prices for home goods relative to foreign goods, making foreign consumption more expensive. This, in turn, decreases demand for foreign goods,

[26] Formally, the relative-implied estimate in the model is calculated as $n(\ell_t - \ell_t^*)$, where ℓ_t and ℓ_t^* represent log deviations from steady state of home and foreign region per-capita employment respectively. n is the size of the home-region. This is exactly the model-analog of the relative-implied estimate reported in equation (4.6).

Figure 4.1: Cumulative Share of Population under Stay-at-Home Orders in the U.S.



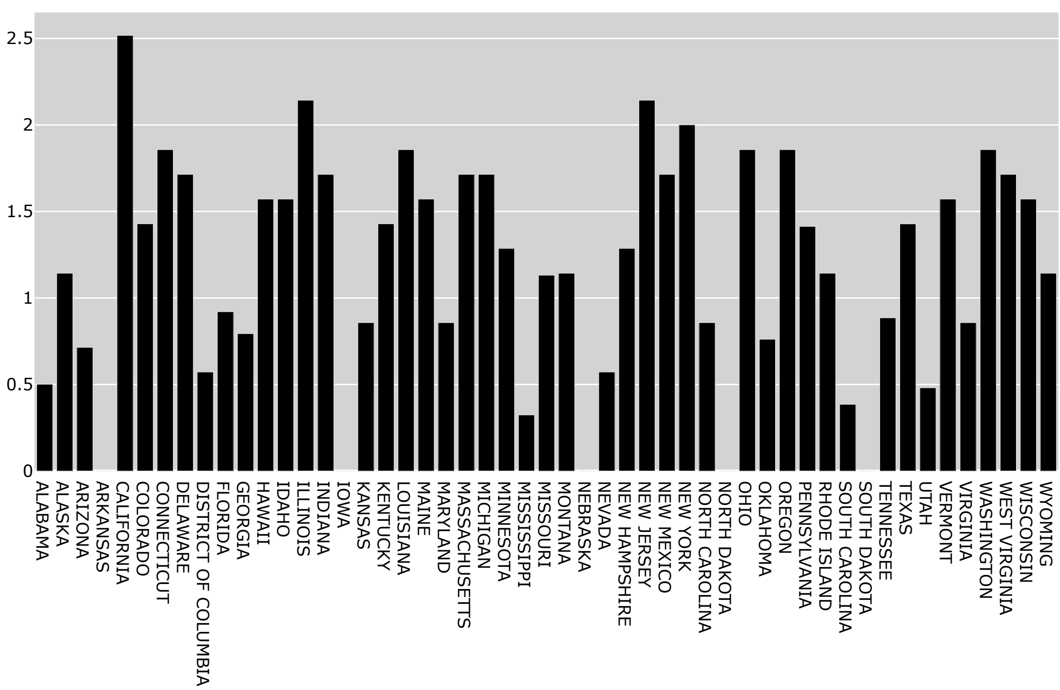
Sources: Census Bureau, the *New York Times*; Authors' Calculations.

Table 4.2: On-Impact Response of Union-Wide Employment and Relative-Implied Aggregate Employment to a Local SAH-induced: (i) Preference Shock with Flexible Prices, (ii) Preference Shock with Sticky Prices, (iii) Technology Shock with Flexible Prices, and (iv) Technology Shock with Sticky Prices

	Flexible				Sticky		
	Total	Implied	Factor		Total	Implied	Factor
Preference Shock	-0.047	-0.021	2.21	$\rho^\delta = 0.9$	-0.032	-0.075	0.43
				$\rho^\delta = 0.0$	-0.093	-0.083	1.12
Technology Shock	0.003	-0.021	-0.16		0.1642	0.1398	1.18

This table shows the on-impact responses of aggregate employment and the relative-implied employment to a local demand (preference) and supply (technology) shocks with flexible or sticky prices. The columns labeled "Total" correspond to the model-implied on-impact aggregate employment change (i.e. a population-weighted average of the employment change in the home and foreign regions). The columns labeled "Implied" correspond to the relative-implied aggregate change in the model. This is calculated as the difference between the on-impact employment effect in the home region and the on-impact employment effect in the foreign region, together multiplied by the size of the home region. This is the model analog of the relative-implied aggregate estimate in equation (4.6). A negative value for the implied column implies that the model is consistent with our cross-sectional estimate. The columns labeled "Factor" takes the ratio of the on-impact aggregate employment effect to the relative-implied effect. A negative value in this column (Flexible prices and Technology shock) implies that the relative-implied employment effect is of the opposite sign to the aggregate employment effect.

Figure 4.2: Employment-Weighted State Exposure to Stay-at-Home Policies Through Week Ending April 4



The Employment-Weighted exposure to SAH policies for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH orders by the 2018 QCEW average employment share of that county in the state, and summing over each states' counties. Sources: Bureau of Labor Statistics, the *New York Times*; Authors' Calculations resulting in a decline in foreign employment, which is necessary for market clearing. When prices are fully flexible and the effect of SAH orders is a pure local demand shock, aggregating the relative employment losses understates the aggregate employment losses by a factor of about two (see Table 4.2, Row 1, Column 3).

The case with sticky prices and SAH orders modeled as a pure local demand shock lies in between the previous two scenarios. When the local demand shock is sufficiently persistent, the immediate, relative effect of SAH orders could potentially *overstate* the aggregate employment effect. This is because employment in the foreign region increases on impact. Meanwhile, when the demand shock has essentially no persistence, so that it only affects demand in the home region for a single quarter, employment in the foreign region also falls on impact, implying that the (aggregated) relative employment effect again understates aggregate employment losses, in the quarter of the shock (See Figure 4.5). Regardless, the degree to which this on-impact effect understates aggregate employment losses is bounded above by the response under flexible prices to a local demand shock.

The evidence presented in Appendix C.1 suggests that SAH orders represented a shock to both the supply of and demand for locally produced goods. This on its own implies that the flexible price, preference shock scenario provides a non-binding upper bound on aggregate employment losses. Specifically, in this scenario the relative-implied aggregate estimate would understate employment losses by roughly a factor of two. The distance from this upper bound increases, moreover, with price rigidity and the persistence of the SAH shock. In the baseline calibration, when prices are sticky and the demand shock has no persistence, the relative-implied job losses understates aggregate employment losses by 12%.

Other Cross-Regional Spillovers

The benchmark currency-union model presented in the previous section illustrates how locally implemented SAH orders would affect the local economy, other regions in the currency union and the entire economy as a whole. The spillover forces in the model work through the trade in goods between regions and associated price and expenditure switching effects. However, there may be other important cross-regional spillovers that are not well-captured by the model, but may nevertheless be important for interpreting our empirical results in light of the aggregate effects of SAH orders.

An important example is an *informational effect* of early SAH implementation in some parts of the economy. For example, the early imposition of SAH orders in some regions may signal to the rest of the country that a SAH order is likely to be imposed some time in the near future. This informational channel can be incorporated into the model by assuming that the foreign region learns, on-impact, that a SAH order will be imposed in the foreign region in the subsequent period. We experimented with this specific informational channel of local SAH order implementation and found that the upper and lower bounds provided in the previous subsection continued to hold.[27]

[27] These results are available upon request.

A more subtle informational effect of SAH implementation relates to the credible signal it sends about the severity of the COVID-19 pandemic and the potential economic disruptions it is likely to induce, even in the absence of any additional SAH orders. In this interpretation, the SAH orders have spillover effects on the rest of the economy through the changes they induce to beliefs held by households and firms about the future path of the economy. As opposed to other signals conveyed by public officials about the severity of the pandemic, SAH implementation is a credible signal because it imposes non-trivial costs on the economy. This could, in turn, lead to a reduction in demand as a result of increased economic anxiety and fear of exposure to the COVID-19.

If this second informational effect of local SAH implementation ultimately led to job losses throughout the rest of the country, then our relative-implied estimate would understate the aggregate job losses attributable to SAH orders. Neither the model nor the empirical design takes this particular spillover mechanism into account. We view understanding the role of SAH orders as credibly communicating the severity of the pandemic as an important and interesting avenue for future research.[28]

Another important example is spillovers through firm networks—internal and external.[29] For example, complex supply chains may cause economic activity to decline in parts of the country where SAH orders are not yet enacted if the sourcing of intermediate inputs is affected. Alternatively, national chains may close establishments located in regions without SAH orders due to losses in other major markets with SAH orders. Arguably, these sorts of spillovers would lead our relative-implied estimate of job losses to understate true aggregate employment losses. However, we believe these channels are minor, as the adjustments would need to occur over a very short period time. The horizon of our empirical specifications is three weeks, during which time existing inventories were likely to be sufficient for production.[30]

4.6 Alternative Specification: County-Level Employment and Unemployment Effects

A major concern with the estimates of Equation (4.4) is that states may have experienced substantial difficulty in scaling up their systems to process the historically unprecedented numbers of unemployment claims. For example, it is well known that some states' unem-

[28]Coibion et al. (2020a) provide evidence that local SAH orders led households in the affected regions to hold more pessimistic views of the future path of the economy. This is a separate, though related, channel than the *aggregate* change in beliefs that may have occurred following the early imposition of SAH orders.

[29]We thank an anonymous referee for pointing this out.

[30]It is a well known observation that inventories generally adjust more slowly to changes in sales, consistent with the claim that this particular source of bias is most relevant at lower frequencies and longer horizons. (Bils & Kahn, 2000; Ramey & West, 1999).

ployment insurance systems rely on archaic computer programming languages.[31] Thus, it is reasonable to be worried that states with more cumbersome systems may systematically report lower UI claims numbers relative to those states with more efficient systems.

A priori, the induced omitted variable bias could go in either direction. On the one hand, states with stronger UI systems may have also been more inclined to respond aggressively to the COVID-19 pandemic with SAH orders, generating an upward bias in our estimates. On the other hand, the severity of labor market disruptions from the COVID-19 pandemic may have both made it more difficult for states to process new claims *and* made them more likely to impose SAH orders earlier—thus, generating a downward bias. While we have already controlled for measures of COVID-19 in our estimates of Equation (4.4), in this subsection we present an alternative design at the county-level using employment and unemployment as outcomes, albeit at a lower frequency. Using total employment, rather than unemployment insurance claims, allows us to sidestep the issue of whether states could meet demand for UI claims. This design also allows for the inclusion of state fixed effects to identify the relative effect of SAH orders using within-state variation in the timing of SAH implementation.

We analyze the effects of SAH orders at the county-level relying upon local area unemployment and employment statistics constructed by the Bureau of Labor Statistics (BLS). The downside is that this data is constructed at the monthly frequency, rather than the weekly frequency in our main specification.[32] The BLS primarily relies upon the Current Population Survey (CPS) as the primary input into constructing estimates of county-level employment and unemployment.[33] Fortunately, the survey reference periods for the CPS aligns quite nicely with measuring household employment and unemployment just prior to the broad implementation of SAH orders and one month hence. The reference week for the CPS for March 2020 was March 8th through March 14th and the reference week for April was April 12th through April 18th.

We estimate analogs of our state-level regression at the county-level, using as our outcome variable either the log change in employment or the change in the unemployment rate between March 2020 and April 2020. County-level treatment is the weekly SAH exposure through April 15, 2020. Formally, we estimate the following regression by ordinary least squares:

$$\Delta y_{c,s,April} = \alpha_s + \beta^{y}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s} \tag{4.9}$$

where $y_{c,s,April}$ indicates the monthly change between March and April in either log employment or the unemployment rate. α_s are state-level fixed effects which control for all state-level policies implemented between mid-March and mid-April that may have been systematically related to observed UI claims during that period. We also report results when

[31] See, for example, "'COBOL Cowboys' Aim To Rescue Sluggish State Unemployment Systems" by NPR (https://www.npr.org/2020/04/22/841682627/cobol-cowboys-aim-to-rescue-sluggish-state-unemployment-systems).

[32] In Appendix C.1 we estimate event study specifications using high frequency employment statistics at the county-level for a subset of counties in the U.S. for which these data exist. We find no evidence of differential changes in county-level employment prior to SAH implementation while at the same time finding that SAH orders lowered employment on average by 1.9% after one week.

[33] For additional details on the methodology employed by the Bureau of Labor Statistics, see https://www.bls.gov/lau/laumthd.htm.

constraining $\alpha_s = \alpha$ to provide a natural benchmark against our state-level regression. We also control for the number of confirmed COVID-19 cases per thousand people and the WAH index, which are our only controls available at the county-level.[34]

Because the first outcome variable we consider at the county-level is the log change in county employment, we expect that the estimated relative effect of SAH orders on local employment, $\hat{\beta}^{emp}_{C,county}$, will be comparable to our estimate of the same parameter at the state-level.[35] If the timing of the decentralized implementation of SAH orders was orthogonal to state-level economic conditions and if there were negligible spillovers from treated counties to untreated counties within the same state, then we would expect to see a relatively stable coefficient regardless of whether we include state fixed effects, α_s, or not.

Table 4.3 provides the results for the effects of SAH orders on employment. The first column shows the results restricting $\alpha_s = \alpha$ (e.g., no state fixed effects). The point estimate suggests that the relative effect of SAH exposure on employment at the county-level is to reduce employment by of -1.8% (SE: .57%). That we use a different outcome variable and different level of disaggregation yet obtain a coefficient of similar magnitude is encouraging.

Columns (2) and (3) focus on the 12 states for which there is variation across counties in the timing of SAH orders. The magnitude of the estimate falls by about one third, regardless of whether we include controls—although this difference is not statistically significant. If, as we argue above, the timing of SAH implementation was orthogonal to policies and economic conditions at the state-level[36], then the decline in the point estimate is suggestive evidence of negative spillovers between treated and untreated counties. While this may be the appropriate interpretation, it appears that the bulk of employment losses were nevertheless concentrated within the labor markets in which SAH orders were implemented.

Finally, in the last column, we include commuting zone fixed effects and find that the coefficient is roughly a third of the effect estimated in column (3). Following a similar logic as in the previous paragraph, this would suggest that not only were the bulk of employment losses concentrated within the labor market, they were moreover concentrated within the specific counties in which the SAH orders were implemented.

Table 4.4 provides the results for the effects of SAH orders on the change in the county-level unemployment rate. As with the employment specification, the first column does not include state fixed effects. In columns (2) and (3) we include state fixed effects; in the final column, we condition further on commuting zone fixed effects. Consider the result reported in column (3), the state fixed effects specification with controls for local COVID-19 pandemic and capacity for the local labor force to work from home: the point estimate is 1.5 (SE: 0.331), implying that each week of SAH exposure at the county-level increased the

[34] We control for the number of confirmed COVID-19 cases through April 15th to align with the timing of the surveys used by the BLS to construct county-level employment and unemployment statistics.

[35] Note that because we use the 2018 QCEW to normalize UI claims at the state-level, we should expect the county-level estimates to be slightly lower in magnitude since the state-level regressions calculates the percent change off of a smaller base value.

[36] And the average treatment effect among counties in the twelve states appearing in columns (2)-(4) is the same as for counties.

local unemployment rate by 1.5.

In sum, we view the the county-level results as corroborating evidence of the main result in this paper: that the cross-sectional effect of SAH orders had real costs to the labor markets in the early weeks of the crisis, but that such costs were likely dwarfed by other factors in the early weeks of the crisis. While not inconsistent with our state-level analysis, broadly the county-level design yields somewhat lower point estimates than in our benchmark specification. In this respect, relative to a null that all observed UI claims were attributable to SAH orders, the state-level specification yields the most conservative estimate of the relative effect of such orders on local labor markets. Through the lens of our theoretical model, these cross-sectional estimates imply, at most, a non-binding upper bound of half of total UI claims through April 4, 2020 being attributable to SAH orders.

4.7 Conclusion

While non-pharmaceutical interventions (NPIs) are necessary to slow the spread of viruses such as COVID-19, they likely steepen the recession curve. But to what extent? We provide estimates of how much one prominent NPI disrupted local labor markets in the short run in the U.S. in the early weeks of the coronavirus pandemic.

In particular, we investigate the effect of Stay-at-Home (SAH) orders on new unemployment claims in order to quantify the causal effect of this severe NPI (i.e., flattening the pandemic curve) on economic activity (i.e., steepening the recession curve). The decentralized implementation of SAH orders in the U.S. induced both geographic and temporal variation in when regions were subject to restrictions on economic and social mobility. Between March 14th and April 4th, the share of workers under such orders rose from 0% to almost 95%. This rise was gradual but steady, with new areas implementing SAH orders on a daily basis. We couple this variation in SAH implementation with high-frequency unemployment claims data to quantify the resulting economic disruption.

We find that a one-week increase in stay-at-home orders raised unemployment claims by 1.9% of state-level employment. This estimate is robust to a battery of controls, including the severity of the local COVID-19 pandemic, the local political economy response, and the industry mix of the local economy. A back-of-the-envelope calculation using our estimate implies that SAH orders resulted in a rise of 4 million unemployment insurance claims, about a quarter of the total unemployment insurance claims during this period. A stylized currency union model suggests that in some empirically relevant cases, this estimate can be seen as an upper bound. When it instead represents a lower bound, it at most understates job losses by a factor of two.

While it is beyond the scope of this paper to uncover all determinants of the unprecedented initial rise in unemployment during the COVID-19 pandemic, there is evidence that the economic downturn was already under way by the time that SAH orders were implemented. Even before the national emergency was announced by President Trump on March 13, 2020, households were reallocating their spending away from in-person goods and services.[37] Consistent with this evidence, our estimates imply that a sizeable share of the increase in unemployment in the early weeks of the COVID-19 crisis was due to other channels, such as decreased consumer sentiment, stock market disruptions, and social distancing that would have occurred in the absence of government orders.

Nevertheless, despite representing a minority share of the overall increase in unemployment in the initial three weeks of the crisis, our estimates suggest that over longer horizons SAH orders played a much larger role. Performing an out-of-sample forecast through April 25 of the relative-implied aggregate effect of SAH orders is illustrative: An additional 7.5 million UI claims between April 4 and April 25 are due to SAH orders, little more than half of the additional overall increase in UI claims nationally during that time.[38]

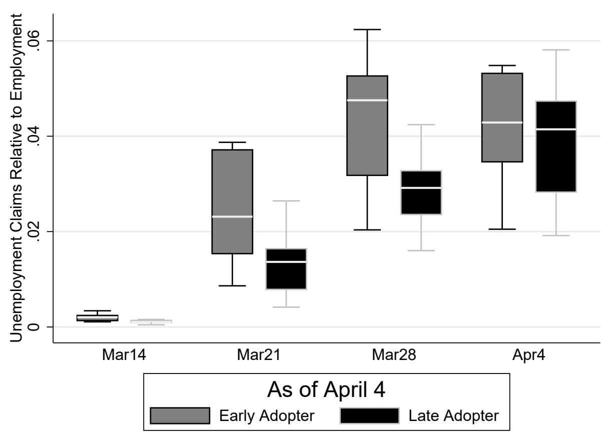
We see our paper as providing evidence that undoing SAH orders may relieve only a fraction of the economic disruption arising from the COVID-19 pandemic while at the same time exacerbating the public health crisis. This implies that the economic downturn may persist at least until the pandemic itself is resolved. At the same time, we document a large elasticity of unemployment with respect to such lockdown measures, suggesting that the costs of SAH orders are non-trivial in the long-run.

The COVID-19 pandemic had extremely large and rapid effects on economic activity both in the United States and around the world. The ripple effects of the pandemic will likely reshape the economic landscape for years to come. This chapter studies the immediate effects of the pandemic on local labor markets, one of the most important components of a modern economy. Understanding how the pandemic will change other aspects of the economy will be a key focus of economic research. If past is prologue, the financial sector is likely to experience drastic changes in its structure, much like the aftermath of the Great Recession. Whether due to new regulation, changes in risk appetites, or even merely spillovers from the real economy (e.g. long-term adjustments in the labor market), understanding the effects of the COVID-19 pandemic on the financial sector will be an important topic of research in the years to come.

[37]By March 13, grocery spending was up 44%, restaurant spending was down 10%, and entertainment and recreation spending was down 23%, all relative to their respective levels in January 2020. At about the same time—and preceding any reported SAH orders—both national consumer spending and small business revenue began their precipitous declines. Statistics calculated from data available at https://tracktherecovery.org/.

[38]This helps to reconcile our estimates with Coibion et al. (2020a) who find a larger contribution of SAH orders to job losses throughout April than we do. In this exercise, we adjust for whether a state reopened before April 25; not adjusting increases the out-of-sample forecast to 7.6 million claims. See https://www.nytimes.com/interactive/2020/us/states-reopen-map-coronavirus.html for state reopening dates.

Figure 4.3: Box Plots by Week of Initial UI Claims Relative to Employment for Early and Late Adopters of SAH orders



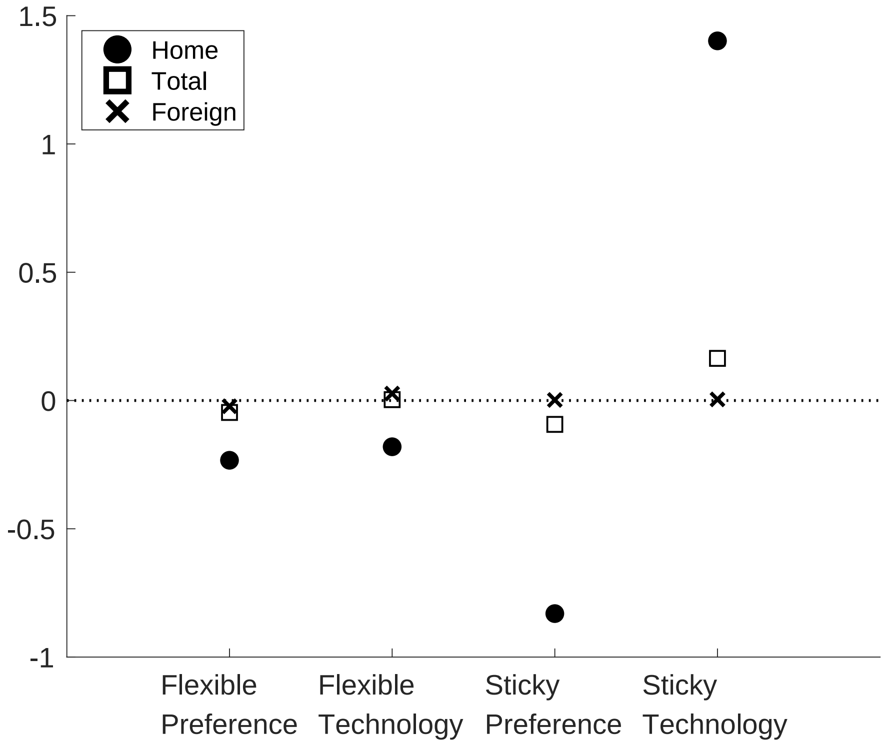
For each state we calculate SAH exposure through April 4th by multiplying the number of weeks each county was subject to SAH through April 4 by the 2018 QCEW average employment share of that county in the state, and summing over each state's counties. Early adopters are those states in the top quantile of SAH exposure and late adopters are those states in the bottom quartile. Sources: Bureau of Labor Statistics, Department of Labor, and the *New York Times*; Authors' Calculations.

Figure 4.4: Scatterplot of SAH Exposure to Cumulative Initial Weekly Claims for Weeks Ending March 21 thru April 4

The Employment-Weighted exposure to SAH policies for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH orders by the 2018 QCEW average employment share of that county in the state, and summing over each states' counties. UI claims are cumulative new claims between weeks ending March 21, 2020 and April 4, 2020, divided by average 2018 QCEW average employment in the state. The size of each bubble is proportional to state population; The color gradient of each observation is determined by the number of confirmed COVID-19 cases per thousand people.

Sources: Bureau of Labor Statistics, Census Bureau, the *New York Times*, USAFacts.org, Department of Labor; Authors' Calculations

Figure 4.5: On-Impact Response of Home Employment, Foreign Employment, and Union-Wide Employment to a Local SAH-induced: (i) Technology Shock with Flexible Prices, (ii) Technology Shock with Sticky Prices, (iii) Preference Shock with Flexible Prices, and (iv) Preference Shock with Sticky Prices



This figure shows the on-impact responses of aggregate employment and employment in each region to local demand (preference) and supply (technology) shocks with flexible or sticky prices. Each column represents different scenarios. In both all cases, the shocks persist for a single quarter only ($\rho^{\delta} = \rho^{A} = 0$; see equations (4.7) and (4.8)). The blue circles show the responses of employment in a home region, the red crosses are the responses of employment in a foreign region, and the black squares are the responses of aggregate employment. In the first three scenarios, the on-impact effect of home region employment declines *relative* to employment in the foreign region; this is consistent with our cross-sectional estimates of a positive coefficient on SAH exposure. The final column in which prices are sticky an the SAH orders are modeled as a technology shock produces a counterfactual prediction that employment is higher in the home region relative to the foreign region.

Table 4.3: County-Level Specification: Effect of Stay-at-Home Orders on Local Employment Growth

	(1) $\Delta \ln Emp$	(2) $\Delta \ln Emp$	(3) $\Delta \ln Emp$	(4) $\Delta \ln Emp$
SAH Exposure thru Apr. 15	-0.0176***	-0.0124**	-0.0129**	-0.00905**
	(0.00568)	(0.00464)	(0.00453)	(0.00397)
Covid-19 Cases per 1K Emp			-0.0000280	-0.000116
			(0.0000348)	(0.000121)
Work at Home Index			0.0549	0.0547
			(0.0457)	(0.0537)
Constant	-0.0824***	-0.113***	-0.129***	-0.135***
	(0.0147)	(0.00900)	(0.0157)	(0.0139)
Dep Mean	-0.12	-0.14	-0.14	-0.14
States	51.00	12.00	12.00	12.00
State FE	No	Yes	Yes	Yes
CZ FE	No	No	No	Yes
Adj. R-Square	0.10	0.62	0.63	0.74
No. Obs.	3141.00	1116.00	1116.00	453.00

This table reports results from estimating equation (4.9): $\Delta \ln Emp_{c,s,April} = \alpha_s + \beta^{Emp}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s}$, where each column considers a different set of controls X_s. The dependent variable in all columns is $\Delta \ln Emp$, which refers to the log change in county employment between March, 2020 and April, 2020 as estimated by the BLS. SAH exposure for a particular county is calculated as the number of weeks that the county was subject to SAH orders through April 15, 2020. Columns (2) thru (4) include state fixed effects; Column (3) includes fixed effects for USDA defined commuting zones (CZ).

Standard Errors Clustered by State in Parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.4: County-Level Specification: Effect of Stay-at-Home Orders on Local Unemployment Rate

	(1)	(2)	(3)	(4)
	ΔUR	ΔUR	ΔUR	ΔUR
SAH Exposure thru Apr. 15	1.574***	1.382***	1.570***	0.944***
	(0.400)	(0.331)	(0.331)	(0.216)
Covid-19 Cases per 1K Emp			-0.000239	0.0110
			(0.00468)	(0.00806)
Work at Home Index			-12.29**	-5.437
			(5.336)	(5.089)
Constant	4.114***	4.425***	7.922***	6.689***
	(0.888)	(0.642)	(2.005)	(1.863)
Dep Mean	7.69	7.11	7.11	7.32
States	51.00	12.00	12.00	12.00
State FE	No	Yes	Yes	Yes
CZ FE	No	No	No	Yes
Adj. R-Square	0.13	0.39	0.40	0.59
No. Obs.	3141.00	1116.00	1116.00	453.00

This table reports results from estimating equation (4.9): $\Delta UR_{c,s,April} = \alpha_s + \beta^{UR}_{C,county} \times SAH_{c,s,Apr.15} + X_{c,s}\Gamma + \epsilon_{c,s}$, where each column considers a different set of controls $X_{c,s}$. The dependent variable in all columns is ΔUR, which refers to the change in the county unemployment rate between March, 2020 and April, 2020 as estimated by the BLS. SAH exposure for a particular county is calculated as the number of weeks that the county was subject to SAH orders through April 15, 2020. Columns (2) thru (4) include state fixed effects; Column (3) includes fixed effects for commuting zones (CZ) classified by the USDA in 2000.

Standard Errors Clustered by State in Parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 5

Conclusion

This dissertation focused on the role of financial institutions in a modern economy. As emphasized in the introduction, the role of financial institutions extends well beyond that of simply allocating loanable funds. This dissertation studies two additional roles that financial institutions have typically taken on. Financial institutions help facilitate foreign exchange transactions in international trade. The usage of foreign exchange in international trade, especially in emerging markets, is ubiquitous. Financial institutions must also manage their balance sheets to contain risks. If the liabilities of financial institution are structured such that investors or depositors are more likely to run on a bank, then ex-ante these institutions may be more susceptible to failure with potential negative effects on local economies.

The first chapter of this dissertation studied how financial institutions are crucial for international payments. In particular, financial institutions play an important role in converting payments from one currency to another. This chapter found that the conversion of payment into different currencies is a substantial trade cost for exporters in emerging markets. As a matter of policy, reducing exchange rate risk for exporters in emerging markets may be a path to economic growth. Economists have long understood the potential role of international trade in driving economic growth.[1] Understanding how to expand the SML system may provide a boost not only to trade, but also to long-run growth.

The second chapter of this dissertation focused on how the structure of financial institutions can generate or amplify economic shocks. As the Great Recession made clear, more research is needed to understand the liability side of financial institutions balance sheets. A heavy reliance on short-term funding caused problems during the 2008 Financial Crisis as credit markets dried up, and similarly in the Great Depression those building and loan associations that relied more on short-term depositors failed a higher rate. To the extent that investors in short-term funding markets were more risk-averse or “flighty” based on which institution they chose to invest in could then have amplified the economic effects of the financial crisis. Whether this pattern exists throughout all financial history, such as in the wave of losses in the Savings and Loan Crisis in the late 1980s or the Japanese Financial

[1]See, for example, Frankel and Romer (1999), Pavcnik (2002), or Bloom et al. (2015)

Crisis of the early 1990s is left to future work.

Finally, the last chapter used the econometric techniques and theoretical tools from the previous chapters. In joint work, we studied the short-term effects of one of the most important public health measures, stay-at-home orders, on local labor markets. We found that the imposition of stay-at-home had a negative effect on local labor markets. However, this effect accounts for at most half of the rise in unemployment during this time period. The remaining effect is likely due to declining consumer sentiment as individuals chose to stay home. Our results suggest that stay-at-home orders had substantial effects on local labor markets, at least in the short-term. Future work will continue to study the effects of the COVID-19 pandemic and policy response.

Financial institutions plan an important role in modern economies around the world. This dissertation argues that their role in facilitating payments and managing risks has important implications for the real economy. Specifically, the need to use foreign currency in international trade lowers export volumes, and the attraction of flighty depositors may increase risks within the financial system with corresponding effects on the local economy. Future work will continue to understand the interactions between financial institutions and the economy.

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Appendix A

Appendix to International Payments as a Barrier to Trade: Evidence from Brazil

A.1 Data Appendix

Construction of Customs Dataset

Customs transactions are recorded from customs forms, filled out online by Brazilian exporters. These forms include, in particular, the unique 12-digit firm identifier, the CNPJ, the 8-digit HS code for the individual sector and the destination. Individual transactions are summed to the monthly frequency. The data is therefore available at the monthly frequency by firm, eight-digit HS sector, and destination. An example of a customs form is given in Figure (A.1).

The *CNPJ*'s are 14-digit identifier coes representing an establishment. This code is comprised of 8 digits representing the parent firm, then 4 digits representing the specific establishment, with two check digits at the end. Following other work in this area, I report all results at the establishment level.

Each sector is defined by the "Nomenclatura Comum do Mercosul" code, or Mercosul Common Nomenclature code, which is effectively an eight digit HS code. I collapse the data to the 4-digit NCM code, which is the group level in the Harmonized System. For example, while chapter 09 refers to Coffee, Tea, Maté, and Spices, 0902 refers to Tea.

The currency of invoicing is included in the overwhelming majority of cases. For a number of very small transactions, as simplified customs form is used that does not include the currency of invoicing. Conversations with customs officials suggests that these transactions are likely to be invoiced in USD, so I record them as such.

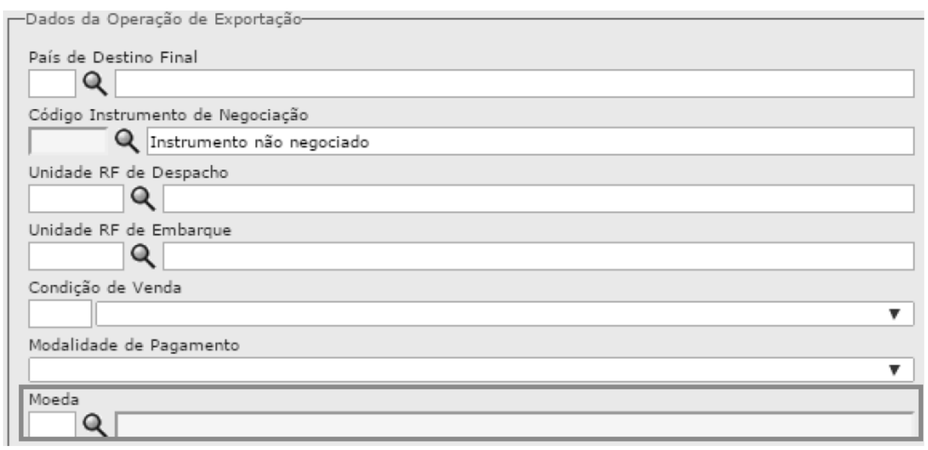
The currency of invoicing is recorded in nearly all cases. Table A.1 reports the share of total value for each currency in the raw sample. Other than USD, Euro, and BRL, no other currency accounts for more than 1% of international trade by volume or by count. As in other emerging markets, the USD accounts for the vast majority of export transactions, over 94% by volume. The count share of approximately 80% does not include the "zero"

Table A.1: Currency Share in Raw Sample

Currency	Value	Count
United States Dollar	94.04%	80.86
Euro	4.07	4.95
Brazilian Real	1.11	3.02
Great British Pound	0.27	0.15
Japanese Yen	0.14	0.04
Swedish Krona	0.03	0.05
Canadian Dollar	0.02	0.02
Australian Dollar	0.02	0.03
Swiss Franc	0.01	0.02
Norwegian Krona	0.00	0.00
Danish Krona	0.00	0.00
Other (0)	0.30	10.86

Share of raw data by currency. Currencies report include at least four firms for disclosure reasons. Other (0) indicates that the currency is not recorded. Zero's are the result of rounding. Zero's are likely invoiced in USD.

Figure A.1: Customs Form (Online)


Dados da Operação de Exportação
País de Destino Final
Código Instrumento de Negociação
Instrumento não negociado
Unidade RF de Despacho
Unidade RF de Embarque
Condição de Venda
Modalidade de Pagamento
Moeda

Screenshot of a customs form to be filled out by an exporter. The key field denoting the currency of invoicing, labeled "moeda", is outlined in red by the author. **Source:** SECEX.

currencies which are likely USD invoiced. Taken together, the USD accounts for roughly 90% of trade by count. The smaller share of USD transactions by count compared with volume is a similar finding as in other datasets, such as in Belgium in Amiti et al. (2018).

From the monthly data, I filter the sample to eliminate non-firms, which account for roughly 23,849 monthly observations. These observations have *CPF*'s (individual identifiers) rather than having *CNPJ*'s (establishment identifiers). I also eliminate two observations which have zero value recorded. The resulting dataset has 10,595,854 currency-firm-HS8-destination-month observations.

I aggregate the data to four-digit HS sector at the quarterly frequency. The key variables of interest are value, prices, and the currency of invoicing. Value is summed over all months and eight-digit HS sectors within a four-digit HS sector and quarter. Following Chatterjee et al. (2013), within each four-digit sector I check which measure has more non-missing observations between net weight and statistical quantity. For those transactions with non-missing values of the more populated measure, denoted Q. I then collapse the data to the quarterly frequency by firm, destination, and 4-digit sector. For values, I take the sum over all shipments within the cell. As for prices, I take a weighted average across all shipments within the cell. Collapsing to the quarterly dataset as explained above and aggregating across currencies results in a dataset of 4,673,465 firm-HS4-destination-quarter observations. At this quarterly frequency, I then winsorize the data at the 1% and 99% level within a Country-HS2 cell to arrive at value V. Prices are calculated as $P = V/Q$

Figure A.2 plots the number of firms in each quarter in the final sample. Two facts stand out. First, the number of exporting firms is relatively stable following the Great Recession, with the average over time being between 14,000 and 15,000 in any given quarter. Second, there was a decline in the number of firms owing to the Great Recession. Figure A.3 plots the number of transactions in each month, defined as the total exports by firm-sector-destination. The number of transactions is relatively stable over the course of the sample. Figure (A.4) plots total value, both raw and winsorized, over the course of the sample. Other than a significant and short-lived dip during the Great Recession, total exports continued to rise over the course of the sample.

Additional Summary Statistics

Figure A.5 presents value shares for firm-HS4-destination-quarters with strictly greater than 0% BRL shares and strictly less than 100% BRL shares.

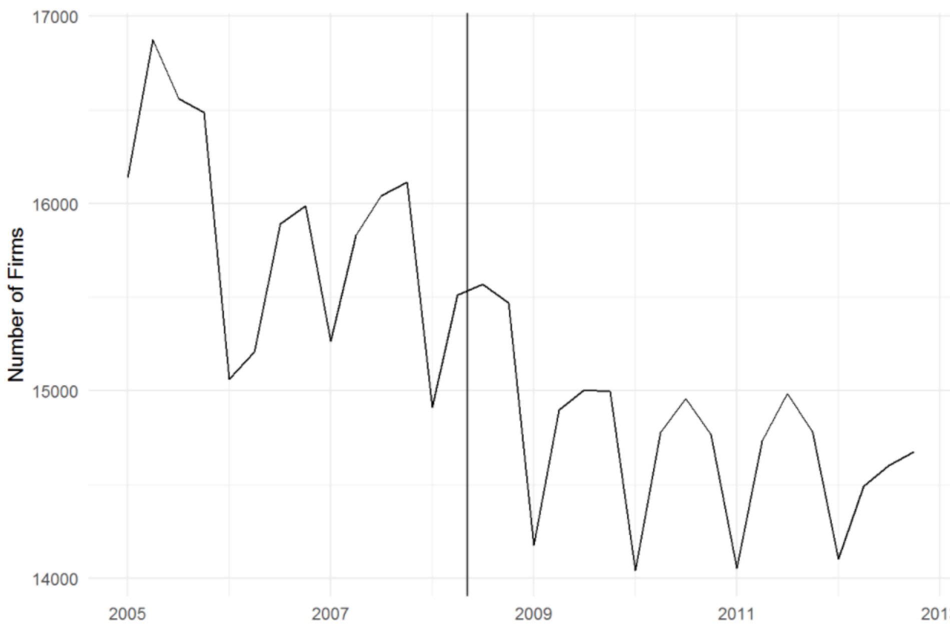
A.2 Additional Empirical Results

Municipality Evidence: Balancing the Panel

One concern with the results in Figure (2.6) is that the panel is unbalanced. In periods when municipality-sectors do not export, I treat this data as missing. Ideally, one would condition only on municipality-sector-destinations for which data from all years is available. Unfortunately, this is difficult because the treatment group to limited only 3% of the resulting sample, mainly due to the usage of HS2 level data. Here, I explore a number of alternative specifications to show that the results are not driven by the unbalanced nature of the panel.

I conduct three alternatives specifications. First, I collapse the data across sectors to the municipality-destination level, condition on continuous municipalities, and plot the triple

Figure A.2: Number of Firms Over Time



This figure plots the number of unique firms each quarter in the final sample. **Source:** SECEX.

Figure A.3: Number of Transaction Over Time

This figure plots the number of transactions each quarter in the final sample. **Source:** SECEX.

Figure A.4: Value Over Time

This figure plots the total value of each quarter in the final sample. The line labeled "raw" denotes the raw value. The line labeled "winsorized" winsorizes the raw data at the 1% and 99% levels within each HS2-Destination cell. **Source:** SECEX.

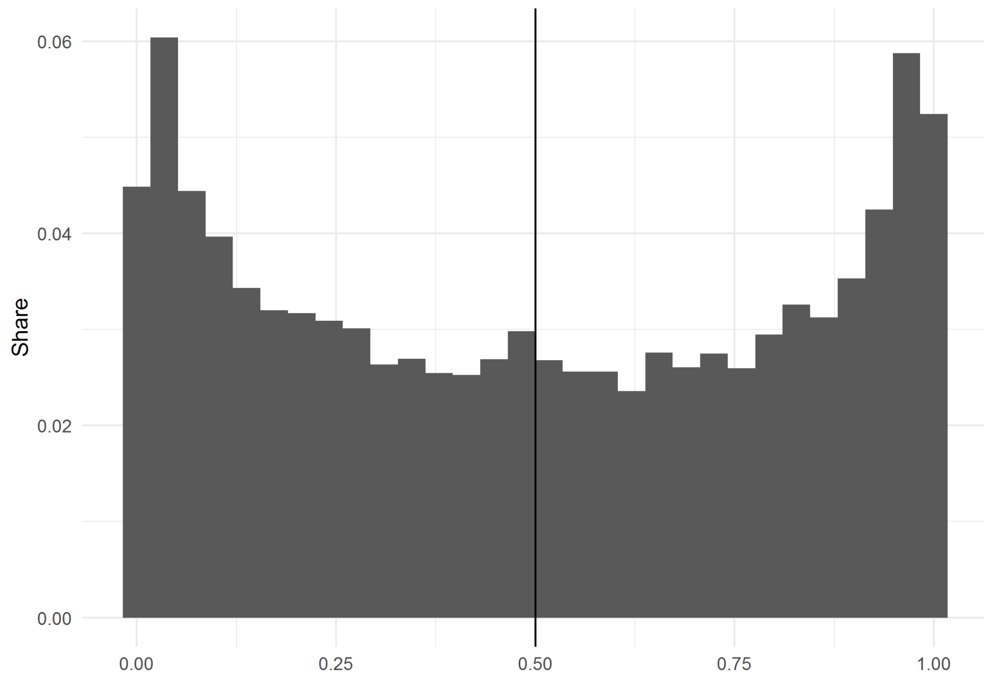
difference specification. The results are displayed in Figure (A.7). Collapsing the data to the municipality level and conditioning on municipality-destination data with the full sample does not affect the pretrend. The estimated effect of the SML system is a bit more delayed, but quantitatively significant.

Second, I drop municipality-sectors for which there are zero or only one pre-period observations. The results are displayed in Figure (A.6). Conditioning on municipalities with at least observable pre-periods enlarges the standard errors, but the results are qualitatively similar.

Municipality Evidence: Drop Largest Banks

Anecdotal evidence from Central Bank employees suggests that the largest banks were not likely to use the SML program due to lower revenues. Specifically, the SML system typically relies on the official exchange rate (although it is not required), so banks do not charge any spread on the exchange rate when clients use the SML system. Large banks with a high volume of foreign exchange transactions can take advantage of this spread in ways that smaller banks do not.

Figure A.5: BRL Value Shares (Greater than zero and less than one)



This figure plots the share of exports invoiced in BRL for shipments with strictly greater than 0% and strictly less than 100%. **Source:** SECEX.

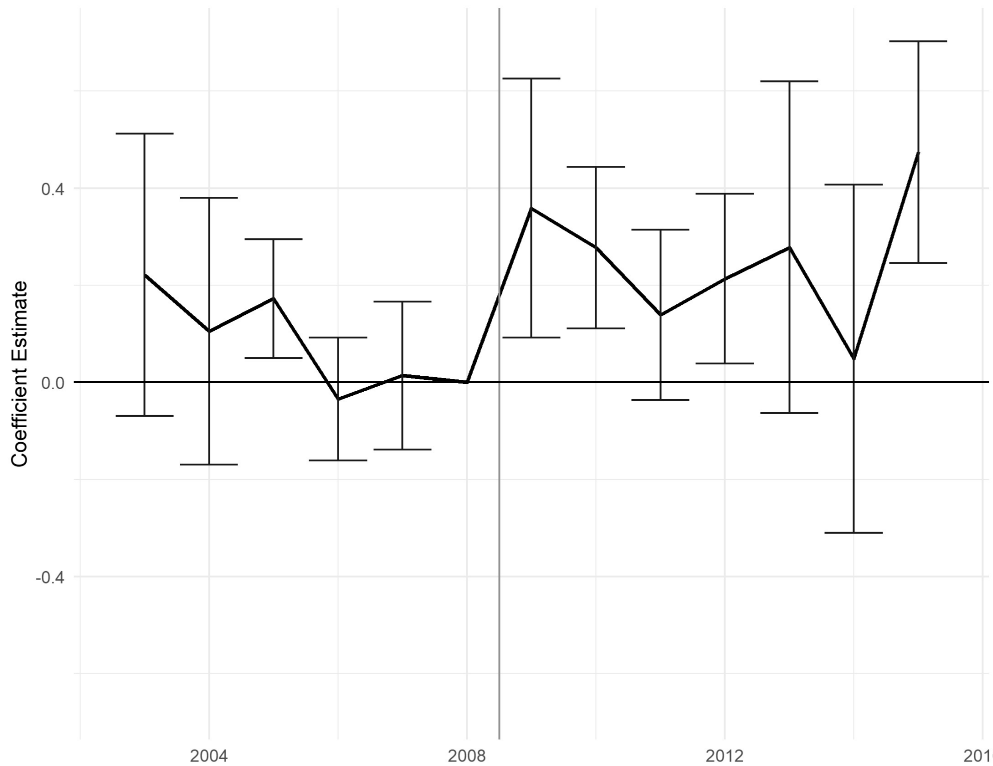
This factor implies that the largest banks bias downwards causal estimates of the usage of the SML system in the municipality results in the main text. Binning the measure of treatment into high and low SML usage partially alleviates this issue. However, another way to check whether this story is true is to assume that the largest banks, although they are authorized to use the SML system, do not actually use it. I recalculate the SML_Share_m variables assuming the two largest banks, Banco do Brasil and Itau, were not authorized to use the SML system.

Figure (A.8) presents results for this regression. The pretrend is in fact slightly smoother in the run up to the introduction of the SML system, and the increase in the years immediately following its introduction are more noticeable. While the long-run effects are attenuated, the SML system still has a noticeable effect on export volumes at the municipality level.

Micro Evidence: Additional Breakdowns

Figure A.9 reports the breakdown of ι for Colombia, while Figure A.10 shows the evolution of ι relative to other export destinations.

Figure A.6: Results with at least one pre-period observation



Micro Evidence: Additional Sectoral Figures

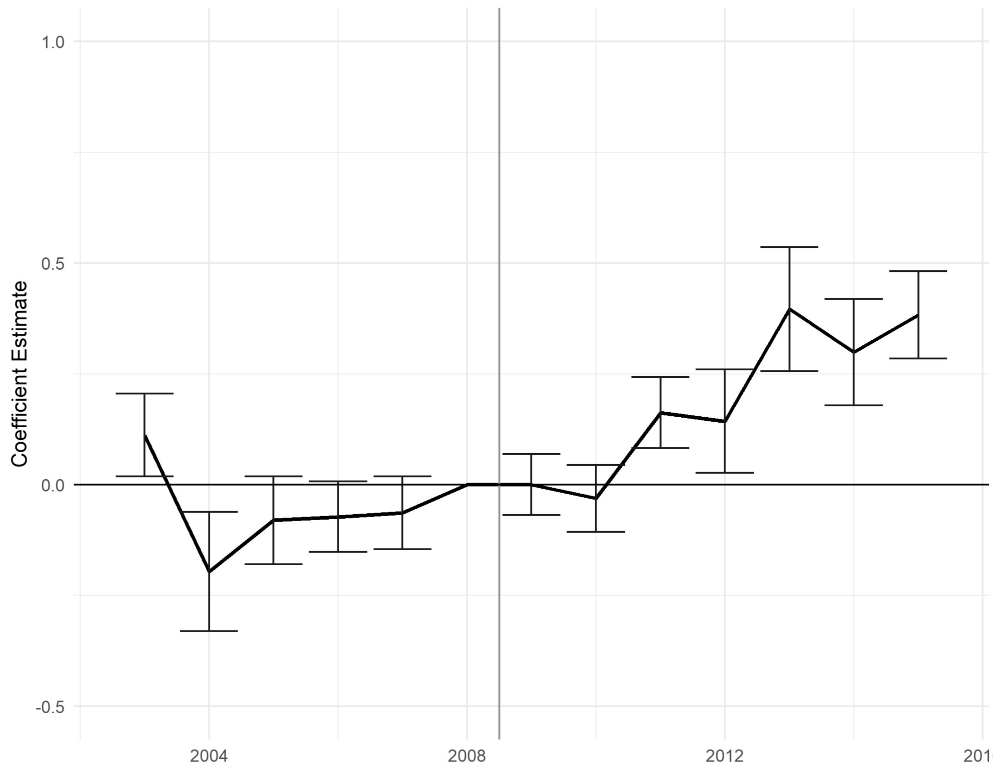
Figures (A.11a) through Figure (A.11t) plot the evolution of ι_{ijst} for $j = ARG$ by two-digit HS sector. While the overall pattern is similar for many sectors, there are also a number of sectors for which there is little to no rise in BRL invoicing.

Micro Evidence: Alternative Fixed Effect Specifications

To check for the presence of firm-level, I estimate Equation (2.4) that explicitly controls for time-varying changes at the firm-sector level. Recall both that firms' currency choice is extremely sticky over time, and that most of the variation in ι^{BRL} following the introduction of the SML system occurs across sectors within a firm. There is therefore little variation across sectors or destinations in invoicing currency. The results are displayed in Column (1) of Table (A.2). The coefficient estimate of 0.438 (SE: 0.094) suggests that invoicing in BRL raises the size of an export shipment by 44% relative to export shipments to other destinations not invoiced in BRL in the same 4-digit sector by the same firm at the same time. This estimate is statistically insignificant from the coefficient estimate in Table 2.6.

For transparency, in Column (2) I interact this measure with a dummy variable for

Figure A.7: Results at municipality level conditional on having all data



Argentina. While the coefficient on this interaction is negative, the large standard errors on both the main effect and interaction effect suggest that there is not much additional variation in BRL invoicing shares outside of Argentina. In other words, there is almost no variation across destinations within an individual firm's four-digit sector at a given quarter outside that of using the SML system.

A.3 Model Appendix

Derivation of Equilibrium Price

The program for the exporter is given by

$$\max_{P^\ell} \frac{S_X^\ell P^\ell Q}{1+\tau_X} - C(Q)$$
$$\text{s.t. } Q = f(S_M^\ell P^\ell (1+\tau_M))$$

Figure A.8: Results assuming largest banks do not use the SML system

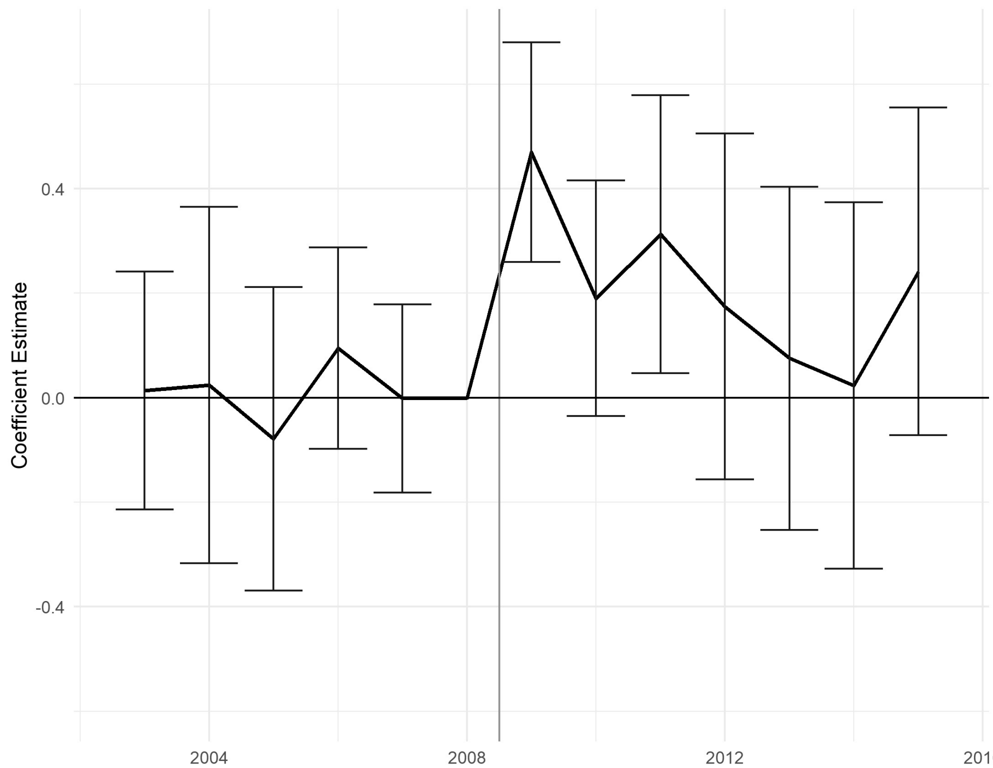


Table A.2: Leveraging Cross-Country Variation

	$\ln V_{ijst}$	
	(1)	(2)
ι_{ijst}	0.433***	0.866***
	(0.098)	(0.316)
$\iota_{ijst} \times ARG_j$		-0.478
		(0.326)
Firm-HS4-Time FE	Y	Y
Dest FE	Y	Y
Obs	1,266,628	1,266,628
$\mathcal{R}^2$	0.728	0.728

$*\ p < 0.1$; $**\ p < 0.05$; $***\ p < 0.01$. Standard errors clustered at the establishment level. Column (1) of this table reports regressions of the form $y_{ijst} = \alpha_{ijs} + \alpha_{jt} + \beta\iota_{ijst} + \varepsilon_{ijst}$, where y_{ijst} represents the log value of exports (in USD) for establishment i in sector s to destination j at time t. ι_{ijst} is a dummy variable equal to 1 if at least 10% of exports in sector s by firm i to destination j are invoiced in BRL. In Column (2), I estimate the equation $y_{ijst} = \alpha_{ijs} + \alpha_{jt} + \beta\iota_{ijst} + \gamma\,(ARG_j \times \iota_{ijst}) + \varepsilon_{ijst}$, where y_{ijst}

Figure A.9: Colombia ι Shares

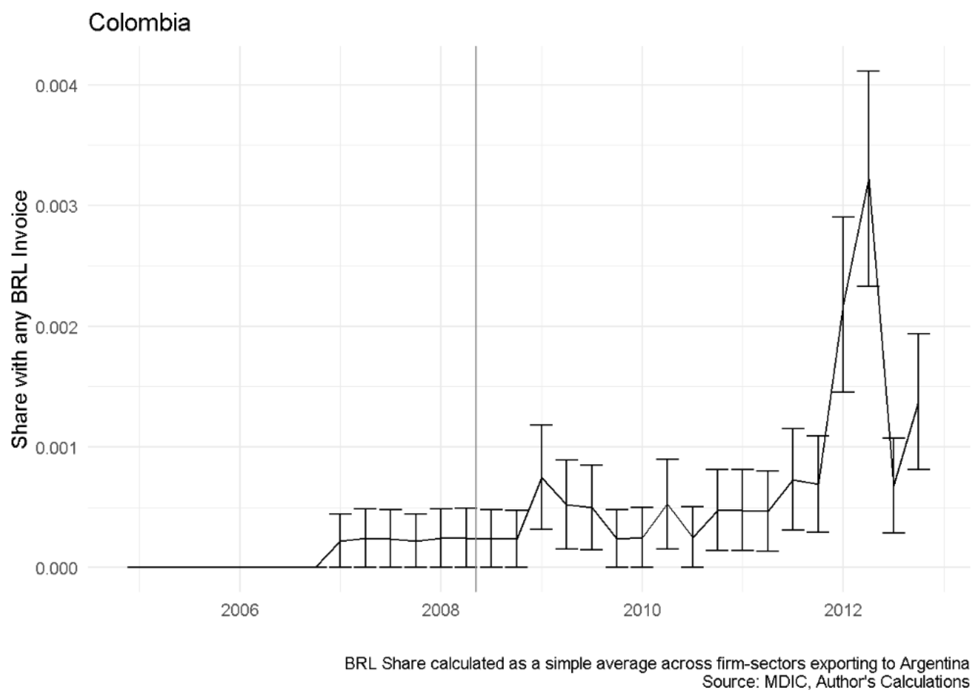
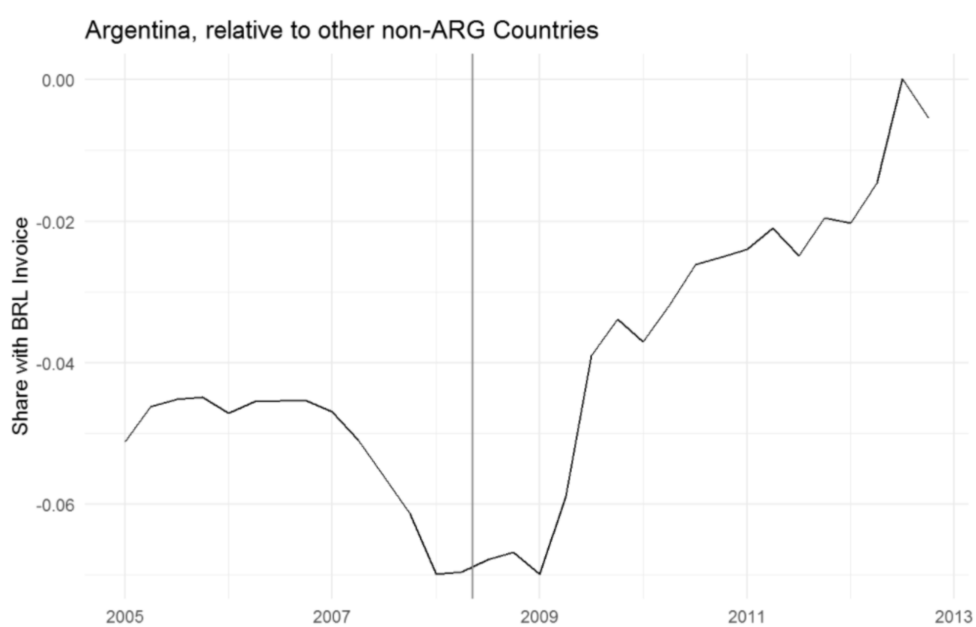
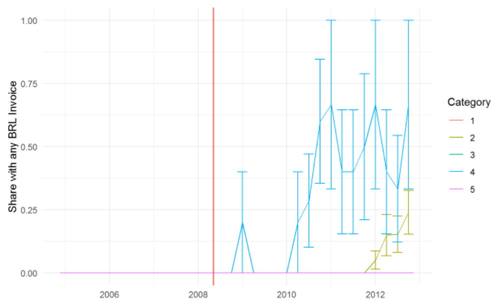
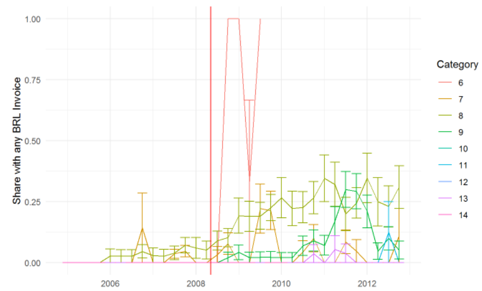


Figure A.10: Argentina ι shares relative to other Latin America countries

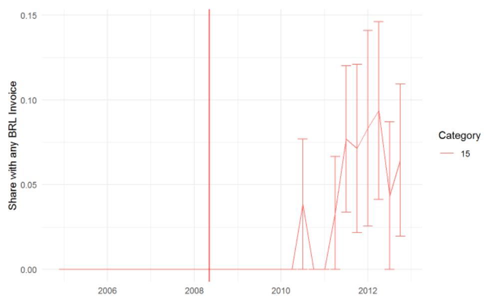




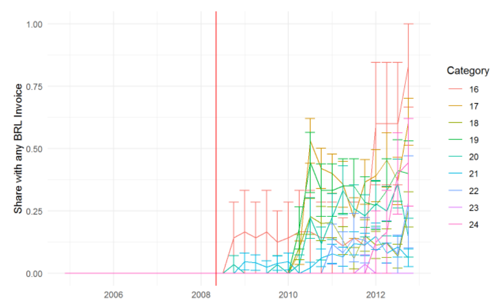
(a) Section 1: Live Animals



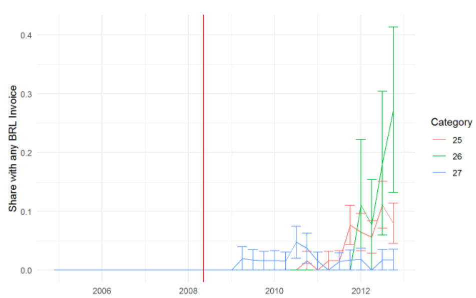
(b) Section 2: Vegetable Products



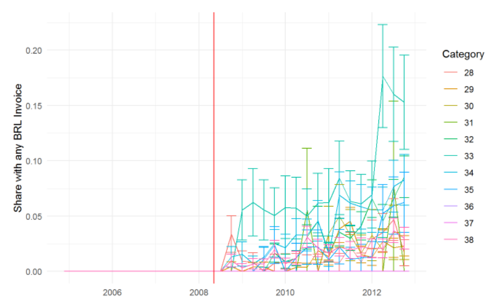
(c) Section 3: Animal and Vegetable Fats and Oils



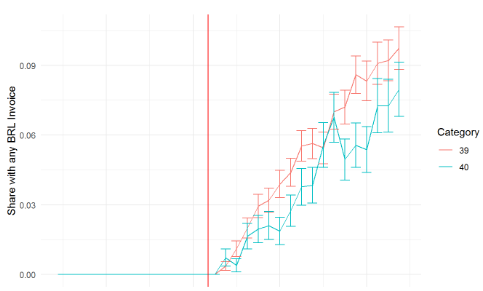
(d) Section 4: Prepared Foodstuffs, Beverages, and Tobacco



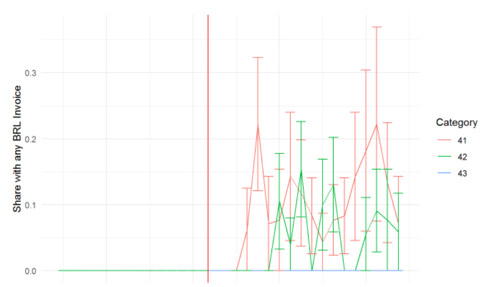
(e) Section 5: Minerals



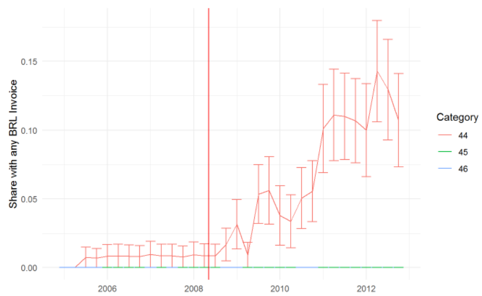
(f) Section 6: Chemical Products



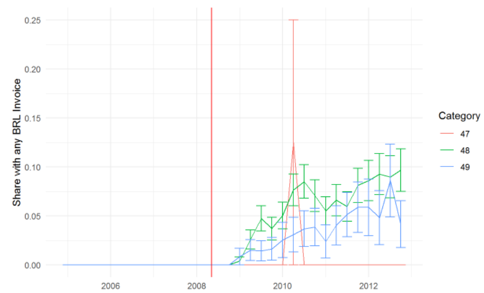
(g) Section 7: Plastic and Rubber Products



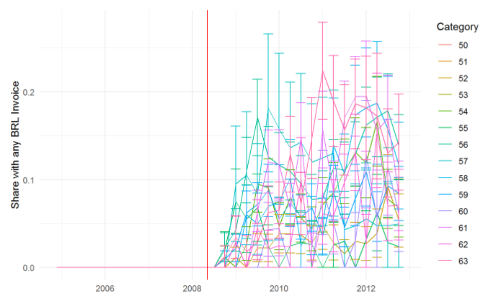
(h) Section 8: Raw Hides



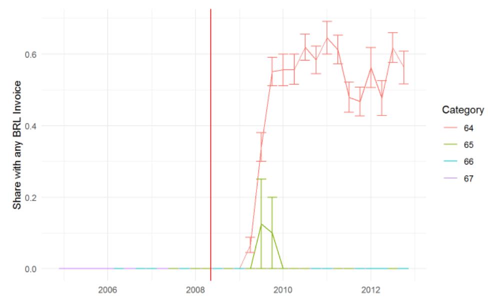
(i) Section 9: Wood Products



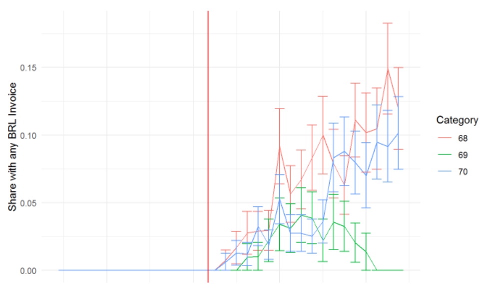
(j) Section 10: Paper Products



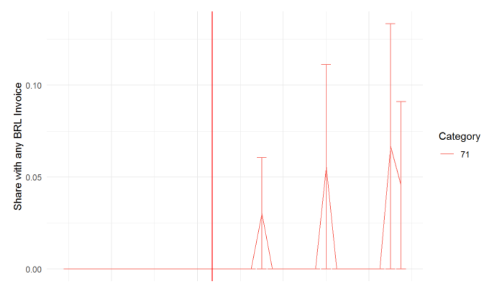
(k) Section 11: Textiles



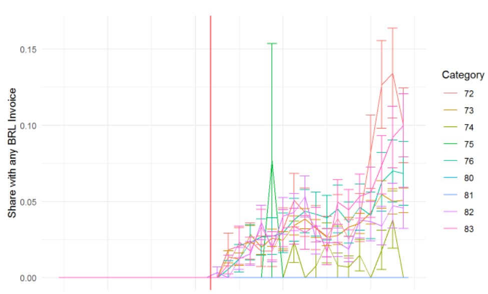
(l) Section 12: Footwear



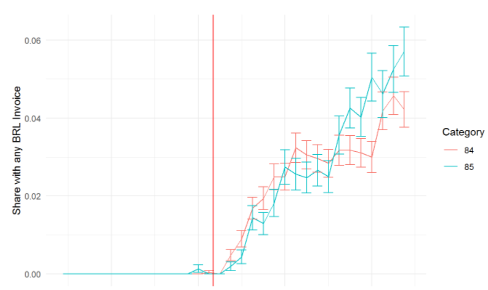
(m) Section 13: Stone



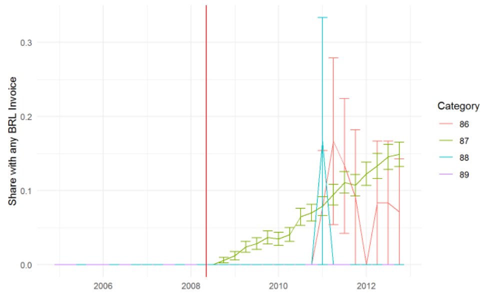
(n) Section 14: Precious Metals



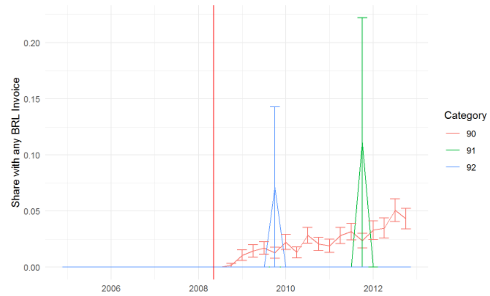
(o) Section 15: Base Metals



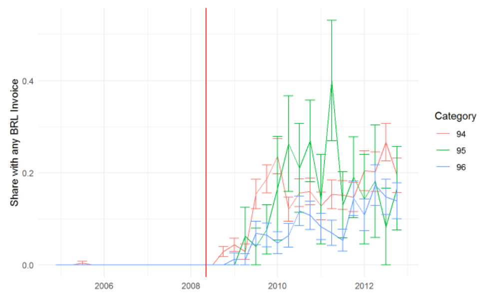
(p) Section 16: Machinery



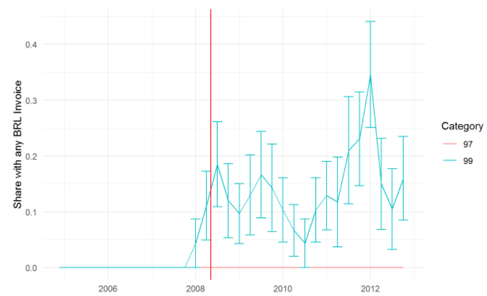
(q) Section 17: Transportation



(r) Section 18: Optical



(s) Section 20: Misc. Manufacturing



(t) Section 21: Art and Other

The first-order condition can be written succinctly as

$$0 = \frac{S_X^\ell}{1+\tau_X}\left(P^\ell \frac{\partial Q}{\partial P^\ell} + Q\right) - C'(Q)\frac{\partial Q}{\partial P^\ell}$$

Recall that demand and costs are given by

$$\begin{aligned} Q^D =& (S_M^\ell P^\ell (1+\tau_M))^{-\rho}\mathbf{X}, \quad \rho > 1 \\ C(Q) =& W Q^{\frac{1}{\alpha}} \end{aligned}$$

Plugging these in and rearranging gives the optimal price as

$$P^\ell = \frac{(1+\tau_X)}{S_X^\ell}\frac{\rho}{\rho-1}\frac{W}{\alpha}(Q)^{\frac{1-\alpha}{\alpha}} \tag{A.1}$$

Because firms commit to supplying as much product as possible at the foregoing price, plugging in for demand into equation (A.1) under this specification and rearranging gives the equilibrium price

$$P^\ell = \left[\frac{(1+\tau_X)/S_X^\ell}{(S_M(1+\tau_M))^{\rho\frac{1-\alpha}{\alpha}}}\frac{\rho}{\rho-1}\frac{W}{\alpha}\mathbf{X}^{\frac{1-\alpha}{\alpha}}\right]^{\frac{1}{1+\rho\frac{1-\alpha}{\alpha}}}$$

The equilibrium quantity can be solved for similarly as

$$Q = \left((1+\tau_X)/S_X^\ell (1+\tau_M) S_M^\ell \frac{\rho}{\rho-1} \frac{Q}{\alpha} X^{-1/\rho} \right)^{\frac{-\rho}{1+\rho\frac{1-\alpha}{\alpha}}}$$

Exporter Risk Aversion

There exist a set of risk-averse exporters, indexed by ω. Profits in a given invoice currency ℓ, $\pi^\ell(\omega)$, are given by the difference between revenues and costs[1]

$$\pi^\ell(\omega) = (S_\ell p^\ell(\omega) - C(\omega))Q(\omega, \ell) \tag{A.2}$$

where $p^\ell(\omega)$ is the price in currency ℓ, $C(\omega)$ denotes constant marginal costs for firm ω and $Q(\omega, \ell)$ is total production. These costs depend only on ω and not the invoice currency ℓ. S_ℓ is the exporter's exchange rate relative to currency ℓ, such that an increase in S_ℓ is a depreciation of the exporters' currency.

I assume that exporters are risk averse. Following Mann (1989), I assume firms' objective functions are the sum of expected profits $E[\pi^\ell(\omega)]$ and the negative of the standard deviation of profits $[V(\pi^\ell(\omega))]^{1/2}$, with the latter scaled by $\gamma > 0$.[2] Firms choose prices to maximize this objective function subject to demand and the definition of profits Equation (A.2).

$$\max_{p^\ell} E[\pi^\ell(\omega)] - \gamma[V(\pi^\ell(\omega))]^{1/2} \text{ s.t. } Q(\omega) = \mathcal{D}(\omega, \ell)$$

where $\mathcal{D}(\omega, \ell)$ denotes the demand function for the exporter's good.

Solving this equation gives the optimal price as

$$p^\ell(\omega) = \frac{1}{1-\gamma\sigma_\ell} \frac{\eta_D(\omega, \ell)}{\eta_D(\omega, \ell) - 1} C(\omega) \tag{A.3}$$

where σ_ℓ is the standard deviation of the exchange rate and $\eta_D(\omega, \ell)$ is the elasticity of demand. With exporter risk-aversion, a higher variability of the relevant exchange rate directly affects the price charged by the exporter. Specifically, higher exchange rate volatility is associated with higher prices. Intuitively, the exporter must be compensated for bearing some risk. Here, $1 + \tau_X = \frac{1}{1-\gamma\sigma_\ell}$.

[1] As this section is meant to be illustrative regarding the effects of the SML system, I assume the invoicing decision is exogenous. I therefore solve the exporter problem by finding the optimal price conditional on currency ℓ.

[2] This ad-hoc assumption ensures that there is an analytical solution due to the result that risk scales linearly with demand. The resulting solution is therefore easily interpretable, but should be seen as qualitative.

Imperfect Markets in Foreign Currency

A representative importer purchases quantity $X(\omega)$ of varieties ω that are bundled according to a CES production technology. Cost minimization over the import bundle is

$$\min \int F_\ell P^\ell(\omega) X(\omega) d\omega$$
$$s.t. X = \left(\int X(\omega)^{\frac{\rho-1}{\rho}} d\omega\right)^{\frac{\rho}{\rho-1}}$$

The price of an individual variety involves two components. First, there is the invoice price $P^\ell(\omega)$, taken as given by the importer. Second, if the variety is invoiced in foreign currency, there is the exchange of local currency to currency ℓ, given by the exchange rate F_ℓ, such that an increase in F_ℓ is a depreciation of the local currency.

The demand for any individual variety can be written as

$$X(\omega) = \left(F_\ell P^\ell(\omega)\right)^{-\rho} \mathbf{P}^\rho X$$

where $\mathbf{P} = \left(\int (F_\ell P^\ell(\omega))^{1-\rho} d\omega\right)^{\frac{1}{1-\rho}}$ is the price index. I assume that a sufficiently small number of varieties are invoiced in foreign currency such that changes in the overall price index due to changes in F_ℓ are close to zero, and so can be ignored.

Financial intermediaries convert domestic currency to foreign currency. There exists a monopoly supplier of foreign currency that can access global exchange rates. The monopoly supplier is small relative to global markets, and so can purchase or sell as much currency as possible at the prevailing market exchange rate.

Financial intermediaries choose the exchange rate F_ℓ to maximize profits given by

$$\Pi = F_\ell Q - S_\ell Q$$
$$\text{s.t. } Q = \int P^j(\omega) X(\omega) d\omega = (F_\ell)^{-\rho} \int \left(P^\ell(\omega)\right)^{-\rho} \mathbf{P}^\rho X$$

where S_ℓ is the market exchange rate that only the financial intermediary has access to. The nominal amount of foreign currency to purchase is given by $Q = \int P^j(\omega) X(\omega) d\omega$.

The optimal F_ℓ posted by the foreign currency supplier is

$$F_\ell = \frac{\rho}{\rho - 1} S_\ell$$

Demand for an individual product can then be written as

$$X(\omega) = \left(\frac{\rho}{\rho - 1} S_\ell P^\ell(\omega)\right)^{-\rho} \mathbf{P}^\rho X$$

Here, $1 + \tau_M = \frac{\rho}{\rho-1}$ is given by the markup charged by financial intermediaries. The SML system acts to potentially lower this markup by allowing firms to access the market exchange rate. The posted price by the exporter includes a markup by the financial sector, which the importer interprets as a higher overall price.

Foreign Currency Loans

Importers are either retailers or domestic producers, who purchase some quantity of the final good X at price in invoice currency P^ℓ, where ℓ denotes the currency of invoicing. To purchase this quantity, importers must borrow in advance the amount in the invoice currency, $B = P^\ell X$. The interest on the loan is given by r_ℓ, signifying that it is the cost of borrowing in currency ℓ. This loan is then repaid with interest and accounting for realized movements in exchange rates at the end of the period, when sales are realized.

Formally, the total cost of this loan is given by the difference between what is realized (principal and interest adjusted for the exchange rate) minus the initial principal borrowed.

$$\left((1+r_\ell)\left(\frac{\Delta S'_\ell}{S_\ell}+1\right)-1\right)B \approx \left(r_\ell + \frac{\Delta S'_\ell}{S_\ell}\right)B$$

Suppose that the importer is perfectly competitive and sells output Q at domestic price $\tilde{P}$ with production technology given by $Q = X^\alpha L^{1-\alpha}$, where $\alpha \in (0,1)$ and L denotes domestic input. Letting W denote the cost of the domestic input, cost minimization can be written as

$$\min \left(1+\left(r_\ell + \frac{E\Delta S'_\ell}{S_\ell}\right)\right)P^\ell X + WL$$
$$\text{s.t. } Q = X^\alpha L^{1-\alpha}$$

where the expectations operator enters because the firm makes production decisions prior to the realization of the exchange rate.

The first-order condition for an interior solution can be written as

$$\frac{\left(1+\left(r_\ell + \frac{E\Delta S'_\ell}{S_\ell}\right)\right)P^\ell}{W} = \frac{\alpha}{1-\alpha}\frac{L}{X}$$

Plugging back into the production function gives

$$Q = X^\alpha \left(\frac{\left(1+\left(r_\ell + \frac{E\Delta S'_\ell}{S_\ell}\right)\right)P^\ell}{W}\frac{1-\alpha}{\alpha}X\right)^{1-\alpha}$$

Rearranging gives the conditional import demand function as

$$X = \left(1+\left(r_\ell + \frac{E\Delta S'_\ell}{S_\ell}\right)\right)^{\alpha-1}\left(\frac{W}{P^\ell}\right)^{1-\alpha}\tilde{\alpha}Q$$

where $\tilde{\alpha} = \left(\frac{\alpha}{1-\alpha}\right)^{\alpha-1}$. Here $1+\tau_M = \left(1+\left(r_\ell + \frac{E\Delta S'_\ell}{S_\ell}\right)\right)^{\alpha-1}$. Ignore the cost of financing r_ℓ, which is a common output in working capital assumptions such as this framework. Expected depreciations in local exchange rates affect demand for imports by directly increasing their marginal cost. The importer takes into account such potential price changes when choosing to purchase from an exporter. If the currency is expected to depreciate, the importer interprets it as a higher cost.

Appendix B

Appendix to Financial Failures and Depositor Quality: Evidence from Building and Loan Associations

B.1 Logit Specification

I present results assuming a logit specification for Equation (1) in the main text. I assume there exists a latent variable y_i^* such that

$$Failure_i = \begin{cases} 1 & y_i^* > 0 \\ 0 & \text{otherwise} \end{cases}$$
$$y_i^* = \alpha + \beta Dayton_i + \Gamma X_i + \eta_i$$

I assume that the error term η_i is distributed by the standard logistic distribution, and estimate this equation via maximum likelihood. The results of this estimation, presented in Table B.1 are shown as odds ratios. The coefficient estimate in Column (1) implies that Dayton institutions are two and a half times more likely to fail compared with Permanent institutions. As before, the point estimate is stable when including the same sets of controls as in the table in the main text, as shown in Columns (2)-(6).

Table B.1: Failure Rates: Logit Model

	Failure	Failure	Failure	Failure	Failure	Failure
Dayton Plan	3.200**	3.259**	3.205**	3.006**	3.077**	2.942*
	(1.658)	(1.702)	(1.699)	(1.568)	(1.616)	(1.635)
Log Assets		0.953				0.964
		(0.125)				(0.267)
Log Members			0.997			1.035
			(0.136)			(0.312)
Share Cash				1.031		1.028
				(0.0373)		(0.0408)
Constant	0.156***	0.292	0.159**	0.142***	0.125***	0.148
	(0.0754)	(0.511)	(0.145)	(0.0713)	(0.0629)	(0.345)
N	163	163	163	163	163	163
Pseudo R-Squared	0.03	0.03	0.03	0.03	0.05	0.06
Large City FE	N	N	N	N	Y	Y

B.2 Dropping 1929 Closures

I present results dropping all B&L's that closed in 1929. Table (B.2) presents the results of this estimation. The results are quantitatively similar to Table (3.5).

Table B.2: Failure Rates: Linear Probability Model Dropping 1929 Closures

	Failure	Failure	Failure	Failure	Failure	Failure	Failure
Dayton Plan	0.170^{**}	0.172^{**}	0.163^{**}	0.165^{**}	0.149	0.136^{*}	0.292
	(0.0686)	(0.0698)	(0.0717)	(0.0702)	(0.130)	(0.0752)	(0.227)
Log Assets		-0.00655				-0.0503	-0.451^{***}
		(0.0271)				(0.0620)	(0.0737)
Log Members			0.0126			0.0567	0.253^{**}
			(0.0283)			(0.0672)	(0.0954)
Share Cash				0.00283		0.000604	0.00000168
				(0.00773)		(0.00780)	(0.0144)
Constant	0.114^{**}	0.199	0.0385	0.106^{*}	0.184^{*}	0.436	4.290^{***}
	(0.0541)	(0.354)	(0.173)	(0.0585)	(0.109)	(0.478)	(0.814)
N	151	151	151	151	104	151	18
R-Squared	0.03	0.03	0.03	0.03	0.21	0.06	0.56
City FE	N	N	N	N	Y	N	N
Large City FE	N	N	N	N	N	Y	N

Robust Standard Errors in Parentheses $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. City includes Los Angeles, Oakland, and San Francisco. Source: Building and Loan Commissioner of the State of California (1927,1935)

B.3 Ordered Logit Specification

I have assumed that the timing of failure is irrelevant. However, it may be the case that permanent/serial plans failed earlier than Dayton plans due to the Great Depression, but Dayton plans failed over time as deflation raised interest rates. If this is true, the finding that Dayton plans fail more may be spurious. To estimate whether or not Dayton plans failed earlier than Permanent/Serial plans, I follow Postel-Vinay (2016) in estimating an ordered logistic model.

$$FailureOrder_i^* = \{j; \kappa_{j-1} \leq y^* \leq \kappa_j\} \tag{B.1}$$

$$y^* = \beta Dayton_i + \Gamma X_i + \varepsilon_i \tag{B.2}$$

where κ_j are estimated cutoff value. $FailureOrder_i$ is an ordered variable of failure for institution i as described below. y^* is a latent variable estimated as the linear combination

Table B.3: Classification of Failure Timing for Ordered Logit

Year	Value	Permanent	Dayton	Total
1929	2	2	10	12
1930	3	4	13	17
1931	4	1	10	11
1932	5	0	3	3
1933	6	1	2	3
1934	7	0	2	2
1935	8	0	7	7
Survive	9	29	79	94
Total		39	144	183

Sources: Building and Loan Commissioner of the State of California (1927)

of controls. The main variable of interest, $Dayton_i$ is the type of the institution (Dayton vs. non-Dayton), and $\mathbf{X}_i$ is a vector of additional controls.

Here, the dependent variable $Fail\_Order_i$ is no longer simply a dummy variable indicating failure. Instead, this variable is equal to the number of years an institution survives from 1927 through 1935.[1] For example, if a bank is alive in 1927 but fails in 1930, this value is 3. If it survives into 1936, then it takes on the highest value of 9. Table B.3 shows the distribution of banks in this way, broken down by type. We can see that both Dayton and permanent plans had high rates of failure early on in the Depression before settling down a bit. At first glance, it also looks like Dayton plans not only had higher failure rates throughout the time period, but also had a peak slightly earlier, in 1929.

The results of estimating Equation B.1 are shown in Table B.4. Coefficients are again expressed as odds ratios. Column (1) shows, consistent with earlier results, that Dayton B&L's had a significantly lower chance of surviving longer into the Recession than Dayton B&L's. The point estimate implies that the odds of a Dayton plan surviving another year is 0.34 times than that for permanent plans. This result is stable when including other controls, shown in the remaining columns.

B.4 Additional Age Regressions

In this section, I estimate the benchmark regression (3.1) excluding institutions formed after x years prior to my sample beginning. For example, the baseline analysis assumes that all institutions formed after 1927 are eliminated, so $x = 0$. I vary this x from 0 to 10, and run the same univariate regression Equation (1) in the main text. Figure B.1 plots the coefficients with 95% standard error bands, as well as the number of institutions in the sample.

[1]With these values, κ_{-1} and κ_9 are equal to minus infinity and infinity, respectively.

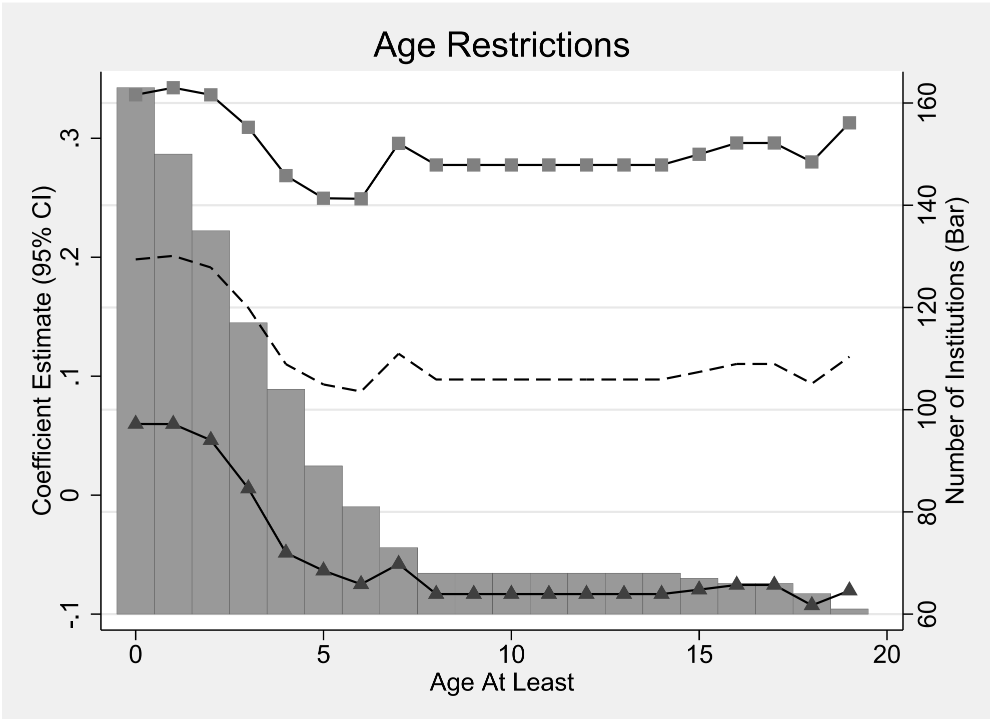
Table B.4: Ordered Logit Specification

	Failure	Failure	Failure	Failure	Failure	Failure
Dayton Plan	0.515	0.560	0.516	0.539	0.586	0.682
	(0.238)	(0.269)	(0.241)	(0.254)	(0.277)	(0.346)
City		0.390^{***}				0.404^{**}
		(0.131)				(0.142)
Log Assets			0.996			1.009
			(0.118)			(0.202)
Log Members				0.928		0.904
				(0.113)		(0.205)
Share Cash					0.935	0.932
					(0.0393)	(0.0405)
N	163	163	163	163	163	163
Chi Squared	2.060	10.21	2.085	2.706	4.531	13.88

Robust Standard Errors in Parentheses $* \, p < 0.1$, $** \, p < 0.05$, $*** \, p < 0.01$. Exponentiated Coefficients. Age calculated as number of years open as of 1927. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. Source: Building and Loan Commissioner of the State of California (1927,1935)

The first point, at $x = 0$, shows the point estimate from the main table in the main text. The bar behind this estimate shows the number of institutions, 171. Moving to the right from this point, we see that the point estimate falls by half as institutions get eliminated to around 0.1, before stabilizing. The largest drop is between 3 and 4. The standard errors also slightly widen. While the point estimate now includes zero, the fact that it is relatively stable after falling provides some comfort that the Dayton plans were not failing solely because of their age.

Figure B.1: Coefficient Restricting Age



B.5 Including Federalization

In the baseline sample, I chose to drop B&L's that are federalized rather than fail or stay open as state institutions. The reason for this is because it is not clear why B&L's choose to federalize. It could be that weak B&L's that might have failed chose to federalize because of additional access to liquidity. On the other hand, strong B&L's may have federalized because, as explained by Snowden (2003), much of the legislation was written by the B&L operators of the time period.

Because of the unknown relative ordering in terms of the outcomes, specifically with respect to the decision to federalize vs. fail, I estimate a multinomial logit model.

$$Pr(Result_i \in \{Fail, Federalize\}) = \alpha + \beta DAYTON_i + \Gamma X_i + \varepsilon_i$$

The results are presented in in Table B.5. The base level is staying open, so all coefficients should be interpreted as the relative risk ratio of the listed result happening relative to staying open for a given change in the independent variable. The independent variable of interest is $DAYTON_i$. For failed institutions, as above, we see that the effect of being a Dayton institution increases the probability of failure. However, there is no significant effect of being a Dayton institution on the probability of being federalized. If anything, being a Dayton plan reduces the probability of federalizing. I interpret these results not as saying that federalization was completely random, but rather the decision to federalize was unrelated to the institution's liability structure.

Table B.5: Including Federalization as an Outcome(Multinomial Logit Results)

	Failure	Failure	Failure	Failure	Failure	Failure
Fail						
Dayton Plan	2.825**	2.780**	2.893**	2.841**	2.617**	2.611**
	(1.278)	(1.284)	(1.302)	(1.296)	(1.193)	(1.272)
City		2.758***				2.702***
		(0.933)				(0.951)
Log Assets			0.940			1.000
			(0.113)			(0.238)
Log Members				0.991		0.993
				(0.120)		(0.244)
Cash (% Assets)					1.044	1.038
					(0.0353)	(0.0371)
Federalize						
Dayton Plan	0.360	0.353	0.362	0.399	0.438	0.596
	(0.304)	(0.302)	(0.311)	(0.330)	(0.360)	(0.491)
City		3.227				3.219
		(2.764)				(2.520)
Log Assets			0.985			1.650
			(0.173)			(0.865)
Log Members				0.826		0.478
				(0.0962)		(0.242)
Cash (% Assets)					0.841	0.839*
					(0.108)	(0.0844)
N	189	189	189	189	189	189
Chi Squared	7.454	16.10	8.015	8.482	10.46	26.68

Robust Standard Errors in Parentheses $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$. Exponentiated Coefficients. Failure is a dummy variable equal to one if a Building and Loan Association were absorbed, closed, consolidated, or transferred. City includes Los Angeles, Oakland, and San Francisco.Source: Building and Loan Commissioner of the State of California (1927,1935)

Appendix C

Appendix to Unemployment Effects of Stay-at-Home Orders: Evidence from High Frequency Claims Data

C.1 Additional Empirical Results

Panel Specification

One concern with the cross-sectional specifications is that there may be some unobserved aggregate factor that induced large increases in UI claims at the same time that states and local municipalities implemented SAH orders. Alternatively, there may be time-invariant state-specific factors that drove both increases in unemployment claims and SAH orders. To address these concerns, we employ a panel specification, which allows us to control for week and state fixed effects.

We modify the specification so that the outcome variable is the flow value of initial claims on date t and the SAH order treatment is the share of the *current week* that a state was subject to SAH orders, where we take a weighted average of county-level exposure as before.[1]

$$\frac{UI_{s,t}}{Emp_s} = \alpha_s + \phi_t + \beta_P \times SAH_{s,t,t-7} + \mathbf{X}_{s,t}\Gamma + \epsilon_{s,t} \tag{C.1}$$

We consider a variety of state-time controls. We include two lags of $SAH_{s,t,t-7}$ to account for dynamics in the effect of SAH orders on unemployment claims. Additionally, we include the share of the population that works from home, the number of confirmed cases per one thousand people, and the Bartik-style employment control from before. Each of these three controls is interacted with a dummy equal to one for weeks ending March 21st, 2020 and

[1] Because in our sample no state or local municipality reopened, once $SAH_{s,t,t-7} = 1$ it remains equal to one for all remaining weeks.

onward.[2] We estimate the following fixed effects panel regression on weekly observations for the week ending January 4 through the week ending April 11.[3]

Table A.1 provides our estimate of $\hat{\beta}_P$ for the contemporaneous effect and two lags. Column (1) presents the results with no lags. The point estimate of 0.90% (SE: 0.35%) suggests that a full week of SAH order exposure increased unemployment claims by .90% of total state-level employment. In column (2), we include two lags of SAH orders. The point estimate on the contemporaneous effect is little changed, though it rises slightly. Importantly, neither of the coefficients on the first nor the second lag is significant. This result suggests that, in our sample, that SAH orders have constant, contemporaneous effects on UI claims. At longer horizons, we would suspect non-linearities to eventually kick in, with the effect of SAH orders declining. Finally, our point estimates are little changed when including additional controls in Column (3).

Our estimates $\hat{\beta}_P$ in the first three columns tend to be somewhat lower than what we find in our benchmark, cross-sectional design. In particular, the panel design implies that each week of SAH exposure increased UI claims by 1% of state employment; in contrast, our estimates of $\hat{\beta}_C$ imply that each week of SAH exposure increased UI claims by approximately 1.9% of state employment. While, at first glance, β_C and β_P aim to estimate the same moment, the inclusion of state and time fixed effects imply that they are not directly comparable.[4] In column (4), we consider the panel specification in which we drop state fixed effects, to make the panel and cross-sectional regressions comparable: the point estimate rises to 1.2% and is statistically indistinguishable from what we find in the cross-section.

High Frequency Effects on Proxies for Local Economic Activity

In this subsection, we provide additional evidence that the SAH orders had immediate and highly localized effects on daily indicators of economic activity. This exercise is important because of concerns that the state-level effects we estimate above simply reflect differential labor market disruptions that would have occurred in the absence of SAH orders in precisely those places most likely to implement SAH orders earliest.

We estimate the local effect of SAH using high frequency proxies for economic activity from Google's Community Mobility Report, which measures changes in visits to establishments in various categories, such as retail and work.[5] Early on in the COVID-19 pandemic, Google began publishing data documenting how often its users were visiting different types of establishments. The data are reported as values relative to the median visitation rates by

[2]Note that because our measures of work-from-home and employment loss are constant across time, we are controlling for the relative effect of each from before the week ending March 21st.

[3]We drop the first two weeks in all specifications to ensure the sample size is constant throughout.

[4]See Kropko and Kubinec (2020) for a discussion of the proper interpretation of two-way fixed effect estimators in relation to one-way fixed effect estimators.

[5]https://www.google.com/covid19/mobility/

week-day between January 3, 2020 and February 6, 2020.[6],[7]

We use the retail and workplace mobility indices because these two indices are consistently recorded for the time sample we study. Failing to find an effect on these proxies for local economic activity would call into question the results we find in the aggregate, at the state-level. We interpret retail mobility as broadly representing "demand" responses to SAH orders and workplace mobility as broadly representing "supply," at least on-impact.[8] Over longer-horizons, workers laid off because of demand-side disruptions will, naturally, cease commuting to and from work.

Formally, we estimate event studies of the following form:

$$Mobility_{c,t} = \alpha_c + \phi_{CZ(c),t} + \sum_{k=\underline{K}}^{\overline{K}} \beta_k SAH_{c,t+k} + X_{c,t} + \underline{D}_{c,t} + \overline{D}_{c,t} + \varepsilon_{c,t} \qquad \text{(C.2)}$$

where $Mobility_{c,t}$ represents either the retail or workplace mobility index published by Google for county c on day t, and $SAH_{c,t}$ is a dummy variable equal to 1 on the day a county imposes SAH orders. We set $\underline{K} = -17$ and $\overline{K} = 21$ so that the analysis examines three weeks prior and two and a half weeks following the imposition of SAH orders.[9] The event study is estimated over the period February 15th through April 24th, 2020. We non-parametrically control for county size by discretizing county employment into fifteen equally sized bins and interacting each bin with time fixed effects. α_c refers to the inclusion of county fixed effects. To isolate the local effect of SAH orders on economic activity, we also include commuting zone-by-time fixed effects.[10] This implies that our event-study estimates are identified only off of differential timing of SAH implementation among counties contained within the same commuting zone.

Results for retail mobility are presented in Figure A.1. The day SAH orders went into effect, there was an immediate decline of approximately 2% in retail mobility. This falls further to 7% the day after SAH order implementation, before slowly recovering to approxi-

[6]One possible limitation of this data is that the sample of accounts included in the surveys is derived from only those with Google Accounts who opt into location services. We believe sample selection bias is unlikely to be a major concern given Google's broad reach (there are over 1.5 billion Gmail accounts, for example).

[7]Note that for privacy reasons, data is missing for some days for some counties. When possible, we carry forward the last non-missing value. Excluding counties with missing values yields the same result; this figure is available from the authors upon request.

[8]Of course, both indicators are equilibrium outcomes of both supply and demand shocks. The on-impact effect on work-place mobility at the very least reflects disruptions to each firm's ability to produce. Similarly, the on-impact effect on retail mobility is indicative of a decline in retail demand by consumers since, presumably, the supply of retail goods is at least fixed in the very short-run.

[9]Because our sample is necessarily unbalanced in event-time, we also include "long-run" dummy variables, $\underline{D}_{c,t}$ and $\overline{D}_{c,t}$. $\underline{D}_{c,t}$ is equal to 1 if a county imposed SAH orders at least $\overline{K}$ days prior. $\overline{D}_{c,t}$ is equal to 1 if a county will impose SAH at least $\underline{\text{K}}$ periods in the future.

[10]We use the United States Department of Agriculture (USDA) 2000 county to commuting zone crosswalk. This is available at https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/.

mately 2% lower retail mobility two and a half weeks following the SAH order imposition.[11] The large transitory dip may reflect sentiment among consumers to shut-in before revisiting grocery stores and pharmacies. Alternatively, given our inclusion of commuting zone-by-time fixed effects, the transitory nature of the shock may reflect negative, within-labor market spillovers of SAH orders. Regardless, the lack of a pre-trend is noticeable and provides additional support for a causal interpretation.

SAH orders may have affected firms' ability to produce by preventing workers from accessing their places of employment. To investigate whether SAH orders may have affected firms' productive capacity through this channel, we re-estimate our event study using workplace mobility as the outcome variable.[12]

Figure A.2 shows the result. As with the retail mobility event study, the workplace mobility index exhibits no differential pre-trend prior to the county-level imposition of SAH orders. In the first two days following the imposition of SAH orders, workplace mobility declined sharply relative to non-treated counties within its commuting zone. This relative decline in workplace mobility persists for nearly two and a half weeks following.

We draw three conclusions from these high-frequency event studies. First, the lack of pre-trends in the event studies suggest that the timing of SAH orders can be seen as plausibly randomly assigned with respect to local labor market conditions. This provides corroborating evidence for our cross-sectional identification strategy. In particular, it suggests that there were real effects of the SAH orders on local economies. Second, with the important caveat that both mobility indices are equilibrium objects, SAH orders appear to have had *both* local supply and local demand effects. Both retail mobility and workplace mobility fell substantially on impact and remained persistently low for at least two weeks following implementation of SAH orders. Third, given that overall workplace and retail mobility in the U.S. fell by 48 and 40 percent through April 24th relative to their baseline levels, our results bolster the claim that alternative mechanisms were responsible for the majority of job losses in the early weeks of the crisis; upon SAH implementation, relative workplace and retail mobility fell by, at most, 2 and 7 percent, respectively.

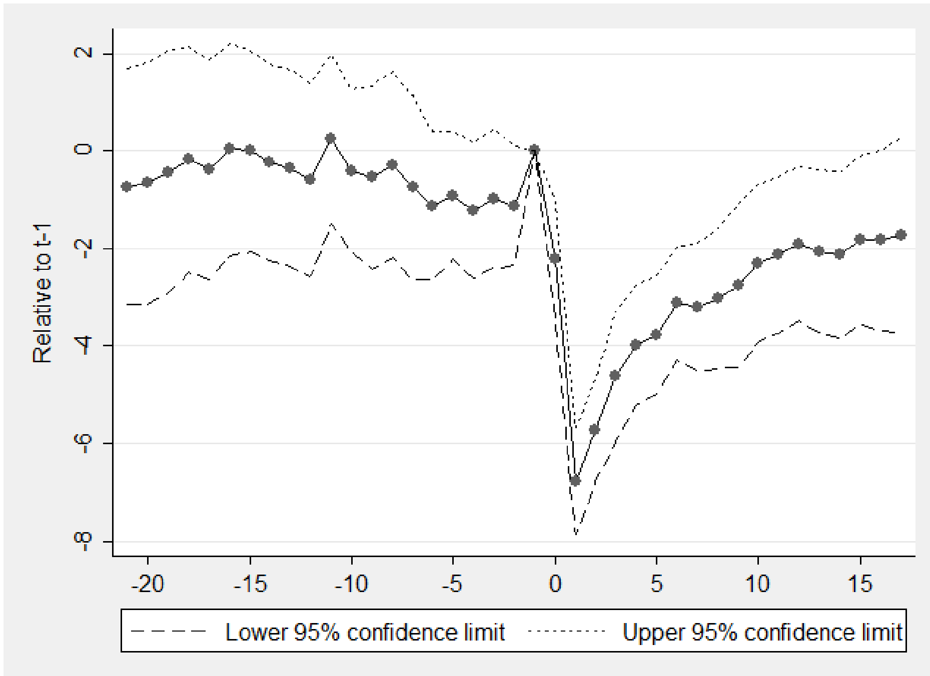
Alternative Cross-Sectional Specifications

The first type of robustness check we do is varying the horizon over which the cross-sectional regression is estimated, considering two natural alternative specifications: a two week horizon and a four week horizon. For the two week horizon specification, we consider cumulative

[11] Restricting the sample to exclude never-takers yields the same result. This design identifies the mobility effects off of counties that ultimately implemented SAH orders but at different times.

[12] An obvious concern with simply replacing the outcome variable is that changes in workplace mobility, unlike retail mobility, is highly dependent on the ability of individuals to work from home. The timing of SAH orders may be partially driven by the ability of workers in some regions to transition to working at home. In unreported regressions, we also non-parametrically control for this possibility by partitioning the WAH variable into 15 equally sized bins and interacting each bin with time fixed effects. The event study is essentially unchanged.

Figure A.1: County Retail Mobility Event Study



This figure plots estimated coefficients from the county-level, event-study specification in equation (C.2), where coefficients have been normalized relative to one day prior to county-level SAH orders went into effect. The model includes as controls county fixed effects, commuting zone-by-time fixed effects, and indicators for county employment bins interacted with time to non-parametrically control for county size. The outcome variable is the retail mobility index published in Google's Community Mobility Report. This index is constructed using visits and duration of visits to retail establishments. The time unit is days.

Standard Errors: Two-Way Clustered by County and Day

Sources: Google, the *New York Times*; Census Bureau; United States Department of Agriculture; Authors' Calculations

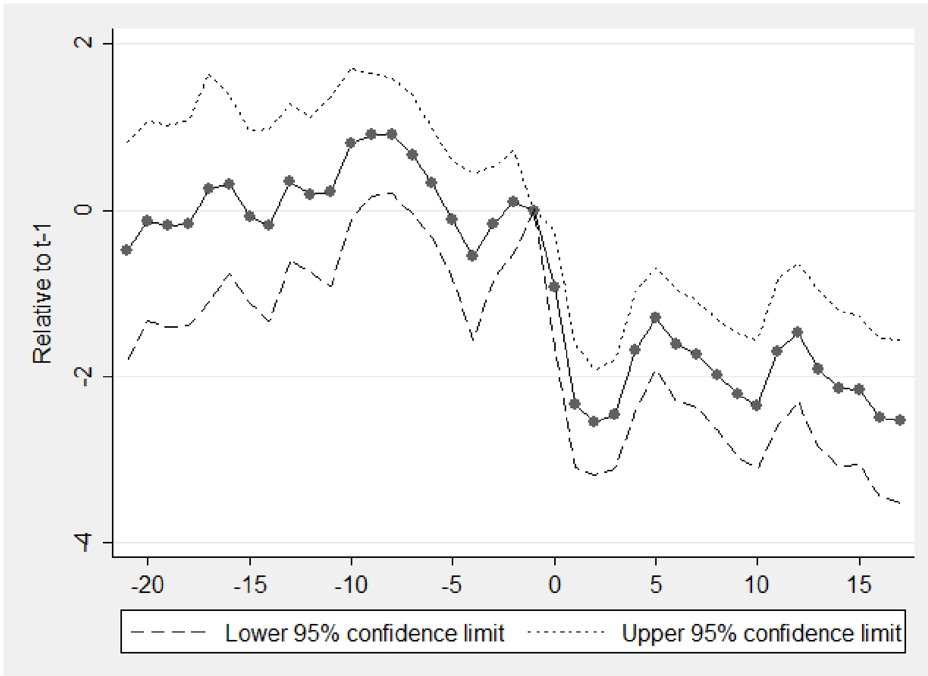
initial claims between March 14 and March 28 regressed on SAH exposure over the same window; for the four week specification, the end date is April 11. We include the same set of controls as in our benchmark specification (Table 4.1, Column (5)).

Columns (1) and (2) of Table A.2 report the results from varying the horizon over which the model is estimated. Relative to our baseline result of 1.9%, estimating the model over just two weeks lowers the point estimate slightly to 1.83% (SE: 0.91%). Conversely, when the model is estimated over a four week horizon, the point estimate is 1.7% (SE: 0.59%).

In Column (3) of Table A.2 we estimate the effect of SAH exposure on UI claims, over the same three week horizon as in the benchmark case, weighting observations by state-level employment from the QCEW in 2018 (an approached advocated for by some papers in the local multiplier literature).[13] Again, we consider the same set of controls as in our benchmark

[13]For arguments in either direction, see Ramey (2019) and Chodorow-Reich (2020), respectively. See also

Figure A.2: County Workplace Mobility Event Study



This figure plots estimated coefficients from the county-level, event-study specification in equation (C.2), where coefficients have been normalized relative to one day prior to county-level SAH orders went into effect. The model includes as controls county fixed effects, commuting zone-by-time fixed effects, and indicators for county employment bins interacted with time to non-parametrically control for county size. The outcome variable is the workplace mobility index published in Google's Community Mobility Report. This index is constructed using visits and duration of visits to places of employment. The time unit is days.

Standard Errors: Two-Way Clustered by County and Day

Sources: Google, the *New York Times*; Census Bureau; United States Department of Agriculture; Authors' Calculations

specification. The point estimate from the WLS regression is elevated slightly: 2.10% (SE: 0.54%). Regardless, weighting delivers quantitatively similar estimates.

Influence of Specific States

One may also be concerned that individual states' responses, either in terms of rising unemployment claims or SAH orders, is driving our results. To understand whether this is the case, we replicate our benchmark specification (column (5) in Table 4.1) from above, dropping one state at a time. The resulting coefficient estimates for β_C are available in Figure A.3, along with 90 percent confidence intervals constructed from robust standard errors.

Solon et al. (2015).

Figure A.3: Benchmark Specification Estimated Dropping One State at a Time

This figure reports results from estimating equation (4.4): $\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAH_{s,Apr.4} + X_s\Gamma + \epsilon_s$, dropping one state at a time from the estimation. The set of controls, X_s, are those that appear in the benchmark specification (Table 4.1, Column (5))—a parsimonious model that controls for pandemic severity, political economy factors, and state sectoral composition. The dependent variable is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (4.3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

Pre-SAH Determinants of UI Claims

In this subsection, we broaden our analysis to adjust for determinants of state-level UI claims that may have been correlated with the timing of SAH implementation at the local level, as reported by the *New York Times*.

The first change that we make, relative to the results presented in Table 4.1, is to control for the March 7 to March 14 change in consumer spending. Because consumption is a leading indicator, changes to consumer spending tend to precede changes to employment. Thus, this allows us to control for leading determinants—as manifested in changes to state-level consumer spending—of employment losses that may have also been correlated with the timing of the implementation of SAH orders.

To do so, we rely upon the newly available, daily consumer spending index constructed by Chetty et al. (2020). These high frequency indicators of state-level economic activity is constructed from proprietary private sector microdata and made publicly available at https://tracktherecovery.org.

The second adjustment made in this subsection relates to the timing of state-level SAH implementation. In a few notable instances, the closure of non-essential businesses by state and local officials did not coincide with the broader SAH orders requiring all individuals to remain at home except for essential activities.[14] For example, on March 19 the governor of Pennsylvania issued a statewide executive order that required non-essential, in-person business activity to cease. This preceded by nearly a week the full statewide SAH order that was put into effect on March 23. A similar discrepancy between SAH dates and non-essential business closure occurred in Nevada.

This is potentially important since both Pennsylvania and Nevada experienced larger cumulative increases in UI claims to employment than the rest of the country through April 4. If the discrepancy between non-essential business closure and SAH implementation (as reported by the *New York Times*) was systematically correlated with the severity of job losses, then our estimate of β_C may be biased. In particular, if the pattern for Pennsylvania and Nevada holds more generally—large UI claims increase and relatively early non-essential business closure—then our estimates of β_C in Table 4.1 will be biased downwards, leading us to understate both the relative employment effect of SAH orders and their implied aggregate effect.

We adjust for the discrepancy between SAH implementation as reported in the *New York Times* and non-essential business closures by constructing a combined SAH/business closure treatment variable:

$$SAHBIZ_{s,t} = \max \{SAH_{s,t}, BIZ_{s,t}\}, \tag{C.3}$$

where $BIZ_{s,t}$ is the number of weeks state s was subject to a non-essential business closure through date t.[15].

Table A.3 records the results after incorporating the March 7 to March 14 change in the consumer spending index and adjusting the treatment variable to handle discrepancies between reported SAH implementation dates and dates of non-essential business closures. This table is structured identically to Table 4.1 except for the aforementioned changes.

Both qualitatively and quantitatively the effect on unemployment of SAH orders is essentially unchanged relative to the benchmark specification. Consider Column (5): The point estimate of 1.9% (SE: 0.88%) implies that each additional week that a state was subject to a SAH order and/or non-essential business closures increased unemployment claims by 1.9% of the state's employment level.

While this point estimate is the same as our benchmark estimate, the relative-implied aggregate estimate of employment losses due to SAH orders through April 4, 2020 needs to be slightly adjusted. Incorporating non-essential business closure dates weakly increases each state's degree of SAH exposure. Recalculating equation (4.6) with the model estimated in Column (5) of Table A.3 yields an estimate of 4.6 million claims through April 4 attributable to SAH orders or approximately 27% of the overall increase in UI claims over the same period.[16]

[14] The closure of non-essential businesses is a prominent feature of most SAH orders.

[15] We use the state-level non-essential business closure dates compiled in Kong and Prinz (2020).

[16] The two controls we consider in this section each slightly alter the estimated coefficient for the specifi-

County-Level Event Study Employment Specification

In Subsection 4.6 we use BLS-reported, month-to-month changes in county employment and unemployment to estimate the effect of SAH orders after controlling for state fixed effects. In what follows, we use county-level, high frequency employment indices to provide additional evidence that SAH orders had highly localized effects on county-level employment.[17]

Not only is the effect we estimate in this subsection consistent with our central finding, but by using high frequency, county-level data we are able to directly assess our assumption that the timing of local SAH implementation was uncorrelated with the relative severity of the local economic downturn. Consistent with the evidence presented in Subsection C.1, we find no evidence of differential pre-trends in employment around the implementation of SAH orders.

For the subset of counties for which the high-frequency employment indices are available, we estimate the following event study specification:

$$EmpIDX_{c,t} = \alpha_c + \phi_{state(c),t} + \sum_{k=\underline{K}}^{\overline{K}} \beta_k SAH_{c,t+k} + X_{c,t} + \underline{D}_{c,t} + \overline{D}_{c,t} + \varepsilon_{c,t} \tag{C.4}$$

where $EmpIDX_{c,t}$ represents the county-level, employment index available at https://tracktherecovery.org, $SAH_{c,t}$ is a dummy variable equal to 1 on the day a county imposes SAH orders, and $\phi_{state(c),t}$ is a state-by-time fixed effect. As in Subsection C.1, we set $\underline{K} = -17$ and $\overline{K} = 21$; the analysis thus examines three weeks prior and two and a half weeks following the imposition of SAH orders.[18] The event study is estimated over the period February 15th through April 24th, 2020. For this event study specification, we include no additional controls beyond county fixed effects and state-by-time fixed effects.

The results of this exercise are reported in Figure A.4. In the three weeks prior to the implementation of SAH orders, there is no statistically discernible pre-trend in employment.[19]

cation analogous to our benchmark specification. Controlling only for the change in the consumer spending index attenuates the point estimate to 1.4% (SE: 0.80%). Only adjusting for the discrepancies between non-essential business closure dates and reported SAH dates amplifies the point estimate somewhat to 2.4% (SE: 0.68); however, this latter effect appears to be driven almost entirely by Pennsylvania and Nevada. Dropping these states from the estimation yields a point estimate of 1.9% (SE: 0.68). These results are available upon request.

[17] The county-level employment indices we use were constructed by Chetty et al. (2020) and are available at https://tracktherecovery.org The county-level employment statistics we use are built out from anonymized microdata from private companies. See Chetty et al. (2020) for a fuller description of the data construction and for evidence that these series tend to track lower-frequency, publicly available series constructed from representative surveys.

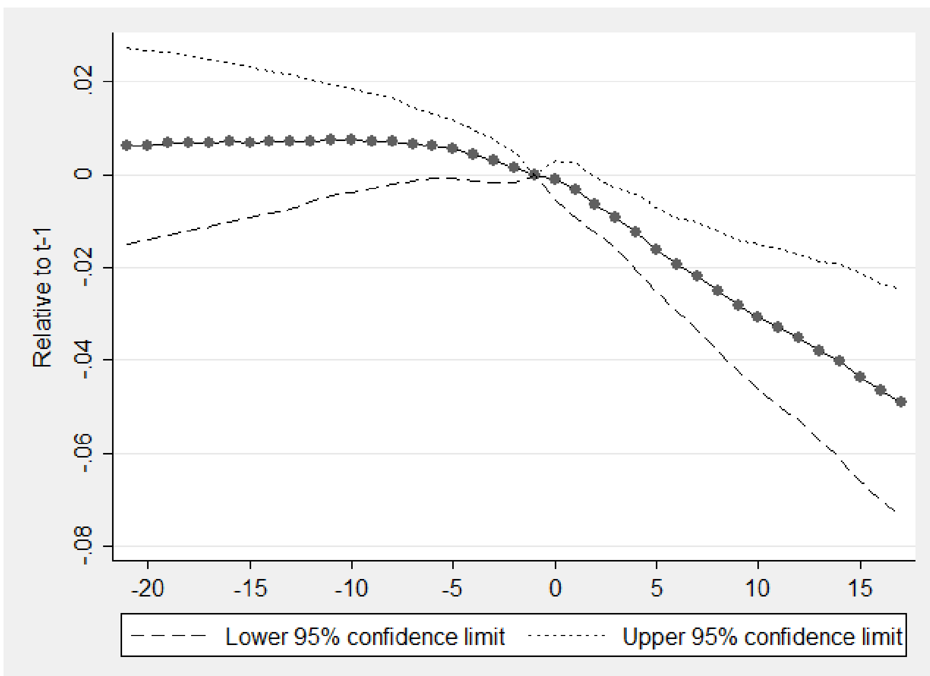
[18] Our sample is necessarily unbalanced in event time, so we include "long-run" dummy variables $\underline{D}_{c,t}$ and $\overline{D}_{c,t}$ which are equal to 1 if a county imposed a SAH order at least $\overline{K}$ days prior or will impose a SAH order at least $\underline{\text{K}}$ days in the future, respectively.

[19] While not statistically meaningful, there appears to be a slight inflection point approximately one week prior to SAH implementation. However, even this is likely a statistical artifact, since the county-level employment statistics we rely upon are primarily reliant upon weekly payroll data from the company

However, there is a clear decline in employment after SAH orders were put into place. By one week following the SAH implementation, the employment index was down by 1.9% (SE: 0.5%). Two weeks following SAH implementation, the county-level index was down by by nearly twice as much.

For this analysis, we rely upon a subset of counties for which we have a high frequency measure of employment changes and for which there exist within-state variation. Nevertheless, despite relying upon a different subset of the variation for identification, the weekly effect on employment we estimate here is remarkably consistent with our state-level analysis, in terms of both magnitude and linearity of the effect. We view this as strongly corroborating our baseline finding and allaying concerns that the timing of SAH implementation was differentially correlated with the severity of each labor markets economic downturn.

Figure A.4: County Employment Event Study



This figure plots estimated coefficients from the county-level, event-study specification in equation (C.4), where coefficients have been normalized relative to one day prior to county-level SAH orders went into effect. The model includes as controls county fixed effects and state-by-time fixed effects. The outcome variable is the county-level employment index available at https://tracktherecovery.org. This index is constructed using anonymized data from private companies; see Chetty et al. (2020) for additional details. The time unit is days.

Standard Errors: Two-Way Clustered by County and Day

Sources: https://tracktherecovery.org, the *New York Times*; Authors' Calculations

Paychex. Chetty et al. (2020) write: We convert the weekly Paychex data to daily measures of employment by assuming that employment is constant within each week.

Table A.1: Panel Specification: Effect of Stay-at-Home Orders on Initial Weekly Claims Relative to State Employment

	(1)	(2)	(3)	(4)
SAH Exposure Current Week	0.00919**	0.0101***	0.00997***	0.0125***
	(0.00350)	(0.00321)	(0.00329)	(0.00353)
SAH Exposure First Lag		-0.00293	-0.00367	-0.00299
		(0.00359)	(0.00358)	(0.00372)
SAH Exposure Second Lag		0.00245	-0.00115	0.000809
		(0.00230)	(0.00302)	(0.00332)
State FE	Y	Y	Y	N
Week FE	Y	Y	Y	Y
Post-March 21 X Work at Home Index	N	N	Y	Y
Post-March 21 X Excess Deaths per 1K	N	N	Y	Y
Post-March 21 X COVID-19 Cases per 1K	N	N	Y	Y
Post-March 21 X Avg. UI Replacement Rate	N	N	Y	Y
Adj. R-Square	0.826	0.822	0.831	0.801
No. Obs.	765	663	663	663

This table reports results from estimating equation (C.1): $\frac{UI_{s,t}}{Emp_s} = \alpha_s + \phi_t + \beta_P \times SAH_{s,t,t-7} + \mathbf{X}_{s,t}\Gamma + \epsilon_{s,t}$, where each column considers a different set of controls X_s. The dependent variable in all columns is weekly initial unemployment claims as a fraction of state employment. The interpretation of the SAH Exposure coefficient ($\hat{\beta}_P$; top row) is the effect on normalized new UI claims of a full week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the share of the current week each county in the state is subject to SAH by the 2018 QCEW average employment share of that county in the state, and summing over each states' counties. UI claims are cumulative new claims during the period, divided by average 2018 QCEW average employment in the state.

Standard Errors Clustered by State in Parentheses

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.2: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment: (i) 2-Week Horizon, (ii) 4-Week Horizon, (iii) Weighted Least Squares

	(1) Thru Mar. 28	(2) Thru Apr. 11	(3) WLS
SAH Exposure (varied horizons)	0.0183^{**}	0.0166^{***}	0.0209^{***}
	(0.00908)	(0.00592)	(0.00541)
COVID-19 Cases per 1K	0.00197	0.000854	-0.00472
	(0.0109)	(0.00463)	(0.00306)
Excess Deaths per 1K	-0.0819	0.0691	0.214^{**}
	(0.0959)	(0.0787)	(0.106)
Work at Home Index	-0.152	-0.587^{**}	-0.486^{+}
	(0.184)	(0.261)	(0.258)
Constant	0.111^{+}	0.303^{***}	0.242^{**}
	(0.0649)	(0.0920)	(0.0921)
Adj. R-Square	0.0125	0.129	0.172
No. Obs.	51	51	51

This table reports results from estimating equation (4.4): $\frac{UI_{s,Mar.21,T}}{Emp_s} = \alpha + \beta_C \times SAH_{s,T} + X_s\Gamma + \epsilon_s$, where columns (1) and (2) estimate the model over horizon T = March 28, 2020 and T = April 11, 2020; column (3) estimates the model with T =April 4, 2020 by weighted least squares, weighting by state employment. In line with our benchmark specification (Table 4.1, Column (5)), in each column we specify a parsimonious model controlling for pandemic severity, political economy factors, and state sectoral composition. The dependent variable in all columns is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (4.3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through T that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

Robust Standard Errors in Parentheses

$+p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A.3: Effect of Stay-at-Home Orders on Cumulative Initial Weekly Claims Relative to State Employment for Weeks Ending March 21 thru April 4, 2020 After Accounting for Additional Pre-SAH Determinants of UI Claims.

	(1) Bivariate	(2) Covid	(3) Pol. Econ.	(4) Sectoral	(5) All
SAH/Business Closure Exposure	0.0214**	0.0218**	0.0215**	0.0224**	0.0191**
	(0.00855)	(0.00916)	(0.00972)	(0.00882)	(0.00884)
Mar. 7 to Mar. 14 Spending Change	-0.158	-0.183	-0.183	-0.310	-0.351
	(0.293)	(0.289)	(0.289)	(0.272)	(0.279)
COVID-19 Cases per 1K		-0.00295			0.00249
		(0.00579)			(0.00592)
Excess Deaths per 1K		0.0537			0.0637
		(0.120)			(0.109)
60+ Ratio to Total Population		0.308			
		(0.266)			
Avg. UI Replacement Rate			0.0740		0.0751
			(0.0764)		(0.0754)
2016 Trump Vote Share			0.00881		
			(0.0589)		
Work at Home Index				-0.500***	-0.563***
				(0.184)	(0.187)
Bartik-Predicted Job Loss				1.219	
				(7.388)	
Constant	0.0743***	0.0144	0.0372	0.259***	0.239***
	(0.0152)	(0.0517)	(0.0536)	(0.0793)	(0.0764)
Adj. R-Square	0.131	0.107	0.106	0.186	0.179
No. Obs.	51	51	51	51	51

This table reports results from estimating a variant of equation (4.4): $\frac{UI_{s,Mar.21,Apr.4}}{Emp_s} = \alpha + \beta_C \times SAHBIZ_{s,Apr.4} + X_s\Gamma + \epsilon_s$, where each column considers a different set of controls X_s. Column (5)—a parsimonious model controlling for pandemic severity, political economy factors, and state sectoral composition—is analogous to our benchmark specification. The dependent variable in all columns is our measure of cumulative new unemployment claims as a fraction of state employment, as calculated in Equation (4.3). The interpretation of the SAH Exposure coefficient ($\hat{\beta}_C$; top row) is the effect on normalized new UI claims of one additional week of state exposure to SAH, broadened to account for occasional discrepancy between non-essential business closure dates and reported SAH dates. The Employment-Weighted exposure to SAH for a particular state is calculated by multiplying the number of weeks through April 4, 2020 that each county in the state was subject to SAH with the 2018 QCEW average employment share of that county in the state, and summing over each states' counties.

Robust Standard Errors in Parentheses

$+\ p < 0.10$, $** p < 0.05$, $*** p < 0.01$

C.2 Local SAH Orders in a Currency Union Model

We develop a framework to help us interpret the "relative effect"—which we estimate in the data—as compared to the "aggregate effect" of stay-at-home orders. To that end, we use a simple version of Nakamura and Steinsson (2014) of a two-country monetary union model, albeit abstracting from government spending as that is not the focus of our paper.

Households

Consider a currency union comprised of two regions: a home region of size n, and a foreign region of size $1-n$. In each region, there are infinitely many households with *identical* preferences and initial wealth.

A household j in home region has the following preferences:

$$\mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t \left[\delta_t \frac{\left(C_t^j\right)^{1-\sigma}}{1-\sigma} - \chi \frac{\left(N_t^j\right)^{1+\psi}}{1+\psi} \right]$$

where

$$C_t^j = \left[\phi_H^{\frac{1}{\eta}} \left(C_{H,t}^j\right)^{\frac{\eta-1}{\eta}} + \phi_F^{\frac{1}{\eta}} \left(C_{F,t}^j\right)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}}, \text{ with } \phi_H + \phi_F = 1,$$

$$C_{H,t}^j = \left(\int_0^n \left(\frac{1}{n}\right)^{\frac{1}{\epsilon}} c_{h,t}^j(i)^{\frac{\epsilon-1}{\epsilon}} \mathrm{d}i \right)^{\frac{\epsilon}{\epsilon-1}}, \quad C_{F,t} = \left(\int_n^1 \left(\frac{1}{1-n}\right)^{\frac{1}{\epsilon}} c_{f,t}^j(i)^{\frac{\epsilon-1}{\epsilon}} \mathrm{d}i \right)^{\frac{\epsilon}{\epsilon-1}}.$$

Total consumption of a household j in a home region is a CES aggregator of a *bundle* of home goods, $C_{H,t}^j$ and a *bundle* of foreign goods, $C_{F,t}^j$. Here, ϕ_F denotes the steady state share of the foreign goods imported from by a household in the home region. When $\phi_H = 1 - \phi_F > n$, there is home bias.[20] η is the elasticity of substitution between home goods and imported goods from a foreign region, and ϵ denotes the elasticity of substitution across differentiated goods. β is discount factor and δ_t denotes consumption-preference shock in a home region, which evolves according to the following law of motion:

$$\log \delta_t = \rho^\delta \log \delta_{t-1} + \epsilon_t^\delta.$$

Then optimal allocations of expenditures (per household) are given by

$$C_{H,t}^j = \phi_H \left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t^j, \quad C_{F,t} = \phi_F \left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t^j,$$

$$c_{h,t}^j(i) = \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon} C_{H,t}^j, \quad c_{f,t}^j(i) = \left(\frac{p_{f,t}(i)}{P_{F,t}}\right)^{-\epsilon} C_{F,t}^j,$$

[20] In the baseline calibration following Nakamura and Steinsson (2014), we calibrate $\phi_H = 0.69$ and $n = 0.1$, so that there is significant home bias.

with price indices defined as follows:

$$P_t = \left[\phi_H P_{H,t}^{1-\eta} + \phi_F P_{F,t}^{1-\eta}\right]^{\frac{1}{1-\eta}},$$
$$P_{H,t} = \left[\frac{1}{n}\int_0^n p_{h,t}(i)^{1-\epsilon}\mathrm{d}i\right]^{\frac{1}{1-\epsilon}},$$
$$P_{F,t} = \left[\frac{1}{1-n}\int_n^1 p_{f,t}(i)^{1-\epsilon}\mathrm{d}i\right]^{\frac{1}{1-\epsilon}}.$$

Here, P_t denotes consumper price index of a home region, and $P_{H,t}$ $(P_{F,t})$ is producer price index of home (foreign) goods.

In our baseline specification, we assume identical households in a given region with the same initial wealth and *complete* financial markets, which makes aggregation straightforward. Thus, we have

$$c_{h,t}(i) \equiv \int_0^n c_{h,t}^j(i)\mathrm{d}j = \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\epsilon} C_{H,t}, \quad c_{f,t}(i) \equiv \int_0^n c_{f,t}^j(i)\mathrm{d}j = \left(\frac{p_{f,t}(i)}{P_{F,t}}\right)^{-\epsilon} C_{F,t}$$
$$C_{H,t} = \int_0^n C_{H,t}^j\mathrm{d}j = \phi_H\left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t, \quad C_{F,t} = \int_n^1 C_{F,t}^j\mathrm{d}j = \phi_F\left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t,$$
$$C_t = \int_0^n C_t^j\mathrm{d}j = nC_t^j,$$

where variables without j superscript are aggregate variables in a home region.

With the optimal allocations, we can write household j's budget constraint (in real terms with the home region's CPI as a numeraire) as follows:

$$C_t^j + \mathbb{E}_t\left[M_{t,t+1}B_{t+1}^j\right] \leq B_t^j + \frac{W_t}{P_t}N_t^j + \int_0^1 \frac{\Xi_{h,t}^j(i)}{P_t}\mathrm{d}i - \frac{T_t^j}{P_t}.$$

Note that W_t is home region's nominal wage, and N_t^j is a household j's labor supply. Here, we assume perfect immobility across the regions, meaning wages will be determined at the regional level. B_{t+1}^j is a household j's state-contingent asset holdings and note again that we assume complete financial markets. Here P_t denotes price index that gives the minimum price of one unit of consumption good, C_t. *i.e.* P_t is the Consumer Price Index (CPI) in the home region.

Optimality conditions for $j \in (0, n]$ are

$$\chi\left(N_t^j\right)^{\psi} = \delta_t\left(C_t^j\right)^{-\sigma}\frac{W_t}{P_t},$$
$$\delta_t\left(C_t^j\right)^{-\sigma} = \beta\mathbb{E}_t\left[\delta_{t+1}\left(C_{t+1}^j\right)^{-\sigma}\frac{1+i_t}{1+\pi_{t+1}}\right],$$

where i_t is one-period nominal spot interest rate which satisfies $\mathbb{E}_t[M_{t,t+1}] = 1/(1+i_t)$.

Households in the foreign region are symmetric relative to those in the home region, and we use $*$ to denote foreign variables. So we have

$$C_t^{*j} = \left[(\phi_H^*)^{\frac{1}{\eta}} \left(C_{H,t}^{*j}\right)^{\frac{\eta-1}{\eta}} + (\phi_F^*)^{\frac{1}{\eta}} \left(C_{F,t}^{*j}\right)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}}, \quad \text{with} \quad \phi_H^* + \phi_F^* = 1.$$

For *aggregate* optimal allocations in the foreign region, we have

$$c_{h,t}^*(i) \equiv \int_n^1 c_{h,t}^{*j}(i)\mathrm{d}j = \left(\frac{p_{h,t}^*(i)}{P_{H,t}^*}\right)^{-\epsilon} C_{H,t}^*, \quad c_{f,t}^*(i) \equiv \int_n^1 c_{f,t}^{*j}(i)\mathrm{d}j = \left(\frac{p_{f,t}^*(i)}{P_{F,t}^*}\right)^{-\epsilon} C_{F,t}^*$$

$$C_{H,t}^* = \int_n^1 C_{H,t}^{*j}\mathrm{d}j = \phi_H^* \left(\frac{P_{H,t}^*}{P_t^*}\right)^{-\eta} C_t^*, \quad C_{F,t}^* = \int_n^1 C_{F,t}^{*j}\mathrm{d}j = \phi_F^* \left(\frac{P_{F,t}^*}{P_t^*}\right)^{-\eta} C_t^*,$$

$$C_t^* = \int_n^1 C_t^{*j}\mathrm{d}j = (1-n)C_t^{*j}.$$

Optimality conditions for foreign households for $j \in [n, 1)$ are

$$\chi \left(N_t^{s,j*}\right)^{\psi} = \delta_t^* \left(C_t^{j*}\right)^{-\sigma} \frac{W_t^*}{P_t^*},$$

$$\delta_t^* \left(C_t^{j*}\right)^{-\sigma} = \beta \mathbb{E}_t \left[\delta_{t+1}^* \left(C_{t+1}^{j*}\right)^{-\sigma} \frac{1+i_t}{1+\pi_{t+1}^*}\right].$$

Terms of Trade, and Real Exchange Rate

Before moving on to firms in each region, let us define terms showing the relationships between various price measures. First, we define terms of trade, S_t as

$$S_t \equiv \frac{P_{F,t}}{P_{H,t}}.$$

From this, we can write the relationship between CPI and Producer Price Index (PPI) in a home region as:

$$g(S_t) \equiv \frac{P_t}{P_{H,t}} = \left[\phi_H + \phi_F S_t^{1-\eta}\right]^{\frac{1}{1-\eta}}, \quad \frac{P_t}{P_{F,t}} = \frac{P_t}{P_{H,t}} \frac{P_{H,t}}{P_{F,t}} = \frac{g(S_t)}{S_t}.$$

For the case of the foreign region, we have

$$g^*(S_t) \equiv \frac{P_t^*}{P_{H,t}^*} = \left[\phi_H^* + \phi_F^* S_t^{1-\eta}\right]^{\frac{1}{1-\eta}}, \quad \frac{P_t^*}{P_{F,t}^*} = \frac{P_t^*}{P_{H,t}^*} \frac{P_{H,t}^*}{P_{F,t}^*} = \frac{g^*(S_t)}{S_t}.$$

Finally, we write the real exchange rate in terms of $g(S_t)$ and $g^*(S_t)$ as follows:

$$Q_t = \frac{P_t^*}{P_t} = \frac{g^*(S_t)}{g(S_t)}.$$

Firms

We assume that there is a continuum of intermediate-goods-producing firms in each region, producing differentiated intermediate goods by using labor as input. We assume a competitive labor market.

Production technologies of each intermediate-goods-producing firms are given by

$$y_{h,t}(i) = A_t N_{h,t}(i)^{\alpha}, \quad \alpha < 1,$$
$$y_{f,t}(i) = A_t^* N_{f,t}^*(i)^{\alpha}, \quad \alpha < 1,$$

where $y_{h,t}(i)$ ($y_{f,t}(i)$) is the production output of a firm i in the home (foreign) region, $N_{h,t}(i)$ ($N_{f,t}^*(i)$) is the amount of labor input hired by a firm i in the home (foreign) region, and A_t (A_t^*) is region-wide technology in the home (foreign) region. Both technology processes evolve according to the following laws of motion:

$$\log A_t = \rho^A \log A_{t-1} + \epsilon_t^A,$$
$$\log A_t^* = \rho^{A*} \log A_{t-1}^* + \epsilon_t^{A*}$$

This implies that region-wide labor demand can be written as

$$\begin{aligned}
N_t &= \int_0^n N_{h,t}(i)\mathrm{d}i = \int_0^n \left(\frac{y_{h,t}(i)}{A_t}\right)^{\frac{1}{\alpha}} \mathrm{d}i = \left(\frac{1}{A_t}\right)^{\frac{1}{\alpha}} \int_0^n y_{h,t}(i)^{\frac{1}{\alpha}}\mathrm{d}i \\
&= \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1}{\alpha}} \int_0^n \frac{1}{n}\left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\frac{\epsilon}{\alpha}} \mathrm{d}i = \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1}{\alpha}} \Delta_t^{\frac{1}{\alpha}}, \\
N_t^* &= \int_0^n N_{f,t}^*(i)\mathrm{d}i = \int_n^1 \left(\frac{y_{f,t}(i)}{A_t^*}\right)^{\frac{1}{\alpha}} \mathrm{d}i = \left(\frac{1}{A_t^*}\right)^{\frac{1}{\alpha}} \int_n^1 y_{f,t}(i)^{\frac{1}{\alpha}}\mathrm{d}i \\
&= \left(\frac{Y_{F,t}}{A_t^*}\right)^{\frac{1}{\alpha}} \int_n^1 \frac{1}{1-n}\left(\frac{p_{f,t}(i)}{P_{i,t}}\right)^{-\frac{\epsilon}{\alpha}} \mathrm{d}i = \left(\frac{Y_{F,t}}{A_t^*}\right)^{\frac{1}{\alpha}} (\Delta_t^*)^{\frac{1}{\alpha}},
\end{aligned}$$

by defining $\Delta_t \equiv \frac{1}{n}\int_0^n \left(\frac{p_{h,t}(i)}{P_t}\right)^{-\epsilon} \mathrm{d}i$, and $\Delta_t^* \equiv \frac{1}{1-n}\int_n^1 \left(\frac{p_{f,t}(i)}{P_t^*}\right)^{-\epsilon} \mathrm{d}i$ as price dispersion terms in each region.

Firms are subject to Calvo-type pricing frictions, so they solve the following problem:

$$\max_{p_{h,t}^{\#}(i)} \mathbb{E}_t \left[\sum_{k=0}^{\infty} Q_{t,t+k}\theta^k \left(p_{h,t}^{\#}(i) - MC_{h,t+k|t}(i)\right) y_{h,t+k|t}(i)\right]$$

subject to $y_{h,t+k|t}(i) = \left(\frac{p_{h,t}^{\#}(i)}{P_{H,t}}\right)^{-\epsilon} \left(C_{H,t} + C_{H,t}^*\right)$, and with $Q_{t,t+k} = \beta^k \frac{\delta_{t+k} u'(C_{t+k})}{\delta_t u'(C_t)}$. Note that here, $C_{H,t}^*$ denotes a composite index of foreign consumption of home goods, and $MC_{h,t+k|t}(i)$ is nominal marginal cost.

Then optimality conditions for pricing are given by

$$p_{h,t}^{\#}(i) = \frac{\epsilon}{\epsilon - 1} \frac{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k \delta_{t+k} u'(C_{t+k}) mc_{h,t+k|t}(i) P_{H,t+k}^{\epsilon} \left(C_{H,t} + C_{H,t}^*\right)}{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k \delta_{t+k} u'(C_{t+k}) P_{H,t+k}^{\epsilon-1} \left(C_{H,t} + C_{H,t}^*\right)},$$

with $mc_{h,t+k|t}(i)$ is real marginal cost of a firm i in terms of PPI, $P_{H,t}$.

Aggregate real marginal cost with $\alpha < 1$ can be written as follows:

$$\begin{aligned} mc_{h,t}(i) &= \frac{W_t/P_{H,t}}{\alpha A_t N_{h,t}(i)^{\alpha-1}} = \frac{w_t}{\alpha A_t} N_{h,t}(i)^{1-\alpha} \\ &= \frac{w_t}{\alpha A_t} \left(\frac{y_{h,t}(i)}{A_t}\right)^{\frac{1-\alpha}{\alpha}} = \frac{w_t}{\alpha A_t} \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1-\alpha}{\alpha}} \left(\frac{y_{h,t}(i)}{Y_{H,t}}\right)^{\frac{1-\alpha}{\alpha}} \\ &= mc_{H,t} \left(\frac{p_{h,t}(i)}{P_{H,t}}\right)^{-\frac{\epsilon(1-\alpha)}{\alpha}}, \\ mc_{H,t} &\equiv \frac{w_t}{\alpha A_t} \left(\frac{Y_{H,t}}{A_t}\right)^{\frac{1-\alpha}{\alpha}}. \end{aligned}$$

with $w_t \equiv W_t/P_{H,t}$.
Combining this with the previous optimal pricing equation then generates

$$p_{h,t}^{\#}(i)^{1+\frac{\epsilon(1-\alpha)}{\alpha}} = \frac{\epsilon}{\epsilon - 1} \frac{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k u'(C_{t+k}) mc_{H,t+k} P_{H,t+k}^{\epsilon/\alpha} Y_{H,t+k}}{\mathbb{E}_t \sum_{k=0}^{\infty} (\beta\theta)^k u'(C_{t+k}) P_{H,t+k}^{\epsilon-1} Y_{H,t+k}}.$$

We have similar conditions for intermediate-goods-producing firms in the foreign region.

International Risk Sharing Condition and Market Clearing Conditions

Combining each region's Euler equation gives

$$\delta_t \left(\frac{1}{n} C_t\right)^{-\sigma} = \kappa \delta_t^* \left(\frac{1}{1-n} C_t^*\right)^{-\sigma} \frac{1}{\mathcal{Q}_t},$$

with complete markets and symmety of initial conditions, $\kappa = 1$, generating

$$\delta_t^{-\frac{1}{\sigma}} C_t = \frac{n}{1-n} \delta_t^{*-\frac{1}{\sigma}} C_t^* \mathcal{Q}_t^{\frac{1}{\sigma}},$$

with $\mathcal{Q}_t \equiv P_t^*/P_t$ for the real exchange rate.

Goods market clearing conditions in each region are:

$$\begin{aligned} Y_{H,t} &= C_{H,t} + C_{H,t}^* = \phi_H \left(\frac{P_{H,t}}{P_t}\right)^{-\eta} C_t + \phi_H^* \left(\frac{P_{H,t}^*}{P_t^*}\right)^{-\eta} C_t^*, \\ Y_{F,t} &= C_{F,t} + C_{F,t}^* = \phi_F \left(\frac{P_{F,t}}{P_t}\right)^{-\eta} C_t + \phi_F^* \left(\frac{P_{F,t}^*}{P_t^*}\right)^{-\eta} C_t^*. \end{aligned}$$

Finally, we close the model by imposing the following monetary policy rule:

$$i_t = \rho_i i_{t-1} + (1 - \rho_i)(\phi_\pi \pi_t^{agg} + \phi_y \hat{y}_t^{agg}),$$

where π_t^{agg} is a union-wide inflation rate and $\hat{y}_t^{agg}$ is union-wide output gap.

Modelling Stay-at-Home Orders

We model the imposition of SAH orders in two ways: (i) as a local supply shock, and (ii) as a local demand shock. When we model the SAH as a local productivity shock, we introduce the negative productivity shock for intermediate-goods-producing firms by setting negative values for ϵ_t^A. Alternatively, we also model the imposition of SAH orders via a negative preference shock, since SAH orders may directly reduce consumption by limiting retail mobility, as discussed in Subsection C.1. In this case, we introduce negative shocks to ϵ_t^δ.

C.3 Data Appendix

Table C.1 reports all sources used in this paper.

Table C.1: Data Sources

Variable	Source
Initial Unemployment Claims (Accessed 6/17/2020)	FRED (Mnemonic *ICLAIMS, where * indicates state abbreviation)
County Employment Data (Accessed 6/4/2020)	BLS https://www.bls.gov/lau
Stay-at-Home Orders (Accessed with *Internet Archive*)	*New York Times* https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html
Covid Confirmed Cases (Accessed 6/5/2020)	UsaFacts https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/
State Excess Deaths (Accessed 6/4/2020)	CDC https://www.cdc.gov/nchs/nvss/vsrr/covid19/excess_deaths.htm
Share Age 60 (Accessed 6/16/2020)	Census Bureau https://www.census.gov/data/tables/time-series/demo/popest/2010s-state-detail.html
Average UI Replacement Rate (Accessed 6/16/2020)	Department of Labor's Employment and Training Administration https://oui.doleta.gov/unemploy/ui_replacement_rates.asp
2016 Trump Vote Share (Accessed 6/17/2020)	*New York Times* https://www.nytimes.com/elections/2016/results/president
Work at Home Index	Dingel and Neiman (2020)
March Employment Losses for Bartik (Accessed 4/10/2020)	BLS https://download.bls.gov/pub/time.series/ce/ce.industry
Google Mobility Reports (Accessed 5/21/2020)	https://www.google.com/covid19/mobility/
Daily Consumer Spending and Employment	Track the Recovery https://tracktherecovery.org
State Non-Essential Business Closure Dates	Kong and Prinz (2020)