U.S. Banking Integration and State-Level Exports*

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This version: February 28, 2011

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Abstract

Using inter-state banking deregulation in the U.S. as an exogenous experiment, we find that a 1% increase in banking integration between U.S. states caused a 0.164-0.184% increase in the exports/domestic shipments ratio for U.S. state level exports in the years 1992-1996. Given our empirical specification, this increase in openness can be attributed to an increase in capital to cover variable and fixed export costs relative to domestic shipping costs and a higher provision of trade finance services. Serving new destinations (the extensive margin defined at the state-country level) accounts for 22% to 28% of the banking integration effect that we observe.

JEL: F10, F15, G21, G28.

Keywords: exports, financial depth, inter-state banking deregulation

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* We would like to thank Dawn Kral from the U.S. ExIm Bank for CLS data, Joe Kafchinsky and Mary May from the Federal Trade Division for the state level exports data, and Thibaud Desfosses for research assistance. All errors remain our own.

1. Introduction

A significant amount of research examines the effects of finance on various aspects of real sector activity. One of the areas in which research has focused on recently is the availability of finance and exports. Many questions arise - can access to finance be a source of a country's comparative advantage? Are exports more or less sensitive to financing availability than domestic sales? etc. The issues are equally important from a policy point of view - for example, whether a lack of proper trade finance constitutes a significant barrier to exports or not.

We add to this growing literature on finance and trade by examining the effects of financial integration on U.S. state-level exports. Specifically, we examine the evolution of exports of the 48 contiguous states of the U.S.A. to different countries as more financing becomes available following the inter-state banking deregulation in the United States in the 1990s. We exploit the exogenous nature of the removal of regulatory barriers in banking to the states' exports in manufactured goods to foreign destinations. Our identifying strategy relies on the fact that different states deregulated entry into their banking markets at different dates and to different sets of counterparty-states before the federal law made it mandatory for all the states in 1995. This interstate bank liberalization caused a consolidation of the industry and a wave of mergers in the previously closed state banking systems (see Berger et al., 1995 for an early account).

The increase in the availability of finance is also likely to trigger (endogenous) re-allocation of factors of production, firm entry and exit, and increases in productivity, or may simply be contemporaneous with (exogenous) changes in manufacturing technologies. Such effects are difficult to account for appropriately in empirical testing[1]. To get around these complications, we examine individual states' exports to different foreign countries *relative* to their shipments of manufactured goods to the rest of the U.S., which we label as "domestic" trade[2]. In other words, we study the effects of banking integration on openness. The use of the relative foreign-to-domestic trade ratio, a measure of *openness*, allows us to account for state-specific dynamics, albeit implicitly, that would otherwise make it difficult to identify the impact of a state's financial integration with the rest of the U.S. on its exports. It helps also our IV-based strategy: it is very unlikely that deregulation patterns of U.S. states could be explained by state openness patterns. Our empirical specification allows us to identify how bank integration had an impact on the relative costs of serving different (foreign and domestic) destinations (see Section 4), either through the provision of finance to overcome variable and fixed costs of exports, or through the

[1] If there was a positive effect of banking integration on domestic production, for example through new firm entry, standard trade theories predict that exports would increase as well. This would have nothing to do with providing more trade finance.

[2] Rerunning our base specifications on export data only shows also a statistically significant and positive link between bank integration and exports, although the estimating specification may be error-prone as discussed below.

availability of trade finance services (that may depend for example on easy access to international banking networks). However, it is difficult to discern between these two channels empirically. Our data allow us to distinguish the intensive margin, i.e., trade between existing state-country pairs, from the extensive margin, i.e., trade between new state-country pairs.

Why should tighter bank integration increase exports by more than domestic shipments? Several channels can be identified, discussed further in Section 3. Bank integration can change capital availability to firms or increase the supply of export-related services at the export location. More capital availability should allow firms to finance any variable or fixed costs of exports more easily on top of just financing production or innovation costs, and hence permits firms to serve more destinations (the extensive margin) and/or export more to old destinations (the intensive margin). Higher availability of trade finance products for potential exporters and better access to international banking networks would further reinforce the effect.

In the empirical work, we first estimate log-linear gravity equations of trade using an IV-*within*-estimator with origin U.S.-state and destination-country fixed-effects. Because our dataset involves zero trade flows between many U.S. state and country pairs, which may lead to potentially biased and inconsistent estimates (see Santos Silva and Tenreyro, 2006 for a discussion precisely in the context of trade flow estimation), we also use count-data based IV-Poisson estimators with fixed-effects. Our specification controls for many factors that are difficult to measure (such as GDP and price levels) or that are difficult to account for (such as import tariffs and nontariff barriers).

We find from our preferred estimates suggest that exports reacted more strongly than domestic shipments to banking integration: a 1% increase in banking integration leads to a 0.164-0.184% increase in the exports/domestic shipments ratio in our sample. This means that, if an integration of a state would increase from 23% -- the average for the 48 states in the sample in 1991 -- to 31.1% in 1995 (a 35% increase in integration), openness should increase by 5.77-6.48% in the same time due to a higher provision of trade services and capital for covering variable and fixed costs. Given the interpretation of our dependent variable and the estimation specification we use, our findings are consistent with Matsuyama's (2007) prediction that an exogenous increase in factors that are extensively used in exports (in our case, the provision of export-specific services) would increase openness. The extensive margin of trade (defined in our case as state-level trade to new countries) accounts for at least 22% to 28% of the banking integration effect that we observe. This means that the increase in banking integration allowed to start new trade relationships between some states and countries.

This article is related with the growing literature on finance and trade. An important and distinctive trait of the paper is that we can sidestep the endogeneity problems that are inherent in this line of research irrespective of whether aggregate (e.g., Manova, 2008b) or firm-level data are used (e.g., Greenaway et al., 2007). For papers based on country-level data, countries' financial depths may not be exogenous to their global trade flows as pointed out by Do and Levchenko (2007): the export financing needs of firms can cause a deeper financial system to develop. In contrast, in our case the change in the states' domestic financial integration with the rest of the U.S. is exogenous to their exports to foreign countries. For papers based on firm-level data the endogeneity problem is entangled with productivity: it may be that more productive firms export more, which allows them to grow faster and access financial capital more easily; or that productive firms are more likely to get financing, which in turns makes it more likely that they export. Moreover, individual firms' export decisions require the modeling of the endogenous self-selection process, something which is difficult because it depends on many firm characteristics that cannot be readily observed. Our research differs from the existing papers in both areas as we examine whether a state's exports to other countries increase *relative* to its domestic shipments (domestic trade) when an exogenous event makes bank financing more available for that state as opposed to other states. In other words, we examine a state's export of manufactured goods to foreign destinations over and above its domestic trade with the rest of the U.S., once that same state's banking sector opens up to banks from other states. We view our contribution as complementary to that of Amiti and Weinstein (2009) and Minetti and Zhu (2011), the only papers that attempt to identify the effect of bank intermediation on firm-level exports for Japanese and Italian data, respectively.

Post-"Great Recession", a new line of research started to examine whether the shocks to the financial sector are responsible for the large drops in the exports/GDP ratios, something that may have exacerbated the slowdown of economic activity during the crisis (for example, Amiti and Weinstein 2009, and Kortum et al., 2010). Our paper is also related to this literature, which focuses on the provision of bank capital, since we examine how exports are affected from positive shocks emanating from the integration of a state's banking system with the rest of the U.S. The staggered deregulatory processes that we use to identify the effect of financial integration on trade may provide an alternative to the testing of the same question. Our results show that the stronger responses of international trade to domestic shipments may be due to both changes in the provision of capital or changes in banking integration.

Finally, the paper contributes to the sizeable literature of the effects of the U.S. banking deregulation on real activity (see, for example, Jayaratne and Strahan, 1996; Black and Strahan, 2002; and Kerr and Nanda 2009). The finding that bank integration affected exports more than domestic shipments is of interest in itself, and we indirectly identify another benefit of the U.S. deregulation experiment – the attenuation of trade finance related barriers.

The structure of the U.S. banking industry at the beginning of the 1990s and the profound transformation it was undergoing at the time leads us to believe that banking deregulation in the U.S. is a compelling natural experiment that can be used to study the increase in the supply of finance for both production and export purposes. At the beginning of 1990s, there were few U.S. banks (15-20) that were engaged strongly in international trade finance out of the fragmented industry of 12000 institutions (American Banker, 1992)[3]. Many of the banks requested additionally government guarantees to provide trade finance products.

The way trade finance was provided in the U.S. was rapidly changing at the beginning of the 1990s, shaped by two factors -- the ongoing banking deregulation and technological changes in the provision of trade finance products. Interstate banking deregulation that occurred in the U.S. between 1978 and 1995 allowed the creation of regional and "superregional" banks -- networks of financial institutions that were controlled by multibank holding companies (MBHCs). These banks, because of their size could offer more complex services and hence a greater product mix - including trade finance - that banks with only a local presence could not, placing the latter at a disadvantage (American Banker 1996, World Trade 1995). This was because small organizations either did not have the required capital base, the expertise (with some trade finance arrangements requiring extensive legal know-how) or the network of correspondent banks and/or overseas branches (see Austin American-Statesman 1996, American Banker 1994, 1997, Chicago Sun-Times 1994). Therefore, for local businesses, the arrival of interstate organizations may have made trade finance more affordable and available, especially for small exporters. Because regional banks were located "close to the customer" they had an advantage over money center banks (located in New York, San Francisco, etc.), which were traditional purveyors of trade finance, in terms of speed and quality of service (World Trade 1995). Non-money center "regionals" and "superregionals" were also more readily serving midsize companies as they wanted to become one-stop shops for their clients, whereas the large money center banks were typically interested in large transactions.

Technological and product advances -- both in the U.S. and globally -- also changed the provision of trade finance. Computerization of operations allowed to lower the costs of origination, preparation of the legal documents needed for different products, the conduct of transactions and the tracking of the payments' status by customers. Firms could originate trade finance products from a distance from their own PC-based systems. As such procedures became more standardized the provision of trade finance products started to have economies of scale. On the product innovation side, securitization allowed banks to avoid the country risk limits that banks could take (American Banker, 1996). These changes favored large organizations -- which were being created as a result of interstate mergers -- to obtain more of the trade finance business. To cater to their clients, for example as a result of NAFTA, some banks went to

[3] This is confirmed by data obtained from the Ex-Im Bank of the United States on banks that sought Ex-Im bank financing. The distribution of banks that engaged actively in contracts with the ExIm Bank is very skewed with at most 30 banks in the years for which data is available engaging in contracts of over $100 million.

acquire foreign operations, for example in Mexico (U.S. Banker, 1995). However, many large banks, instead of creating their own networks, entered into alliances with foreign partners to provide letters of credit and other trade finance products (American Banker, 1996, 1997, U.S. Banker 1993).

The paper is constructed as follows. In Section 2 we review the current literature on trade and finance. In Section 3 we sketch the effect of banking integration on the ability of firms to export. In Section 4 we present the empirical specification and discuss the data at our disposal. Section 5 discusses the results and Section 6 concludes.

2. Literature Review

This paper is related to the growing body of research on trade intermediation in general – the role of networks, wholesalers, specialized traders in international trade (see for example, Rauch, 2001, Feenstra and Hanson, 2004, Ahn et al., 2009, and Blum et al., 2009, Antras and Costinot, 2010). In contrast to these papers we focus on the role financial intermediation in trade, which is a nascent area of research. Most of the papers in this new research area make use of firm-level data to examine the role of bank financing on exports. For example, Amiti and Weinstein (2009) identify main-banks providing trade finance services to major Japanese companies (for each company it is either the bank that handles most firm transactions or the most important commercial bank if the former is a regional bank) and relate main-bank financial health to client-firm export activity. Their story is inherently about capital provision: they find a positive impact of bank health on exports in Japan. While very interesting and clever in its approach, Amiti and Weinstein (2009) also leave certain questions unanswered. For example, these authors cannot observe the destinations served, and as a result can only evaluate the effects of main-bank health on the intensive margin, but not on the extensive margin. Moreover, adverse bank health (as measured by stock valuations in their study) may be caused by expected bad performance of their borrowers in the first place. Amiti and Weinstein (2009) focus on the largest Japanese firms may be underestimating the total effects of bank health on exports, even if their sample accounts for approximately 80% of Japanese exports, as it is the small- and medium-sized firms that rely on banks the most for their financing needs. In another interesting paper, Minetti and Zhu (2011) study a survey of Italian firms that detailed their export participation and potential credit constraints in year 2000. They use as instruments historical Italian banking regulations that limited the provision of banking services on a regional basis and find a statistically significant effect of credit constraints on firm-level exports. Given their data, Minetti and Zhu (2011) can study the intensive as well as the extensive margin at the firm level, but cannot control for many unobservables in a one-year cross-section. Endogeneity is an important problem in this literature, and despite best efforts Minetti and Zhu (2011) cannot account for certain of its dimensions, such as firms' decision to locate in different regions depending on the banking services on offer.

Our work differs from both of these papers in a number of dimensions. First, we use state-country level exports data as opposed to the firm-level exports data in Amiti and Weinstein (2009) and Minetti and Zhu (2011). One advantage of state-country level data is that we do not have to model firms' export decisions, which is not an easy task given the many unobservables involved in the process. Second, we do not have to model firm-level differences in production technologies or productivity. Third, we are able to assess the macroeconomic impact of financial integration on exports, as opposed to estimating the aggregate impact from firm-level analysis. Moreover, our data include all exports and domestic shipments from individual U.S. states, as opposed to the largest Japanese exporters in Amiti and Weinstein (2009), or the majority of the Italian exporting firms in Minetti and Zhu (2011). Fourth, we are able to work with panel data, albeit short ones using two different data series (4 or 5 years), which allow us to study the dynamic effects of banking integration on exports (as opposed to, for example, Minetti and Zhu (2011) that relies on cross-section data linked to a survey). As such, we do not expect firms to change their location in response to changes in financial integration over such a short panel (which may be an issue in Minetti and Zhu, 2011). Fifth, our story is about the deepening of the financial sector as well as the increase in the provision of trade-related financial services by virtue of bank integration. This broader perspective differs from Amiti and Weinstein (2009), for example, who focus on the effects of capital provision by Japanese main-banks. In our case, a higher integration of the banking system may lead to more intermediation – as more sophisticated services (including those involving international trade) are provided in what were more isolated local banking markets. This, in turn, will lead to higher trade-to-GDP (globalization) as predicted by Matsuyama (2007) because international trade costs are lowered (though Matsuyama 2007 model is more general in its scope).

Importantly, we are able to identify effects of inter-state banking deregulation on exports *via* the provision of trade-specific services banks can provide, which is different than a simple provision-of-capital story. If bank-entry deregulation leads to increase in loanable funds in a state and/or to lower interest rates on loans (as findings of Rice and Strahan, 2010, on inter-state branching would suggest), then our dependent variable, which is the ratio of foreign exports to domestic shipments, would not be necessarily affected: to the extent the manufacturing costs are the same irrespective of the shipment destination, availability of more and/or cheaper capital should not affect the production of goods for the export market any more than their production for the domestic market. If integration allows the provision more trade-related financial services after inter-state bank-entry deregulation, then we could expect exports to increase more than domestic shipments. The staggered inter-state banking deregulation that took place at different points in time for different states, provides us with an opportunity to identify the effects of the provision of more trade-related financial services on exports, over and above a simple story of provision of more capital. We believe that the banks' important role in providing trade-related services and these services impact on trade is an important research question. Given these observations, we view our contribution as complementary to that of Amiti and Weinstein (2009) and Minetti and Zhu (2011).

In some respects our work is related to Michalski and Ors (2010), who treat the U.S. inter-state banking deregulation as a natural experiment as we do, and find that higher banking integration between U.S. states led to higher trade between them. In their case, state pairs that experienced an increase in banking integration from zero to the sample mean had their common trade increased by 11%-25%, a result that could provide a lower bound estimate for the trade effects of *international* bank integration. The focus on international exports in this paper differs from Michalski and Ors (2010) analysis of within-U.S. trade across states.

Our work is also related to a growing literature on financial sector development (depth) and international trade. For example, using a panel of 107 countries and 27 industries between 1985 and 1995, Manova (2008b) finds that countries with deeper financial markets export more in capital dependent industries as well as in those that have few collateralizable assets. Her findings indicate that credit constraints affect both fixed and variable export costs. Among other papers in this new strand of literature, Beck (2002) finds in a 30-year panel of 65 countries that those with more developed financial systems have a higher export share and trade balance in manufactured goods. Svaleryd and Vlachos (2005) find that among OECD countries (i) differences in financial development impact industrial specialization patterns; and (ii) a well-developed financial system is a source of comparative advantage. Becker and Greenberg (2003) find that financial development helps the exports more in industrial sectors with large up-front investments. However, the causality between the availability of finance and trade remains difficult to resolve. As Do and Levchenko (2007) show, using geography as an instrument for trade flows in a cross-country study, the relationship between financial depth and trade may go the other way: a comparative advantage for a country in financially dependent industries appears to spur the development of its financial sector.

Our work differs from the last set of papers as we focus on the effects of the financial integration between regions, as opposed to financial system depth in a given country, on trade flows. The two need not be equal. First, financial depth, which is typically measured as the ratio of banking assets to GDP, may not fully reflect the provision of loanable funds, which can be transferred across different units of a multi-state or -country banking organization. Second, financial depth may not be the best measure to account for the provision of trade-related financial services, which may depend more on the presence of large out-of-state or -country banks and trade-specialist banking organization. We would argue that measures of financial integration, such as the share of out-of-state banks, may be more useful when trying to differentiate the effect of higher capital provision from that of more trade-related services provision. From this perspective, the inter-state banking entry deregulation in the U.S. offers a compelling natural experiment to study the impact of financial integration on exports. Using changes in financial integration in this setting should go a long way to solve the problem of endogeneity posed by Do and Levchenko (2007). Moreover, U.S. states share a common legal background, and the banks operate in a common federal structure of supervision and regulation. U.S. exporters from different states face the same political and economic risks and have access to the same export support programs through the Export-Import Bank of the United States (U.S. ExIm Bank) or private lenders such as Private Export Funding Corporation (PEFCO). Therefore, the possibility that country-specific factors (either at the origin or at the destination) will be driving the results is lower in our case than in typical cross-country investigations.

Our work is also linked to the growing research on financial or liquidity constraints and exports. Chaney (2005) builds a theory model in which liquidity constraints affect's firms' ability to export. Greenaway, Guariglia and Kneller (2007) show evidence that exporting firms are financially healthier than non-exporting firms. They also find that firms that start to export have lower liquidity and higher leverage, suggesting that these firms are more likely to need bank financing. Zia (2008) studies the withdrawal of export subsidies to Pakistani firms, and finds that exports of financially constrained firms decrease, whereas those of non-constrained firms do not. Similarly, Ronci (2004) finds that a fall in trade financing that corresponds to a domestic banking crisis leads to significantly lower exports. Suwantaradon (2008) builds a theory model in which among equally productive firms credit-constrained ones never accumulate enough liquidity to be able to export, and finds support for her theory in survey data from Brazil and Chile. Muuls (2008) uses Belgian firm-level data and Coface scoring of firm creditworthiness to control for financial constraints as she assesses the characteristics of exporting firms. She finds that financially weak firms are less likely to become exporters, and when exporting they reach fewer destinations with fewer products. Manova et. al. (2009) study the export performance of firms in China showing that foreign affiliates and joint ventures perform better than private domestic firms, especially in sectors with higher financial vulnerability. They ascribe their evidence as being consistent with the existence of credit constraints and advantages of multinational companies in international trade. Coulibaly et al. (2011) use balance sheet firm-level data to investigate the usage of trade credit (i.e., how much was borrowed from suppliers or lent to buyers) during the credit crunch in Asia; they find *inter alia* that export intensive firms did not use trade credit and their sales declined more strongly than for non-exporting firms.

Our state-country level data combined with the inter-state banking deregulation as a natural experiment may help overcome the inherent endogeneity problems that is inherent in all papers based on firm level data. For example, it may be that more productive firms are more likely to export and at the same time such firms may also have better access to capital. It may also be that banks select to lend to more productive firms, which helps the latter to export more easily. Moreover, individual firms' export decisions require additional modeling of the self-selection process to export goods, something that depends on many unobservables and for which there are few good exogenous (instrumental) variables. We are also able to account for the extensive margin of trade that some of the aforementioned papers cannot.

Given our identification strategy (see Section 4.2.2), our paper is also related to the recent contributions trying to explain the fall in trade to GDP ratio during the 2008-2009 crisis (Chor and Manova, 2009, Amiti and Weinstein, 2009, Levchenko et al., 2010, Kortum et al., 2010). Chor and Manova (2009) document that the fall in U.S. *imports* during the crisis was larger for countries and sectors with adverse credit conditions. According to Amiti and Weinstein (2009) the effect of negative bank health in Japan in the 1990s would explain 1/3 of the drop in the trade to GDP for that country in that period, and presumably be responsible in as much a fall in the trade/GDP ratio during the recent crisis as well. In contrast, Kortum et al. (2011) use general equilibrium model calibration and find that the decline in the global durable goods consumption (towards which international trade is geared) would be the main reason explaining the steep decline in trade and that finance played a minor role in the current crisis (except for some effect in Japan and China).

Finally, our paper contributes to the large literature of the real effects of U.S. banking deregulation. For example, Cetorelli and Strahan (2006) find that changes in banking competition brought by deregulation of the depository institutions sector shifted the distribution of firms towards smaller establishments. Their findings would suggest that if younger firms are less export oriented, an increase in financial depth following banking deregulation, may actually *decrease* foreign exports relative to domestic shipments. On the other hand, there is some evidence that U.S. banking deregulation reduces lending rates (e.g., Rice and Strahan, 2010). If bank deregulation is followed by decreases in the cost of trade related services, we may observe an increase in the foreign exports to domestic shipments ratio. To our knowledge no paper so far studied the effects of U.S. banking deregulation on U.S. export activity.

3. Links to Standard Theories of Trade

Various models of international trade -- those based on increasing returns with homogenous (Krugman 1980) or heterogenous firms (Melitz 2003) but also those based on Ricardian technology differences (Eaton and Kortum 2002) -- yield a similar gravity specification of expected trade flows $E\{X_{ij}\}$ from region (state) *i* to a destination (country) *j* (see a summary in Kortum et al. 2010):

$$E\{X_{ij}\} = E\left[N_i \left(\frac{I_j}{P_j}\right)^{\eta} \left(\psi_{ij}\right)^{-\phi} \Gamma_{ij}\right] \tag{1}$$

where, N_i is the number of firms in the sector at location *i*, I_j is the income at destination *j*, P_j is the price level, ψ_{ij} is the (constant) marginal cost of serving destination *j* from location *i*, Γ_{ij} is a collection of parameters that contain, *inter alia*, variable and fixed costs of exporting to the destination *j*. The assumption on the other parameters is that $\eta>0$ and $\varphi>0$.

In this general set-up, banking integration can affect exports from a region through several channels. The working of these channels can be summarized by whether bank integration changes capital availability to firms or increases the supply of export-related services at the export location (in our case, the state). A good discussion is provided in Amiti and Weinstein (2009).

The effect of bank integration on exports through changes in capital availability is similar to that identified by the finance and growth literature on domestic output and will not be extensively reviewed here. For example, higher banking competition as a result of new bank entry can lead to lower financial barriers to firm entry and affect the number of firms entering the sector N_i (see evidence in Cetorelli and Strahan, 2006), the costs of lending to firms (see a discussion in Rice and Strahan, 2010) - so directly the marginal cost of production ψ_{ij}. Arguments can be advanced as to why loosening of credit constraints or cheaper cost of borrowing by firms could affect exports more strongly than production for the domestic market. Export transactions require typically more working capital due to the fact that there are higher up-front costs and that payment settlement on international contracts typically is much longer than on domestic ones (see discussions in Amiti and Weinstein, 2009, and Manova et al., 2009). Moreover, exporting is a more risky activity than shipping domestically. For export activity, a higher availability of capital and lower credit constraints can also affect the variable and fixed exports costs captured succinctly in expression (1) by Γ_{ij} (see Manova 2008b, Manova et al. 2009 for further discussion).

The other important channel through which banking integration may affect exports is the provision of export-supporting financial services. An exogenous increase in such services would lead, for example as discussed by Matsuyama (2007) in his theoretical model, to an increase in exports not only in global terms, but also relative to domestic output. First, banks may serve as processors of information on economic conditions in different regions (states or countries), as they may serve many transactions from many sectors; therefore they may have additional private information that a single exporting firm may not possess (for example, about the various dimensions of local demand in the target export market). It may have also more precise information on the probability of counterparty survival (the contractual risk) that is crucial in contracting. Second, banks may play a crucial role in safeguarding and enforcing payments in international contracts. A firm that has a one-time export contract may find payment enforcement problematic in the absence of banks. This is because the costs of payment enforcement - for example fixed costs of a legal action in a foreign country - may be too large for the contract to be profitable if the importer reneges on payments. A potential solution to the problem is the usage of the banking systems in the exporter and importer country to process payments, and there is a wide variety of different types of contracts serving this purpose in practice (see for example Grath, 2010 for a description). Larger banks, created in the process of banking integration, may enter in relationships with correspondent banks in other countries more easily and find it therefore easier to enforce payments or go after recalcitrant payers in other countries. There may be economies of scale and scope in these activities. The arrival of these large banks in under-banked areas may lower the cost of accessing the export-supporting activities for a potential exporter, thus directly affecting Γ_{ij}. Given the existence of these channels and their effects, we expect that banking integration in the U.S. would have a larger impact on exports relative to domestic shipments.

4. The Empirical Model and Data

4.1. The Empirical Specifications

For our analysis we need to match the above-mentioned theoretical considerations with the available state-to-country level trade data. We have aggregate export data from individual U.S. states (denoted *i*) to foreign country *j* ($FEXPORTS_{i,j}$), but unfortunately we cannot observe the firm-level production or export data. Theoretically this is not a problem as modern trade models relate the expected exports to destination-specific demand and trade barriers (including variable and fixed costs of trade). Moreover, the domestic market can be treated as the destination that has the lowest (possibly zero) trade barriers. Hence, by applying a proper empirical strategy, we can overcome the difficulty that we do not observe the exact number of exporters to a given destination. In fact, as firms may be created for a variety of reasons, we eliminate the number of firms from consideration altogether. This will allow us to use only the total state production (in the aggregate or in groups of sectors) and the exports to foreign destinations. To counter any potential effect of firm creation we use the ratio between exports and domestic U.S. shipments (calculated as the difference between total shipments and total exports). This approach allows us also, as described below, to counter any differential cross-state changes in production factor costs that could potentially occur because of banking integration.

The ratio of the exports from state *i* to foreign country *j* to the shipments to destination *k* (in our case the domestic U.S. market) at time *t* given equation (1) is:

$$\frac{E\left\{X_{ijt}\right\}}{E\left\{X_{ikt}\right\}} = \frac{N_{it}\left(\frac{I_{jt}}{P_{jt}}\right)^{\eta}\left(\psi_{ijt}\right)^{-\phi}\Gamma_{ijt}}{N_{it}\left(\frac{I_{kt}}{P_{kt}}\right)^{\eta}\left(\psi_{ikt}\right)^{-\phi}\Gamma_{ikt}} \tag{2}$$

Taking logarithms, and simplifying (2) we obtain (notice that the number of firms N_{it} cancels out[4]):

$$\ln\left(\frac{E\{X_{ijt}\}}{E\{X_{ikt}\}}\right) = \eta \ln I_{jt} - \eta \ln P_{jt} - \phi \ln T_{ijt} - \eta \ln I_{kt} + \eta \ln P_{kt} + \phi \ln T_{ikt} + \varepsilon_t \tag{3}$$

where, $T_{ijt} = \psi_{ijt}(\Gamma_{ijt})^{-1/\varphi}$ and $T_{ikt} = \psi_{ikt}(\Gamma_{ikt})^{-1/\varphi}$. We stipulate that at time t, T_{ijt} can be modeled as:

$$T_{ijt} = \psi_{it} \times d_{ij} \times d_t \times d_{jt} \times d_i \times d_{it} \times \Phi \tag{4}$$

where, ψ_{it} is the marginal cost of production, d_{ij} is the (time-invariant) trade barrier between *i* and *j* (e.g., distance), d_t is the yearly barrier (e.g. general transport costs in a given year), d_{jt} is the time-varying general destination trade barrier (e.g., a general level of tariffs at the destination), d_i is any barrier that may be specific to the exporting U.S. state (e.g. access to a deepwater port) and Φ are invariable parameters.

While looking at the behavior of the ratios, notice that a cost factor like wages or interest rates (which are a part of ψ_{it}) is going to cancel-out in this expression since it is the same for both exports and domestic shipments to *k*. Therefore, any changes in factor prices in states will not have any effect on this ratio (as long as the marginal costs of producing the same product for the domestic and foreign markets are the same). An important issue is that any time-varying changes within each state that would affect both the exports and domestic shipments are not going to matter as d_{it} in T_{ijt} and T_{ikt} will cancel out. The differential cost between shipping to the two different destinations is captured by $ln(d_{ij}/d_{ik})$ and $ln(d_{jt}/d_{kt})$, i.e., the changes in the *relative* trade barriers (e.g., the higher provision of trade-related financial services).

The resulting empirical specification in the log-linear form is (where destination *k* is the rest of the U.S., i.e., the "domestic" market):

$$\ln\left(\frac{E\{X_{ijt}\}}{E\{X_{ikt}\}}\right) = \begin{bmatrix} \alpha + \beta \ln Z_{it} + \\ +\gamma_i + \gamma_t + \gamma_{jt} + \gamma_{ij} + \gamma_{ik} \\ +\varepsilon_t \end{bmatrix} \tag{5}$$

where, γ_i is the state dummy, γ_t is the year dummy, γ_{jt} is a destination-time dummy, γ_{ij} and γ_{ik} are the state-destination dummies and ε is the error term. Given that the base in terms of the destination (*k*) – the home

[4] In modern trade theories, firm heterogeneity is captured by the characteristics of productivity distributions of the firms that exist in a market separately from their number N_{it}. Any firm heterogeneity would be captured by the term *Γ,* and given (4) would be captured by d_i.

market, the entire U.S. - always remains constant,[5] there is no need for including base destination year dummies γ_{kt}, which would be equivalent to γ_t. The time-invariant dummies γ_i, γ_{ij} and γ_{ik} are all going to be absorbed by fixed effects inherent in our estimators. The year-effects, γ_t, filter out any general (across-the-board increases in U.S. exports) and base-destination related (i.e., domestic U.S.) income, price level and general trade barriers. Destination income, price levels, free trade agreements (FTAs) with the U.S., general tariff level, political risk and any trade barriers such as wars are captured by the destination-year dummy γ_{jt}. It should be noted that the state-country pair fixed-effect γ_{ij} would account for the presence of country *j*'s banks in state *i* throughout the sample period, whereas the time-varying destination-country effect γ_{jt} would control for entry by country *j*'s banks into the U.S. banking market.

The variable of interest Z_{it} is a measure of banking integration with the rest of the U.S. for a given state *i* across time *t*. It is clear that β may be positive if, for a given state the change (fall) in export costs is higher than that of serving domestic destinations. On the other hand, if banking integration leads to more funding at a lower cost for domestic flows, β may well be negative. Given our empirical specification, β would capture the effect of higher capital provision as well as more trade-related services by banks.

4.2. Data

4.2.1. Trade and production data

To estimate the impact of financial integration on exports, we combine information from various sources to build our database. We obtain state-level U.S. export data from the Federal Trade Division (FTD) of the Census Bureau of the U.S. The data come from the Shipper's Export Declaration (SED) forms that all exporters have to file. The data were collected starting with 1992 by state and by industry sector at the two-digit SIC code level. The SED forms contain three location-related fields in which the exporter is asked to provide (i) U.S. state of origin of the shipment, (ii) the ZIP-code of the location of the exporter, and (iii) the foreign country to which exports are shipped. FTD obtains two different datasets depending on whether the data aggregation at the state level is done using the exporter provided (i) US-state or (ii) ZIP-code information, which results in the Origin of Movement (OM) data and the Exporter Location (EL) data, respectively. There are two main reasons why the US-state information for the originator of the exports need not match the US-state information contained in the ZIP-code for the export location. First, multi-plant firms could report their headquarters' address in the ZIP-code field in the SED form and fill-in the U.S.-state code of the plant that manufactured the products that are being exported. Second, the exporters may be wholesalers rather than producers, in which case they could provide the

[5] The data on domestic shipments detailed by the origin-state or destination-state are not available on a year-to-year basis.

ZIP-code information for their firm and the U.S. state-code information for the actual manufacturer.[6] According to surveys conducted by the FTD, the data provided by producers tend to reflect plant or establishment location rather than firm headquarters. In any case, it is unclear which type of data would be more accurate in matching exports with their "actual" or "primary" origin. As a result, we use both datasets.[7]

We apply a number of exclusion rules to obtain our final databases. First, we use only the trade data for manufacturing (SIC codes 20 to 39), and exclude agricultural and mining sectors. Second, we exclude shipments with unknown destination countries. Third, we also drop export flows reported as "manufactured goods not identified by kind". Fourth, we exclude Alaska, Hawaii and the District of Columbia, which have small and undiversified manufacturing sectors when compared with the 48 contiguous states of the Union. Moreover, Hawaii did not allow for any inter-state bank deregulation prior to the enactment of IBBEA in 1995. Fifth, we keep only data pertaining to sovereign destinations, i.e., foreign countries. Sixth, we exclude all countries that, prior to 1989 or within the sample period, (i) were communist, (ii) ceased to exist or (iii) were in the process of disintegration (U.S.S.R., Yugoslavia, Czechoslovakia, Ethiopia) or integration (Yemen[8]) during the span of our sample. The reason for this last series of exclusions is that, until mid-1990s, the so-called U.S. government COCOM restrictions on trading high-technology products with communist economies were in place. Even though these restrictions began to be lifted successively starting with 1993, we nevertheless exclude ex-communist countries because their trade patterns could be biased in unexpected ways. Seventh, we also exclude countries that had U.S. trade embargoes in place during the period (for example, Iraq or Iran).

Our sample starts in 1992 for the OM data and 1993 for the EL data, the respective years in which FTD started to collect industry-segment exports data per state-country-SIC-code from the SEDs to create the two datasets that we use. U.S. state-level exports data are available back to 1987, however they are aggregated and inclusive of not only of agriculture and mining sectors but also of government transactions (such as arms sales), which preclude their use for our purposes for the reasons cited above. Our datasets end in 1996 for two reasons. First, IBBEA (the so-called Riegle-Neal Act) of 1994, which mandated U.S.-wide inter-state bank entry deregulation, came into effect in 1995. As a result, we choose not to extend the datasets beyond 1997 since we no longer have a series of exogenous events that would help us identify changes in banking integration. Second, eventually we would like to exploit sector-level variation in the data. However, after 1996 the U.S. shipment data (to be detailed below) were no longer

[6] This should be less of a problem for manufactured products, which are differentiated and often exported by producers themselves, than for agricultural or mining products that we exclude from our datasets.

[7] A detailed study of this data and state export patterns, albeit for a later period and according to the NAICS classification that was introduced in 1997 is provided by Cassey (2009) and Cassey (forthcoming).

[8] Yemen united in 1990, but in the FTD data that spans later years still North and South Yemen are listed separately.

collected under the SIC industry classification, but under the NAICS scheme. Unfortunately, the two industry classifications cannot be reconciled at the equivalent of the 2-digit SIC-code level, which rules out a longer panel at the state-country-SIC-code level beyond 1996.

The final sample consists of 48 origin states and 148 destination countries for which foreign exports in U.S. dollars, *FEXPORTS*, are available. As a result, our full sample includes 7,104 (=48×148) state-country pairs on an annual basis, which results in 35,520 (=48×148×5) observations for exports in the OM data (*FEXPORTS_OM*) over 1992-1996, and 28,416 (=48×148×4) observations for exports the EL data (*FEXPORTS_EL*) over 1993-1996, including the state-country pair observations for which exports were zero in one or more years. The correlation between *FEXPORTS_OM* and *FEXPORTS_EL* between 1993 and 1996 is 0.957.

Total state manufacturing shipments (*TSHIPMENTS*), which are inclusive of exports, for SIC-codes 20 through 39, are taken from the Annual Survey of Manufactures of the U.S. Census. To obtain *domestic* shipments (*DSHIPMENTS*), i.e., all U.S. shipments from a given state net of exports but including own-state shipments, we subtract the state-level aggregate exports (*FEXPORTS_OM* or *FEXPORTS_EL*, depending on the database) from *TSHIPMENTS*.[9] Our dependent variables are the ratio of a state's exports in manufacturing (*FEXPORTS_OM* or *FEXPORTS_EL*) to that state's domestic shipment of manufactured goods to the rest of the U.S. (*DSHIPMENTS*), which we label as *FEXPORTS/DSHIPMENTS_OM* and *FEXPORTS/DSHIPMENTS_EL*.

4.2.2. Inter-state banking deregulation and bank-entry data

We use standard and publicly available U.S. data sources to create our banking integration (our endogenous and instrumented test variable) and inter-state banking deregulation variables (our exogenous instruments). To calculate a measure of banking systems' integration of a state with the rest of the U.S. banking system, we use data from the Summary of Deposits (SoD) dataset, in which all U.S. banks have to report their *branch*-level total deposits as of end-of-June of each year to the Federal Deposit Insurance Corporation. We use *branch*-level deposits data because after 1994 it became more difficult to account for out-of-banking organizations' deposits, loans, or total assets using *bank*-level balance sheet data from the Call Reports: IBBEA of 1994, which into effect in 1995, allowed banks to collapse their existing banks and/or Bank Holding Company (BHC) structures into simpler organizations, allowing banking operations spread over a number of states to be represented by a single balance sheet that does not partition its contents per state of operation. We also collect information on the holding company structures from the Y-9 datasets, in which BHCs report their income statements and balance sheets on a quarterly basis. We

[9] Unfortunately, in OM or EL data we cannot separate out own-state shipments, which are not available from another source either.

account for the holding company structure since, prior to the IBBEA, inter-state bank entry occurred mostly through the acquisition of banks or BHCs in a given state by BHCs from other states, something that was allowed to the extent the two states' laws permitted it. The practice continued during post-IBBEA period, but banks were allowed to simplify their organizational structures across state-lines. Moreover, post-IBBEA (i.e., starting with 1995) banks were allowed to open *de novo* branches in other states without having to own or create a new bank. Hence we use the SoD to properly account for the presence of out-of-state banks. We match SoD and Y-9 data to calculate: (i) deposits held at branches of banks whose highest-BHC is an out-of-state institution and (ii) state-level total deposits of banks. We define banking integration (*BANKING_INTEGRATION*) as the ratio of the state-level deposits held at branches whose ultimate holder is an out-of-state BHC to the state-level total deposits held in all bank branches in the state. Since the SoD data are available as of June 30th each year, we lag this variable by one year to avoid the possibility that out-of-state bank entry into a state be jointly determined with that state's exports.

The above variable, even if lagged, may still be correlated with exports – states that enjoy long-run export growth may attract entry by out-of-state banks seeking to diversify their commercial and industrial lending portfolios by extending loans to manufacturers in new markets. As a result, we instrument the market share of out of state banks with variables describing inter-state banking deregulation that occurred in the United States between 1978-1995.

It can be argued that the growth of state exports abroad were not the driver of bank-entry deregulation *vis-à-vis* other states. Before IBBEA came into effect 1995, inter-state banking was possible between two states only if a state's legislature allowed entry by banks of one or more states. Most states opened up their banking systems to some or all states at once on a reciprocal basis. That is, state i would grant to state m's banks the permission to acquire (or merge with) its banks only if the state m would grant the same privileges to i's banks. Therefore the effective opening dates, which are the deregulatory events used in our models, would not only depend on the state that deregulated based on reciprocity (possibly an endogenous decision), but also on the counterparty states' willingness to reciprocate (unlikely to be an endogenous decision from the point of view of the first state that initiated the deregulation). For example, the first state to deregulate entry into its banking market was Maine in 1978, and it did so in a national reciprocal manner. The effective opening of the Maine banking market, however, occurred first time effectively in 1982 when some other states reciprocated the priveleges to Maine banks. As of 1991, 36 states had already allowed some but not all states banks' to enter their markets. Twelve states in our sample opened up their banking systems non-reciprocally (unconditionally) towards Multi-Bank Holding Companies (MBHCs) from all other states, and they did so prior to 1992.

Existing explanations of inter-state banking deregulation dates point to different political economy factors that do not to include exports-driven concerns of lobbying groups (Kane, 1996, and Kroszner and Strahan, 1999). The main economic arguments advanced for the deregulation process to occur was to allow banks to have asset and deposit diversification, increase competition in banking markets so as to improve customer services and loan availability and to let banks exploit economies of scale and scope in back office operations. In practice, according to existing research, deregulation was driven by the importance of lobbying groups such as small banks that were against deregulation for fear of loss of local market power, insurance companies that opposed banks' sale of insurance products at their expense, and small businesses that were for deregulation in order to access cheaper financing (Kroszner and Strahan, 1999). Kane (1996) further argues that the bank and thrift (i.e., savings and loans) failures, which occurred in separate waves in different regions due to different economic shocks, were important triggers of financial deregulation. The concept of regional bank-opening was prevalent among the states during the earlier period of deregulation; states (and pressure groups) typically feared an unconditional opening of their banking systems would lead to acquisitions by large money-center banks. One idea behind regional liberalization of inter-state banking was that such deregulation would lead to a creation of regional banks that, in the event of a nationwide deregulation, would be strong enough to compete with these money-center banks. If anything, then, this would be against exporter interests: they would like the money-center banks (traditional providers of trade finance at the time) to locate near them and offer trade services cheaper and more quickly.

We collect inter-state banking entry deregulation dates from Amel (2000) that are the basis for constructing our banking integration instruments, for which we adapt the ones used in Morgan, Rime and Strahan (2004) to our particular setting. First, we find the effective dates (years) of deregulation, which are established to make sure that the deregulation by a given state *i* could possibly lead to integration of its banking sector with those of all the relevant other states. For example, if state *i* allows entry by all the other states on a *reciprocal* basis, entry of another state's banks into state *i*'s banking sector cannot occur unless the former state allows access of state *i*'s banks into its own banking sector. Our first instrument is the square-root of the number of *full* years since the state had effectively deregulated its banking market and allowed out-of-state BHCs to enter for the first-time from any other state. If the state for example deregulated in 1992 for the first time (which is the case of Kansas), this variable takes on the value of zero in 1992, 1.000 in 1993 and 1.414 in 1994, etc. We use the square-root of the years since initial deregulation year (*SQRT(YEARS_OPEN)*), because deregulations that took place long-ago should have less of an impact for banking integration than those occurring more recently.[10] We lag this instrument by two years (L2.*SQRT(YEARS_OPEN)*) so that it would be lagged by one year with respect to the lagged-instrumented variable (L1.*ln(BANK_INTEGRATION)*), which is measured as of June 30th of any given year: this allows us to account for the *full* number of years since deregulation in a way that would be contemporaneous with (*ln(BANK_INTEGRATION)*).[11] Our second instrument, *D_OPEN* is an indicator variable that is equal to 1 in a given year if a state deregulated entry towards at least one other state in the said year, and zero otherwise. Note that this variable changes back to zero if no new deregulation takes place for the said state in the next year. *D_OPEN* is lagged by one year with respect to exports to explain contemporaneously the changes in the instrumented variable (*ln(BANK_INTEGRATION)*), which is also lagged by one year with respect to the dependent variable of our IV-regressions (*ln(FEXPORTS/DSHIPMENTS)*).

4.2.3. Other variables

We also rely on the following variables in some of the specifications in which the large number of indicator variables becomes computationally costly. We use country GDP levels (adjusted for purchasing power parity or PPP), *ln(DESTINATION_GDP),* taken from the Penn World Tables (version 6.3), to account for destination country's income and purchasing power. We expect this variable's coefficient estimate to be close to positive one, in line with the gravity model estimates in the literature. We use the logarithm of the "great-circle" distance between the capital cities for each state-country pair to account for trade barriers. Geographical variables for state-country distance calculations are taken from CEPII datasets. We expect the coefficient estimate of *ln(DISTANCE)* to be between -0.5 and -1.0, again in line with the gravity equation estimates in the international trade literature.[12] We control for the free trade agreements (*D_FTA*) between the U.S. and countries with an indicator variable that is equal to 1 if such an agreement is in place in a given year, or zero otherwise. This "dummy" controls in particular for the NAFTA that entered into force in 1994, as the United States did not enter into any other trade agreements in the time period we study. The free-trade agreements already in place would be absorbed by country fixed-effects. *D_CLS_LIMITS* indicator variable controls for the potential credit constraints while exporting to a given destination country: it is equal to 1 if the U.S. ExIm Bank has any restrictions, as published in the Bank's Country Limitation Schedules (CLS), on lending for or

[10] A natural alternative would have been to use the logarithm of the years since deregulation, which we rule out as some states, such as Kansas and Montana, deregulated for the first-time during our sample years, and hence had zeros in our dataset for the years preceding their very-first deregulation.

[11] In this we follow Morgan, Rime and Strahan (2004): lagging allows for bank mergers and acquisitions, through which banking integration proceeded, to start to take place (see their footnote 10 on page 1565).

[12] For Canada and Mexico, we use state-to-nearest-provincial capital distances, even though exports number remain at the U.S.-state to Canada or Mexico (i.e., at the country) level.

guaranteeing lending for export transactions to a given country, and zero otherwise. We use this particular measure in *lieu* of other ones (such as sovereign credit ratings or Institutional Investor country credit ratings) for numerous reasons. First, the U.S. ExIm Bank limitation schedules directly pertain to trade transactions and those directly from the United States, taking into account not only economic but also political risk relevant to U.S. companies. Second, country limitations would affect the lending of private banks for trade transactions either by restricting credit directly or indicating the riskiness of a transaction to the market. Lastly, CLS limits is available for a wider number of countries (all of which are in our sample) than the alternatives cited above.

4.2.4. Summary statistics

The summary statistics for our datasets are presented in Table 1. The average ratio of exports (to one destination) to domestic shipments is small: 0.08% in the case of OM and EL data. The minimum is zero, which means that no trade took place between some states and some countries in some years. The maximum is 56.64% for the OM data (which means that exports to one destination were more than 50% of domestic shipments) and 52.23% for the EL data. This is a rather rare case of Vermont (a small border state) in 1993 for its exports to Canada.

On average, the typical destination country has a GDP of 16.7 billion (=$e^{16.63156}$) U.S. dollars (international-PPP adjusted), and is 8,393 (=$e^{9.0351}$) kilometers away from the typical state's capital. The number of constrained countries in terms of export lending at one point or the other is 74 or 50% of all destinations. This somewhat large number is due to the fact that our dataset covers 148 countries, many of them small emerging economies. As a result, for 44.6% of the observations *D_CLS_LIMITS,* which captures whether export lending to a particular destination by the ExIm Bank was constrained in a given year, takes on the value of 1. There is one country – Israel – with which the USA has a free trade agreement at the beginning of the sample (which is captured by the fixed- or time-varying country effect). After the creation of NAFTA in 1994, Canada and Mexico are the other two countries with which the US has such an agreement.

There is a lot of variation in the variable measuring bank integration, *BANK_INTEGRATION*, (share of out-of-state banks in the deposit market). It attains a minimum of 0.14% for North Carolina in 1991 and maximum of 95.7% for Arizona in 1995. On average, banking integration is at 27.3% for the average state. The first variable that we use as an instrument, L2.*SQRT(YEARS_OPEN),* has an average value of 2.45. The maximum is attained for Maine, with 13 from years of effective openness in 1995, and with a minimum of 0 for several states prior to 1994. In the period 1991-1995 we have 37 of the states in our sample granting access to out-of-state BHCs in at least one of the years so that our second instrument, L1.*D_OPEN* takes a value of 1 in the deregulation year. L1.*D_OPEN* is always zero for states that deregulated in a national-nonreciprocal way (i.e., to all states without any reciprocity conditions) prior to 1991. Thirty-six states had to deregulate access because of federal legislation imposed on them by the 1994 IBBEA, which came into effect in 1995.

To get a better understanding of our data, we also provide information on other dimensions. The average yearly export *flow*s from a state to a destination country is 57.83 millions of U.S. dollars for OM data (*FEXPORTS_OM*) and 61.00 millions of U.S. dollars for EL data (*FEXPORTS_EL*), whereas the average total shipment (*TSHIPMENTS*) from a state in manufactures, which include exports to foreign countries, is 69.78 billions of U.S. dollars. The largest recorded shipment in the OM data is from Texas to Mexico in 1996, of 23.61 billion U.S. dollars, while it is 21.46 billion for the EL data for the exports from Michigan to Canada in 1996. There is a great variation in the data in terms of the served countries by the states, as pictured by the number of years each destination is served (shown in Figure 1). In the the OM (EL) data 66.13% (68.14%) of countries are served by exports for all the years the data are available, while 9.85% (11.19%) of state-country pairs never experience exports in the OM (EL) data.[13] There are a non-trivial number of state-country pairs that do not trade all the time, something we account for in our empirical work.

5. Results

Our results consist of three sets of empirical model estimates. First, we estimate log-linear gravity equations of foreign exports-to-domestic shipments and present the results in Tables 2 and 3. Second, given the various deficiencies of log-linear gravity models (to be discussed in detail further below), we estimate various Poisson regression models and present their results in Tables 4 and 5. Third, and finally, we re-estimate our preferred Poisson regression model capital- versus non-capital-intensive industry sectors to explore sub-sample-level variation in our data and report the results in Table 6.

5.1. Log-linear gravity equation models

We estimate the following empirical gravity equation in the log-linear form:

$$\ln(FEXPORTS/DSHIPMENTS_{i,j,t}) = \alpha + \beta L1.\ln(BANK\_INTEGRATION_{i,t}) + \sum_{t=1993}^{1996}\gamma_t D\_Y_t + \sum_{i=1}^{48}\sum_{j=1}^{148}\gamma_{i,j} D\_S\&C_{i,j} + + \sum_{j=1}^{148}\sum_{t=1993}^{1996}\gamma_{j,t} D\_C\&Y_{j,t} + \varepsilon_{i,j,t} \qquad (6)$$

where, D_Y_t are year fixed-effects, $D\_S\&C_{i,j}$ are origin state-and-country of destination *pair* fixed-effects, $D\_Y\&C_{j,t}$ are time-varying destination country fixed-effects. Note that, while simple, this specification

[13] In our sample there is trade between the U.S. and each country in each year, so at least one state should have positive exports towards any country in our sample.

soaks up all of the variation in the dependent variable, except in the *i* and *t* dimension: if we were to include year-varying indicator variables for states' that are the origins of exports, there would be no more variation left to be explained in the dependent variable. Our dummies take care thus of such important trade flow determinants as destination GDP and price level, physical distance to the destination, the general level of tariffs at the destination, cultural proximity etc. The coefficient β measures the percentage change in the foreign exports to domestic shipments ratio ($FEXPORTS/DSHIPMENTS_{i,j,t}$) for a given change in the lagged bank integration for state *i* (L1.ln($BANK\_INTEGRATION_{i,t}$)). The latter is proxied by the one-year lagged market share of out-of-state banks in state *i* in year *t*. We estimate the empirical equation (6) first using a *Within* estimator and second using Instrumental Variables-Fixed Effects regression with a 2-Stage General Method of Moments (IV-FE-2SGMM) estimator, for both the OM and the EL data. We check for the success of our identification strategy by conducting standard under-, weak- and over-identification tests, for which we report the results at the bottom of our tables whenever possible.

In Table 2, we report the estimates for the log-linear model using the "full" samples of OM and EL data: these samples exclude the cases of zero exports from certain states to certain countries (a point to which we will come back to in Section 5.2). The results of the log-linear *Within* model with the OM data are presented in column (1): the coefficient estimate for bank integration is -0.0140, which is not significant statistically. If bank integration were endogenous to state's industrial production, which may be the case even when it is lagged in case it is persistent over time, these estimates would be biased and inconsistent (see for example, Cameron and Trivedi, 2005, p. 96). In column (2), the IV-estimate for β is 0.0970, which is statistically significant at the 10%-level: one percent increase in banking integration (as measured by the market share of out-of-state banks) would increase foreign exports by almost 0.10% *over-and-above* the increase in domestic shipments, a $1/10^{\text{th}}$ increase.

To check the validity of our IV-estimation, we conduct a series of identification tests, which are presented at the bottom of column (2) of Table 2. First, the *under-identification* test strongly rejects (at the 1% level) the null hypothesis that our model is under-identified. The result of this test indicates that the rank condition necessary and sufficient for identification of our model is satisfied. However, the rejection of under-identification test does not rule out the problem of weak instruments, which we test for in a second step. It should be noted that with valid instruments IV-estimates are asymptotically consistent but inevitably biased in finite samples (see, for example, Cameron and Trivedi, 2005, p.108). The question is whether the IV bias is at a tolerable level, such as 5% or 10% of the OLS bias. The *weak-identification* test rejects the null hypothesis of weak instruments with a Stock and Yogo (2005) critical value that corresponds to a 10%-level. This critical threshold value suggests that the finite-sample bias of the IV estimate is in the lowest range. Finally, in the *over-identification* test we check for the validity of over-identifying restrictions of our model. The underlying joint-null hypothesis is that our instruments are valid and that the exclusion restrictions imposed on the instruments are correct. The reported Hansen-J statistic cannot reject the null, validating the over-identifying restrictions imposed on the model. The results of these tests suggest that our empirical model is properly identified, does not suffer from the weak instruments problem, and has a minimal finite-sample IV bias compared to the OLS bias.

In columns (3) and (4) of Table 2, we revisit the same model with EL data, and find similar results: the coefficient estimate for L1.ln($BANK\_INTEGRATION_{i,t}$) is equal to -0.0043, which is not statistically significant, in the *Within* regression of column (3); but it is equal to 0.1080, albeit statistically significant at the 10%-level, in the IV-FE-2SGMM regression. One caveat for the latter estimate is that, even though we reject the *under-identification* and *weak-identification* tests as we should, we also reject the *over-identification* test, even if marginally at the 10%-level: the marginal rejection of the Hansen J-test may suggest that either our set of instruments are not valid and/or that the exclusion restrictions are not valid in the particular case of the "full" sample EL data.[14]

These coefficient estimates indicate a given increase in banking integration would increase the state-level exports to foreign destinations by approximately 1/10th of that amount. However, the estimates for β include both the effects of the intensive margin (if one were to define it changes in exports to existing countries) and the extensive (if one were to define it as changes in destinations served by states). In Table 3 we focus on the intensive margin, as defined above, by re-estimation specification (6) for the "square panels", i.e., for state-country pairs with non-zero exports in all years of OM or EL data (which correspond to 66% to 68% of the OM and EL data, respectively). Our results are consistent with those of the unbalanced panel in Table 2. The *Within* estimate of β, which is potentially biased and inconsistent, is equal to -0.0121 for the OM data (column (1)) and to -0.0134 for the EL data (column (3)) with neither estimate being statistically significant. The IV-estimate of β is equal to 0.0938 for the OM data (column (2)) and to 0.1007 for the EL data (column (4)), both of which are statistically significant at the 10%-level. In both cases our tests reject both *under*- and *weak-identification* tests, but cannot reject the *over-identification* tests: our identification strategy appears to work, allowing us to obtain consistent estimates that are only marginally biased (given that all IV estimates are biased to a degree). We also observe that our square-panel IV-estimates of Table 3, which are exclusive of the extensive margin effects at the state-country level, are very similar to the corresponding estimates with the "full" sample in Table 2: the IV-estimate of β with OM data is equal to 0.0938 in Table 3 compared to 0.0970 in Table 2, whereas the same estimate with EL data is equal to 0.1007 in Table 3 compared to 0.1080 in Table 2. What may be driving these similar estimates is the fact that the "full" sample in the log-linear case excludes zero export

[14] Note that the number of observations varies across columns in Table 3 for the unbalanced panels, because IV-FE-2SGMM estimator drops "singleton" observations whereas the *Within* estimator does not.

data observations: the results in Table 2, which include both the effects of the intensive and the extensive margins at the state-country level, incorporate very little, if any, of the latter given that changes from zero export levels are excluded from the "full" sample.

Santos Silva and Tenreyro (2006) point out that OLS estimates of log-linear models would lead to potentially biased and inconsistent estimates for at least two reasons. First, as noted above, the log-linear specification does not consider information contained in zero export-to-domestic shipments that we observe in the data for certain state-country pairs. This would lead to biased gravity equation estimates. Second, as pointed out in Santos Silva and Tenreyro (2006), the Jensen's inequality inherent in log-linear specifications (the expected value of a logged variable not being equal to the logarithm of the expected value) engenders a systematic heteroskedasticity problem that would lead to inconsistent OLS estimates. We follow these authors' suggestion and estimate Poisson regressions to get around these problems and obtain consistent and unbiased estimates.

5.3. Poisson regression models

However, we face a number of problems in estimating specification (6) with IV regressions in a Poisson setting. First, while we can use a *Within*-Poisson estimator, there is no equivalent of a IV-FE-2SGMM estimator for Poisson. The best we can do is to use an IV-Poisson estimator and add indicator variables for the various sets of fixed-effects. Unfortunately, this pragmatic solution is very costly in terms of computational requirements given that we would like to add 7,700 indicator variables for the OM data (7,551 for the EL data) to account for all the fixed effects in (6): up to 4 for year-effects plus 48×148 state-country pair fixed effects plus up to 148×4 country-year fixed effects, depending on data we use. To reduce the computational burden, we chose to estimate a gravity equation specification with only separate year-, state-, and country-fixed-effects, which reduces the number of indicator variables down to a manageable 200 (=4+48+148), by adding other variables that vary by state-and-country, country-and-year as well as time:

$$FEXPORTS/DSHIPMENTS_{i,j,t} = \exp\left[\alpha + \beta L1.\ln(BANK\_INTEGRATION_{i,t}) \right.$$

$$+ \lambda_1 \ln(DESTINATION\_GDP_{j,t}) + \lambda_2 \ln(DISTANCE_{i,j})$$

$$+ \lambda_3 D\_FTA_{i,j,t} + \lambda_4 D\_CLS\_LIMITS_{j,t} \tag{7}$$

$$\left. + \sum_{t=1993}^{1996} \gamma_t D\_Y_t + \sum_{i=1}^{48} \gamma_i D\_S_i + \sum_{j=1}^{148} \gamma_j D\_C_j \right] + \varepsilon_{i,j,t}$$

where, D_Y_t are year t fixed-effects; D_S_i are origin-state fixed-effects for state i; D_C_j are destination-country j fixed-effects; ln($GDP\_DESTINATION_{j,t}$) is the the logarithm of the gross domestic product (GDP) of the destination country adjusted by international purchasing price parity (PPP)[15]; ln($DISTANCE_{i,j}$) is the distance between state and country capitals; $D\_FTA_{i,j,t}$ is an indicator variable that equals 1 if there is a free trade agreement between the U.S. and country j in year t, and 0 otherwise; and $D\_CLS\_LIMITS_{j,t}$ is an indicator variable that equals 1 if the U.S. ExIm Bank imposes credit or loan guarantee limits on country j in year t, and 0 otherwise. In IV-Poisson regressions for (7) L1.ln($BANK\_INTEGRATION_{i,t}$) is instrumented the same way as in (6).

Before we proceed to estimate (7) with IV-Poisson, we first check whether we obtain similar coefficient estimates for β in (6) using a *Within*-Poisson estimator and (7) using a Poisson estimator with 200 indicator variables. The results are presented in Table 4, columns (1) and (2) for OM data: the estimate for β is 0.0599 (statistically significant at the 5%-level) in (6) and 0.0690 (statistically significant at the 5%-level) in (7). We also obtain similar results for EL data (presented in columns (4) and (5) of Table 4): the estimate for β is 0.0549 (statistically significant at the 10%-level) in (6) and 0.0581 (statistically significant at the 10%-level) in (7). The similarity of the β estimates gives us comfort that the variables we added in (7) to replace the state-country-pair and the year-varying-country "dummies" in (6), do a proper job at soaking-up the variation in the same dimensions. Moreover, the coefficient estimates for ln($GDP\_DESTINATION_{j,t}$) and ln($DISTANCE_{i,j}$) are along the lines found in the empirical international trade literature: in Table 4 the coefficient estimate for ln($GDP\_DESTINATION_{j,t}$) is close to +1 (1.1890 and 1.2159 in columns (2) and (3) for OM data, 0.8559 and 0.8761 in columns (5) and (6) for EL data, respectively, all of which are statistically significant at the 1%-level); whereas the coefficient

[15] Since we use PPP for country j to adjust its GDP, we do not add the former as a separate explanatory variable as it is typically done in the empirical gravity equation models of international trade.

estimate for ln($DISTANCE_{i,j}$) is close to -1 (-0.8940, -0.8912, -0.8416, and -0.8383 in columns (2), (3), (5) and (6), respectively, all statistically significant at the 1%-level). The $D\_FTA_{i,j,t}$ and $D\_CLS\_LIMITS_{j,t}$ variables perform less well. The coefficient estimates for $D\_FTA_{i,j,t}$ are unexpectedly negative but statistically *insignificant*, whereas the coefficient estimates for $D\_CLS\_LIMITS_{j,t}$ have the expected negative sign but are not statistically significant either.

Next, we turn our attention to estimates of β in (7), which are estimated with Poisson and IV-Poisson estimators using the full-sample and are presented in Table 4. We compare them with the estimates of β in (6), which are estimated with *Within* and IV-FE-2SGMM estimators using unbalanced-panel of non-zero export observations (the "full" sample in the log-linear case) and are presented in Table 2. First, using OM data, we note that while the estimate for β in (6) using *Within*-OLS estimator is equal to -0.0045 and statistically *insignificant* (column (1) of Table 2), its (7) counterpart in column (2) of Table 4 is equal to 0.0690 and statistically significant at the 5%-level.

More importantly, the result in column (3) of Table 4 suggests that the estimate for β when L1.ln($BANK\_INTEGRATION_{i,t}$) is instrumented is equal to 0.1846 (statistically significant at the 1%-level). In other words, a 1% increase in banking integration (i.e., as proxied by a 1% increase in the market share of out-of-state banks) leads to a 0.1846% increase in exports over-and-above domestic shipments to the rest of the U.S. Doing the same exercise with the EL data, in the last two columns of Table 4 we observe a coefficient estimate of 0.0581 (statistically significant at the 10%-level) for the non-instrumented L1.ln($BANK\_INTEGRATION_{i,t}$) and an IV-estimate of 0.1644 (statistically significant at the 5%-level) compared to -0.0043 (statistically insignificant) and 0.1080 (statistically significant at 10%-level) in the last two columns of Table 2.

We draw a number of conclusions from these results. First, log-linear models, even the ones estimated with IV-FE-2SGMM, under-estimate the impact of bank integration. Dropping the observations in which trade between certain state-country pairs is zero biases the estimates downwards: we observe less of an impact for bank integration given that some of the larger changes in exports that would be due to increases with respect to zero are ignored in a log-linear setting. Moreover, log-linear estimates (even when estimated with IV-methods), are likely to be inconsistent (Santos Silva and Tenreyro, 2006). Second, the measured impact of banking integration varies very little irrespective of whether the estimate is obtained from OM or EL data. In Table 4 the IV-Poisson estimates for β differ by 0.0202, in Table 2 by 0.0110. This suggests that the differences in the construction of the OM and EL data, which are due to differences in aggregation methods based on two different location fields in the SED forms, make little difference, despite the fact the EL data have one less year of observations than the OM data.

In Table 5, in order to focus on intensive margin alone, we revisit regressions of equation (7) using the square-panel of state-country pairs that had non-zero exports in all the years of the data, and compare them with the corresponding estimates in Table 4. We observe that the *square* panel IV-Poisson coefficient estimates for β in Table 5 are systematically smaller than the comparable estimates in Table 4: with the OM data the estimate for β is equal to 0.1442 versus 0.1846 (in columns (3) of Table 5 and 4, respectively), with the EL data the same estimate is equal to 0.1190 versus 0.1644 (in columns (6) of Table 5 and 4, respectively). This suggests that roughly 22% to 28% of the banking integration effect that we observe can be attributed to state-to-country extensive margin, while the remainder to the intensive margin. We would like to emphasize that the intensive and extensive margins in our state-to-country exports setting would necessarily differ from their firm-to-country counterparts. The state-country level extensive margin that we estimate is potentially a lower bound for the *firm*-level extensive margin aggregated at the state-level: due to the aggregation scheme used in the FTD data (be it OM or EL), *first-time* exports of firms in state i to country j would be subsumed in the *intensive* margin between states i and j, rather being counted towards the *extensive* margin as they should. As such, our state-to-country *intensive* margin estimate is likely to be overstated compared to aggregated *firm*-level intensive margins between state i and country j.

5.3. Poisson regression estimates for capital intensity

Next, to gain further insight, we examine whether the observed effect of bank integration on foreign exports to domestic shipments ratio varies by types of industry categories. For this analysis we turn to the SIC-level data and aggregate trade flows and domestic shipments by different industry types.

In Table 6 we present the results of the estimation for capital intensive and non-intensive industries. Capital intensity is defined as the fraction of gross operating surplus (remuneration of capital) to total value added in the industry, and measured for 1991, the year preceding the start of our sample, for the entire U.S. in a given sector. Industries that were in the upper 50% are labeled as capital intensive, and flows and shipment data was aggregated for these industries. The same procedure is performed for the industries with capital intensity lower than the median.

The results shown in Table 6 are interesting and at first may appear puzzling. We observe – both for the OM and the EL data – that the estimate of β for L1.ln(*BANK_INTEGRATION*$_{i,t}$) is positive and statistically significant for capital non-intensive industries (column 2 and 4 for the OM and the EL data respectively) while it is negative (albeit not statistically significant) for the capital intensive industries (column 1 and 3 for the OM and EL data respectively). For non-capital intensive industries, we obtain a result that a 1% increase in banking integration would increase the ratio of foreign to domestic shipments by 0.168%. How can we interpret these findings? The dependent variable is a ratio of exports to domestic shipments. It may be that – for capital intensive industries – the domestic shipments increased *faster* than exports as more capital and trade services were provided by integrating banks. For industries where capital was not an important factor of production and hence the domestic supply of it for production was not a vital constraint – i.e. the capital non-intensive sectors – banking integration could have a stronger effect as it were trade related barriers that would be removed once integration took place.

We are in the process of extending our analysis to different sub-sectors (for example, sectors that are more dependent on external finance, those in which there is high asset tangibility, etc., as in Manova, 2009) to corroborate and refine the findings of this sub-section.

6. Conclusions and Forthcoming Extensions

In this paper, we use a careful IV-based identification strategy and find that a 1% increase in banking integration between U.S. states caused a 0.164-0.184% increase in the exports/domestic shipments ratio for U.S. state-level exports in the years 1992-1996. This increase in openness can be attributed to an increase in capital to cover variable and fixed export costs relative to domestic shipping costs and a higher provision of trade finance services. The extensive margin of trade accounts for at least 22% to 28% of the banking integration effect on the openness ratio.

In further work, we wish to extend our analysis to sector-level data (at the 2-digit SIC code level) to test whether financial integration affects certain manufacturing sectors more than others: durable versus non-durable goods, more or less external finance-dependent sectors, sectors with higher or lower asset tangibility, etc. We would like also to disentangle the role of trade-specific finance from a more general increase in the availability of finance, the two channels through which bank integration theoretically affects exports (as in Section 3). For this purpose, we are in the process of obtaining a detailed list of trade-finance specialist banks from the U.S. Exim Bank through the U.S. "Freedom of Information Act". Banks that do not specialize in trade-related services and that enter a state may not have more international expertise than the local ones. In contrats, if a bank that specializes in trade finance expands into the same state, the effect on the latter's exports may be much higher.

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Table 1. Descriptive Statistics

This table presents the summary statistics for the variables used in the analysis. The dependent variables come from the Federal Trade Division surveys of state exports for 48 states. Two datasets using different methodologies were collected: Origin of Movement (OM) data between 1992-1996 and Exporter Location (EL) data between 1993-1996. Our sample consists of the 48 contiguous states and 148 countries, which result in 35,520 state-destination-year observations for the OM data (=48×148×5) and 24,816 for the EL data (=48×148×4). We exclude Alaska, Hawaii, and the District of Columbia. *FEXPORTS/DSHIPMENTS_OM* and *FEXPORTS/DSHIPMENTS_EL* are the ratios of foreign exports from a U.S. state to a foreign country to the domestic shipments to the rest of the U.S. for the OM and EL data, respectively. *ln(FEXPORTS/DSHIPMENTS_OM)* and *ln(FEXPORTS/DSHIPMENTS_EL)* are the natural logarithms of these ratios. The explanatory variables are as follows: *ln(DESTINATION_GDP)* is the destination country GDP at PPP prices calculated from the Penn World Table version 6.3; *ln(DISTANCE)* is the logarithm of distance (in thousand kilometers) between origin-state and destination-country capitals; *D_CLS_LIMIT* is a dummy variable equal to 1 if the U.S. ExIm Bank would have any restrictions on lending or on providing guarantees to exports from the U.S. to a destination country in a given year and zero otherwise; *D_FTA* is a dummy equal 1 if the destination country had a free trade agreement with the U.S. in a given year and zero otherwise. The endogenous variable L1.ln*(BANK_INTEGRATION)* is the logarithm of the fraction of banking assets owned by out-of-state banks in a given year, lagged one year. IVs are adapted from Morgan, Rime and Strahan (2004) to this panel dataset: L2.*SQRT(YRS_OPEN)* is the square root of the number of full years since the state has deregulated inter-state banking entry for the first time (contemporaneous with the instrumented variable, i.e., lagged by one year with respect to the dependent variables *ln(FEXPORTS/DSHIPMENTS_OM)* or *ln(FEXPORTS/DSHIPMENTS_EL)*); and L1.D*_OPEN* is an indicator variable that equals 1 if the state has deregulated entry in a given year to at least one state, and 0 otherwise (lagged by one year with respect to the instrumented variable L1.ln*(BANK_INTEGRATION)*, i.e., lagged by two years with respect to the dependent. The variables that additionally describe the dataset are *FEXPORTS_OM* and *FEXPORTS_EL,* the value (in millions of U.S. dollars) of exports from states to a country in a given year respectively for the OM and EL data; *TSHIPMENTS* is the total value of shipments in manufacturing (inclusive of total exports) from a state in a given year in millions of U.S. dollars; *YRS_DEST_SERVED_OM* and *YRS_DEST_SERVED_EL* are the average number of years a country is served within the sample for the "OM" and "EL" data respectively.

Variable	**Number of observations**	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Dependent variables :					
FEXPORTS/DSHIPMENTS_OM	35,520	0.0008	0.0069	0	0.5664
FEXPORTS/DSHIPMENTS_EL	28,416	0.0008	0.0064	0	0.5223
ln(FEXPORTS/DSHIPMENTS_OM)	27,741	-10.2380	3.0761	-18.2642	-0.5683
ln(FEXPORTS/DSHIPMENTS_EL)	22,297	-10.2584	3.0772	-18.1988	-.6493
Explanatory variables:					
ln(DESTINATION_GDP)	35,520	16.6315	2.2843	11.4134	21.8347
ln(DISTANCE)	35,520	9.0351	0.5561	4.8029	9.8218
D_CLS_LIMIT	35,520	0.4459	0.4970	0	1

D_FTA	35,520	0.0149	0.1210	0	1
Endogenous (Instrumented) Variable:					
L1.BANK_INTEGRATION	35,520	0.2731	0.2258	0.0015	0.9570
L1.ln(BANK_INTEGRATION)	35,520	-1.8707	1.3359	-6.5257	-0.0439
Instrumental (Excluded) Variables (IVs):					
L2.SQRT(YRS_OPEN)	35,520	2.4530	0.6610	0	3.6056
L1.D_OPEN	35,520	0.4625	0.4987	0	1
Other Variables describing the dataset:					
FEXPORTS_OM	35,520	57.83	449.02	0	23,614.16
FEXPORTS_EL	28,416	61.00	454.29	0	21,465.34
TSHIPMENTS	35,520	69,783.94	70,174.57	2,382.40	368,328.70
YRS_DEST_SERVED_OM	35,520	3.90	1.76	0	5
YRS_DEST_SERVED_EL	28,416	3.14	1.42	0	4

Table 2. Log-linear Gravity Equation Estimates with "Full" Samples

This table presents the estimates of the log-linear gravity equation models with the "full" samples (unbalanced panel, excluding zero exports) of the Origin of Movement (OM) and Exporter Location (EL) data. The dependent variable is the natural logarithm of foreign exports to domestic shipments ratio (*ln(FEXPORTS/DSHIPMENTS)*). The explanatory variable is the log of bank integration variable lagged by one year (L1.ln(*BANK_INTEGRATION*)), for which the instruments are the square-root of years since first inter-state banking deregulation lagged two-years (L2.*SQRT(YEARS_OPEN)*) and an indicator variable that is equal to 1 if a state opens its banking market to one or more states in a given year, and zero otherwise, lagged by one year (L1.*D_OPEN*). All regressions include (i) origin (U.S.) state-country pair, (ii) year, and (iii) year-and-country of destination fixed-effects. The null hypothesis for *the under-identification test* is that the matrix of reduced form coefficients has rank k-1 (i.e., under-indentified). The null hypothesis for *the weak-identification test* is that the equation is weakly identified. The null hypothesis for *the over-identification test* is that all instruments are valid instruments (i.e., uncorrelated with the error term) *and* that the instruments are correctly excluded from the estimated equation. Standard-errors and identification tests account for clustering of observations at the origin state-destination country level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	OM Data			**EL Data**		
	***Within* Estimator**	**IV-FE-2SGMM Estimator**		***Within* Estimator**	**IV-FE-2SGMM Estimator**	
	(1)	**(2)**		**(3)**	**(4)**	
L1.ln(*BANK_INTEGRATION*)	-0.0140	0.0970	*	-0.0043	0.1080	*
	(0.81)	(1.82)		(0.21)	(1.81)	
Number of observations	27,741	27,271		22,297	21,784	
IV-estimation	No	Yes		No	Yes	
Fixed Effects						
Year	Yes	Yes		Yes	Yes	
State-Country Pair	Yes	Yes		Yes	Yes	
Country-Year	Yes	Yes		Yes	Yes	
Clustered std. errors (state-country pair-level)	Yes	Yes		Yes	Yes	
Number of clusters	6,404	5,934		6,309	5,796	
F-statistic	.	7.19	***	.	6.52	***
Under-identification test:						
Kleibergen-Paap rk LM statistic		759.11	***		416.16	***
Weak identification test:						
Kleibergen-Paap rk Wald F statistic		458.66	*		254.50	*
Stock-Yogo weak-identification test critical values for 10% maximal IV size		19.93			19.93	
Over-identification test of all instruments:						
Hansen J statistic		0.04			3.50	*
Hansen J p-value		0.8480			0.0613	

Table 3. Log-linear Gravity Equation Estimates with Square Panels

This table presents the estimates of the log-linear gravity equation models with square panels (for state-country pairs with non-zero exports in all of the sample years) of the Origin of Movement (OM) and Exporter Location (EL) data. The dependent variable is the natural logarithm of foreign exports to domestic shipments ratio (*ln(FEXPORTS/DSHIPMENTS)*). The explanatory variable is the log of bank integration variable lagged by one year (L1.ln(*BANK_INTEGRATION*)), for which the instruments are the square-root of years since first inter-state banking deregulation lagged two-years (L2.*SQRT(YEARS_OPEN)*) and an indicator variable that is equal to 1 if a state opens its banking market to one or more states in a given year, and zero otherwise, lagged by one year (L1.*D_OPEN*). All regressions include (i) origin (U.S.) state-country pair, (ii) year, and (iii) year-and-country of destination fixed-effects. The null hypothesis for *the under-identification test* is that the matrix of reduced form coefficients has rank k-1 (i.e., under-indentified). The null hypothesis for *the weak-identification test* is that the equation is weakly identified. The null hypothesis for *the over-identification test* is that all instruments are valid instruments (i.e., uncorrelated with the error term) *and* that the instruments are correctly excluded from the estimated equation. Standard-errors and identification tests account for clustering of observations at the origin state-destination country level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	OM Data			**EL Data**		
	***Within* Estimator (1)**	**IV-FE-2SGMM Estimator (2)**		***Within* Estimator (3)**	**IV-FE-2SGMM Estimator (4)**	
L1.ln(*BANK_INTEGRATION*)	-0.0121	0.0938	*	-0.0134	0.1007	*
	(0.72)	(1.81)		(0.69)	(1.71)	
Number of observations	23,490	23,490		19,364	19,364	
IV-estimation	No	Yes		No	Yes	
Fixed Effects						
Year	Yes	Yes		Yes	Yes	
State-Country Pair	Yes	Yes		Yes	Yes	
Country-Year	Yes	Yes		Yes	Yes	
Clustered std. errors (state-country pair-level)	Yes	Yes		Yes	Yes	
Number of clusters	4,698	4,698		4,841	4,841	
F-statistic	.	9.67		.	9.40	
Under-identification test:						
Kleibergen-Paap rk LM statistic		663.67	***		364.85	***
Weak-identification test:						
Kleibergen-Paap rk Wald F statistic		396.02	*		223.60	*
Stock-Yogo weak-identification test critical values for 10% maximal IV size		19.93			19.93	
Over-identification test of all instruments:						
Hansen J statistic		1.26			1.64	
Hansen J p-value		0.2621			0.2002	

Table 4. Poisson Regression Estimates of the Gravity Equation with Full Samples

This table presents the estimates of the gravity equation models with the full sample (including zero exports for state-country pairs when they occur) of the Origin of Movement (OM) and Exporter Location (EL) data. The dependent variable is the foreign exports to domestic shipments ratio (*FEXPORTS/DSHIPMENTS*). The explanatory variables are the log of destination country's GDP adjusted by international-PPP (ln(*DESTINATION_GDP)*); the log of distance between U.S. state and foreign country capitals in kilometers (ln(*DISTANCE*)); an indicator variable (*D_FTA*) that equals one if there is a free trade agreement between the U.S. and country of destination, and zero otherwise; and an indicator variable (*D_CLS_LIMITS*) that equals one if the U.S. ExIm bank puts restrictions on the destination country loans or loan guarantees, and zero otherwise. The instrumented endogenous variable is the log of bank integration variable lagged by one year (L1.ln(*BANK_INTEGRATION*)), for which the instruments are the square-root of years since first inter-state banking deregulation lagged two-years (L2.*SQRT(YEARS_OPEN)*) and an indicator variable that is equal to 1 if a state opens its banking market to one or more states in a given year, and zero otherwise, lagged by one year (L1.*D_OPEN*). Fixed-effects, which vary between regressions, are listed below. In all regressions the standard-errors account for clustering of observations at the origin state-destination country level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	OM Data						EL Data					
	Poisson-*Within* Estimator		**Poisson Estimator**		**Poisson-IV Estimator**		**Poisson-*Within* Estimator**		**Poisson Estimator**		**Poisson-IV Estimator**	
	(1)		**(2)**		**(3)**		**(4)**		**(5)**		**(6)**	
L1.ln(*BANK_INTEGRATION*)	0.0599	**	0.0690	**	0.1846	***	0.0549	*	0.0581	*	0.1644	**
	(2.42)		(2.50)		(3.39)		(1.87)		(1.71)		(2.47)	
ln(*DESTINATION_GDP*)			1.1890	***	1.2159	***			0.8559	***	0.8761	***
			(5.83)		(7.93)				(2.92)		(4.16)	
ln(*DISTANCE*)			-0.8940	***	-0.8912	***			-0.8416	***	-0.8383	***
			(7.34)		(7.37)				(5.40)		(5.41)	
D_FTA			-0.0460		-0.0514				-0.0468		-0.0510	
			(0.70)		(0.72)				(0.59)		(0.61)	
D_CLS_LIMIT			-0.0189		-0.0217				-0.0306		-0.0352	
			(0.32)		(0.40)				(0.81)		(1.01)	

Number of observations	32,020	35,520	35,520	25,236	28,416	28,416
IV-estimation	No	No	Yes	No	No	Yes
Fixed Effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	No	Yes	Yes	No	Yes	Yes
Country	No	Yes	Yes	No	Yes	Yes
State-Country	Yes	No	No	Yes	No	No
Year-and-County	Yes	No	No	Yes	No	No
Robust Std. Errors	Yes	No	No	Yes	No	No
Clustered std. errors (state-country pair level)	No	Yes	Yes	No	Yes	Yes
Number of clusters		7,104	7,104		7,104	7,104

Table 5. Poisson Regression Estimates of the Gravity Equation with Square Panels

This table presents the estimates of the gravity equation models with the square panel (only state-country pairs with non-zero exports in all years) of the Origin of Movement (OM) and Exporter Location (EL) data. The dependent variable is the foreign exports to domestic shipments ratio (*FEXPORTS/DSHIPMENTS*). The explanatory variables are the log of destination country's GDP adjusted by international-PPP (ln(*DESTINATION_GDP)*); the log of distance between U.S. state and foreign country capitals in kilometers (ln(*DISTANCE*)); an indicator variable (*D_FTA*) that equals one if there is a free trade agreement between the U.S. and country of destination, and zero otherwise; and an indicator variable (*D_CLS_LIMITS*) that equals one if the U.S. ExIm bank puts restrictions on the destination country loans or loan guarantees, and zero otherwise. The instrumented endogenous variable is the log of bank integration variable lagged by one year (L1.ln(*BANK_INTEGRATION*)), for which the instruments are the square-root of years since first inter-state banking deregulation lagged two-years (L2.*SQRT(YEARS_OPEN)*) and an indicator variable that is equal to 1 if a state opens its banking market to one or more states in a given year, and zero otherwise, lagged by one year (L1.*D_OPEN*). Fixed-effects, which vary between regressions, are listed below. In all regressions the standard-errors account for clustering of observations at the origin state-destination country level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	OM Data						**EL Data**					
	Poisson-*Within* Estimator		**Poisson Estimator**		**Poisson-IV Estimator**		**Poisson-*Within* Estimator**		**Poisson Estimator**		**Poisson-IV Estimator**	
	(1)		**(2)**		**(3)**		**(4)**		**(5)**		**(6)**	
L1.ln(*BANK_INTEGRATION*)	0.0603	**	0.0695	**	0.1442	***	0.0552	*	0.0584	*	0.1190	**
	(2.44)		(2.52)		(3.09)		(1.88)		(1.72)		(1.96)	
ln(*DESTINATION_GDP*)			1.1971	***	1.2231	***			0.8479	***	0.8666	***
			(5.84)		(6.34)				(2.88)		(3.19)	
ln(*DISTANCE*)			-0.8902	***	-0.8884	***			-0.8376	***	-0.8348	***
			(7.32)		(7.31)				(5.40)		(5.39)	
D_FTA			-0.0453		-0.0483				-0.0467		-0.0501	
			(0.69)		(0.67)				(0.59)		(0.60)	
D_CLS_LIMIT			-0.0178		-0.0204				-0.0302		-0.0339	
			(0.30)		(0.36)				(0.79)		(0.93)	

Number of observations	23,490	23,490	23,490	19,364	19,364	19,364
IV-estimation	No	No	Yes	No	No	Yes
Fixed Effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	No	Yes	Yes	No	Yes	Yes
Country	No	Yes	Yes	No	Yes	Yes
State-Country	Yes	No	No	Yes	No	No
Year-and-Country	Yes	No	No	Yes	No	No
Robust std. errors	Yes	No	No	Yes	No	No
Clustered std. errors (state-country pair-level)	No	Yes	Yes		Yes	Yes
Number of clusters		4,698	4,698		4,841	4,841

Table 6. Poisson Regression Estimates for Capital Intensive and non-Capital intensive sectors

This table presents the estimates of the gravity equation models for capital intensive and capital nonintensive industries for the Origin of Movement (OM) and Exporter Location (EL) data. The dependent variable is the foreign exports to domestic shipments ratio (*FEXPORTS/DSHIPMENTS*). The explanatory variables are the log of destination country's GDP adjusted by international-PPP (ln(*DESTINATION_GDP)*); the log of distance between U.S. state and foreign country capitals in kilometers (ln(*DISTANCE*)); an indicator variable (*D_FTA*) that equals one if there is a free trade agreement between the U.S. and country of destination, and zero otherwise; and an indicator variable (*D_CLS_LIMITS*) that equals one if the U.S. ExIm bank puts restrictions on the destination country loans or loan guarantees, and zero otherwise. The instrumented endogenous variable is the log of bank integration variable lagged by one year (L1.ln(*BANK_INTEGRATION*)), for which the instruments are the square-root of years since first inter-state banking deregulation lagged two-years (L2.*SQRT(YEARS_OPEN)*) and an indicator variable that is equal to 1 if a state opens its banking market to one or more states in a given year, and zero otherwise, lagged by one year (L1.*D_OPEN*). Fixed-effects are listed below. In all regressions the standard-errors account for clustering of observations at the origin state-destination country level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	OM Data				EL Data			
	Capital Intensive		**Not Capital Intensive**		**Capital Intensive**		**Not Capital Intensive**	
	(1)		**(2)**		**(3)**		**(4)**	
L1.ln(*BANK_INTEGRATION*)	-0.1371		0.1688	**	-0.0270		0.1685	*
	(1.64)		(1.97)		(0.39)		(1.75)	
ln(*DESTINATION_GDP*)	0.8814	***	1.8341	***	0.4618	***	1.6575	***
	(5.16)		(9.37)		(2.69)		(7.84)	
ln(*DISTANCE*)	-0.7632	***	-0.8058	***	-0.5472	***	-0.7285	***
	(6.17)		(6.42)		(4.90)		(5.84)	
D_FTA	-0.0038		0.0244		-0.0573	**	0.0632	
	(0.11)		(0.61)		(2.14)		(1.41)	
D_CLS_LIMIT	-0.0565		-0.0355		-0.0418		-0.0104	
	(0.83)		(0.46)		(1.21)		(0.29)	
Number of observations	35520		35520		28416		28416	
IV-estimation	Yes		Yes		Yes		Yes	
Fixed Effects								
Year	Yes		Yes		Yes		Yes	
State	Yes		Yes		Yes		Yes	
Country	Yes		Yes		Yes		Yes	
State-Country	No		No		No		No	
Year-and-Country	No		No		No		No	
Robust std. errors	No		No		No		No	
Clustered std. errors (state-country pair-level)	Yes		Yes		Yes		Yes	
Number of clusters	4,698		4,698		4,841		4,841	

Figure 1.

The figure depicts the statistics of the number of years state-country destinations pairs observed exports for the OM and the EL data respectively.