(Inter-state) Banking and (Inter-state) Trade: Does Real Integration Follow Financial Integration?*

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Abstract

We examine whether financial sector integration leads to real sector integration through trade. Our conjecture is that banking integration between two regions leads to higher trade flows between them. In our stylized model, this happens because banks with presence in the two regions are better able to assess risks and charge the appropriate premiums for trade-related projects pertinent for the two markets; whereas the same banks charge higher average interest rates for projects that involve trade to other markets from which these banks are absent. We use the deregulation of inter-state banking in the U.S. as a natural experiment to test the implication of our theory model with the state-level Commodity Flow Survey data. Our empirical evidence indicates that there is a trade channel associated with the finance-growth nexus. Based on difference-in-differences estimates, we find that the trade share of state-pairs that have opened their banking market to each other's financial institutions increased by 14% over a ten year period relative to the trade shares of state-pairs that did not. This increase in trade flows is due to actual bank integration following deregulation: based on instrumental variables estimates, we calculate that an increase in bank integration from zero to 2.28%, the mean of the data, increases trade in the range of 15% to 25%. These magnitudes are probably lower bound estimates for financial barriers in international trade, given that the international financial system is much less integrated than the U.S. financial system.

JEL: F10, F15, G21, G28, R12

Keywords: inter-state trade, inter-state banking deregulation, finance-growth nexus

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1. Introduction

A significant body of empirical evidence, accumulated over the past decade, indicates that the development of the financial sector furthers economics growth.[1] More recently, research has focused on the channels through which this observed growth may take place. For example, Black and Strahan (2002) show that intra-state branching and inter-state bank entry deregulations in the U.S. between mid-1970s through mid-1990s had positive and separate impacts on entrepreneurial activity in the form of new business incorporations. Another and more recent literature examines the link between financial sector depth and international trade. For example, Manova (2008a) finds that financial liberalization increases country-level exports more in finance-dependent industries as well as in sectors with fewer tangible assets compared to the other sectors of the economy.

We combine these two strands of literature and study a channel of the finance-growth nexus that has received little attention until recently: the effect of bank-provided finance on trade. Specifically, we examine whether the informational advantage that certain banks possess in resolving information problems has implications for the trade channel. We argue that multi-market banks would make use of the additional information that they gather due to their presence in different economic environments. This comparative advantage in obtaining information, vis-à-vis single-market banks, would then be put to use when evaluating loan applications and setting up (or renewing) lending relationships for projects that involve trade between the markets in which the bank is present. If so, the resulting trade patterns would not be random, but would be instead influenced by the multi-market banks' comparative advantage in information gathering over the regions in which they have already a presence.

This point is formally made in our partial equilibrium model of inter-regional trade.[2] In this stylized model, banks in a given region evaluate loan applications for local manufacturing projects whose target market is another (non-overlapping) region. Our stylized theory model has specific implications for trade shares between regions (the 48 contiguous U.S. states in our empirical set-up) given the level of integration of their banking systems. Importantly, the resulting trading activity is not *ad hoc* but instead shows patterns indicative of multi-region banks' superior ability in capital allocation. Banks with a presence in both the manufacturing and the product-destination regions charge appropriate risk-premiums on loans for approved projects, given the region-specific information that they already possess. The appropriate (*ex ante*) pricing and allocation of loans increases trade as the projects with the higher *ex ante*

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[1] See for example King and Levine (1993a, b), Demirguc-Kunt and Maksimovic (1998), Levine and Zervos (1998), Rajan and Zingales (1998), Beck, Levine and Loayza (2000).

[2] Our theory model can be extended to full-blown general equilibrium framework.

chance of success are provided capital at lower costs when the target market is the one in which the bank has already a presence. If banks have no presence in the target-market, they charge an average risk premium that reflects their expectations of the overall probability of success of the average project targeting the unfamiliar product market. In such a case, projects with a higher chance of success suffer a higher cost of capital. [3] As a consequence of a less efficient capital allocation process, given the banks' lack of information due to their absence from the targeted-market, the trade shares would be lower between regions without integrated banking systems.

We use the 1977 and 1993 Commodity Flow Survey (CFS) data on U.S. inter-state shipments to test the implications of our theory model. The staggered deregulation of the U.S. inter-state banking restrictions serves as a natural experiment that provides variation across states and time that proves useful in identifying the effects of financial integration on trade shares. Between mid-1970s and mid-1990s, various states deregulated their banking markets and opened up to competition from other states' depository financial institutions at different points in time. Many states formed agreements allowing banks from certain states to enter their markets, typically, but not always, on the basis of reciprocity. When we examine trade patterns across state-pairs in the post banking-deregulation period, our results support the implications of our stylized model of trade. We find that for a given state, the inter-state trade share increases more with states with which bank-entry was deregulated at an earlier date than with states with which no such deregulation was undertaken.[4] In other words, state-pairs that allowed their financial institutions entry to each other's banking markets are associated with an increase in trade shares (by 14% in 1993 with respect to 1977) compared to state-pairs that have no such common bank-entry deregulation. Looking at *actual* bank entry data, our preferred estimate leads us to conclude that an increase in banking integration, as measured by the fraction of common banking assets for a state-pair, from zero to 2.28% (the mean of the data), leads to an increase in trade by almost 17% over the same period. This is consistent with the hypothesis that financial institutions entering a new market make use of their informational advantage on the two markets when screening projects that involve trade between the same markets. Our results, which are robust to different specifications and estimators, shed light on a channel of the finance-growth nexus that has received little attention up until now. The magnitudes of the effects that we find are well in line with those predicted by a simple calibration of a standard monopolistic competition trade model and Dixit-Stiglitz love-of-variety preferences, where the marginal costs of production would decrease by 2.5% as a result of bank integration and the markups in the economy would

[3] In an alternative version, banks ration credit to projects on which they do not have a comparative advantage in screening.

[4] An equivalent reinterpretation of our findings is that *relative* trade *flows* from an origin state to deregulating destination states vs. non-deregulating destination states increase. We insist on speaking about trade *shares* to stress this *relative* difference; we do not, and given the survey data that are at our disposal we cannot, investigate how absolute trade volumes from each state increases as a result of bank deregulation.

range between 10% and 20%. Our findings have implications for the integration of the financial sector and international trade: we believe that our study can provide a cautious lower bound estimate of the effects of banking and trade finance barriers on international trade.

The paper is organized as follows. In section 2, we review the strands of literature that are relevant for our hypothesis. Section 3 presents the theoretical model that formalizes our main argument as to why liberalization of banking entry between two states would increase trade flows between them relative to flows between state-pairs for which no such liberalization took place. In section 4 we present our empirical strategy and the data that we use in empirical tests. In section 5 we discuss the results and their robustness. Section 6 concludes.

2. Literature Review

The foundations of the current finance-growth nexus research go back almost a hundred years to Schumpeter (1912) who argued that economies with more effective financial systems grow faster. The culminating evidence over the past 15 years, starting with the new empirical tests of King and Levine (1993a and 1993b), shows that deeper financial systems further economic growth.[5]

More recently, research on the finance-growth nexus has focused its attention on the channels of the financial system's impact on observed growth. For example, Jayaratne and Strahan (1996) examine the impact of U.S. intra-state branching deregulation on the state's real economy and find that per capita income and output grow faster after deregulation due to increased competition among banks. Consistent with this evidence, Rice and Strahan (2010) find that small-business loan terms improve following removal of restrictions on *inter-state* branching, and Black and Strahan (2002) find that the rate of new business formation increases after *intra-state* branching deregulation. Kerr and Nanda (2009) find that inter-state banking deregulation unleashes Schumpeterian forces of creative destruction: bank entry liberalization increases both entry by new firms but also leads to higher level of exit among new entrants. Cetorelli and Strahan (2006) study whether market power of banks has any impact on the real sector by examining the number of firms in an industry as well as the size distribution of firms in that sector. They find that both the number of establishments in a given industry as well as the fraction of small firms in that sector increases with U.S. banking sector deregulation. In a cross-country analysis Cetorelli and Gambera (2001) find that the banking market structure can both stifle overall growth in the economy, while at the same time promoting the growth of finance-dependent industries through financing to younger firms.

[5] See, for example, Demirguc-Kunt and Maksimovic (1998), Levine and Zervos (1998), Rajan and Zingales (1998), Beck, Levine and Loayza (2000).

In this paper we examine whether there is a trade specific component to the finance-growth nexus that would be in line with the informational story of loan provision. In a setting where banks would have more information about the geographic regions in which they are present, we examine whether their expansion into new markets would affect trade growth between geographic regions. If the finance-growth nexus is affected by the banks' ability to resolve information problems in trade-related projects, then for a given region we would expect the integration of its banking sector with that of another region would promote trade between these two regions more than trade with other regions with which no such financial integration took place. This forms our testable hypothesis. The alternative hypothesis is that the finance-growth nexus works solely through the provision of higher amounts of credit, leaving trade shares unaffected. In other words, if banks have no geography-based informational advantage, or if our theory is not economically significant, then trade shares for different geographic areas would not be affected as the overall volume of trade increases when credit becomes more available following bank entry deregulation (as in Jayaratne and Strahan, 1996, for example). Thus under this alternative hypothesis all trade *flows* from a given state would increase by the same proportion on average, leaving the trade *shares* of this state with others unaffected.

To the best of our knowledge, the particular mechanism of the finance-growth nexus proposed here, through which financial integration between particular regions would lead to higher trade flows amongst them, has not been studied before, even though the seeds of our conjecture were sown by Morgan, Rime, and Strahan (2003, 2004; henceforth MRS). These authors, in their study of the impact of U.S. banking entry deregulation on state-level output volatility, indicate that a possible link may exist between inter-banking deregulation and inter-state trade. However, the focus of their paper is clearly on the former rather than the latter: in the published (2004) version of their paper, MRS make only a passing reference to a possible link between banking deregulation and inter-state trade, an argument that is made in somewhat more detail in the draft (2003) version of the same paper. Using CFS data on 50 states and the District of Columbia, MRS (2003) conduct a preliminary examination of unconditional correlations between banking integration and inter-state trade and find no apparent link between the two. While we explore the same potential link between financial integration and trade, a tangential topic in MRS (2003, 2004), there are significant differences between our approach and theirs. First, we examine trade *shares* across all *state-pairs*; whereas MRS (2003) study *aggregate* trade *flows* (exports) that a state has with *all* of the other states. Second, we examine the potential link between *pair-wise* financial integration after inter-state banking deregulation and *pair-wise* inter-state trade; whereas MRS (2003) focus on the possible effect of the *aggregate* financial integration of a particular state with the rest of the Union on that state's *aggregate* exports to the rest of the U.S. As a result of these differences, we are able to exploit the variation that exists in the CFS dataset using 4,512 state-pair observations; whereas MRS (2003) limit themselves to an examination of aggregate trade flows using 51 data points at the state level from the same dataset. Therefore, our tests have higher statistical power than the unconditional correlations that they examine. Third, we use a stylized trade theory that explicitly links multi-state banks' impact on inter-state trade and we are able to obtain a *gravity* equation that incorporates the impact of financial intermediation and that can be estimated with state-pair data; whereas the theory model of MRS (2003, 2004) is focused on state-level banking deregulation and output volatility, with no explicit link for the relation between pair-wise banking-entry deregulation and trade.

Our work is also related to the growing literature on financial sector development and trade as well as the research on financial constraints and 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.

Our paper differs from these papers in two dimensions. First, we focus on the effects of the financial *integration* between regions, as opposed to financial system *depth* in a given country, on trade flows. Second, we conduct our tests with intra-U.S. inter-state trade data, which do not suffer from the complications of country-level international data, such as differences in trade barriers, trade agreements, and legal system origins, to name a few. The challenges of international data are not limited to these. For example, in an international context it is difficult to control for *de jure* and *de facto* differences in banking regulations, or more broadly, financial regulations. Moreover, cross-country comparisons typically require the use of test variables based on broadly aggregated data (like credit to the private sector as a share of the GDP) and the classification of financial dependence measures that may be influenced by spurious factors (such as foreign direct investment) that also affect trade. In this respect, inter-state banking entry deregulation in the U.S. offers a compelling natural experiment to study the impact of financial integration on real sector integration. This is because the 50 states of the union share a common legal background, in which the constitution bans levying tariffs on trade with other states of the Union, and allow the banks to operate in a common federal structure of supervision and regulation.

Our work is also related 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, and 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.

We suggest one particular channel through which such credit constraints may be eased for exporting firms - the role of multi-market banks' informational advantage in evaluating loans for projects that target markets in which the bank is present. This provides the setting for a more specific test than just the banks' role in the provision of financing that eases credit constraints for all exporting firms. Moreover, our tests are conducted with U.S. state-level data, which alleviate some of the problems associated with firm-level data: firms' decision to export require additional modeling of the endogenous self-selection process for which there are few good instruments and which involve many unobservables.

Our work is also related, albeit tangentially so, with the literature on the home bias and border effects in trade that relies on U.S. data. Wolf (2000) uses the publicly available version of the 1993 CFS data and finds that state borders within the U.S. form a trade barrier that generate a bias for trade within the home state. Hillberry and Hummels (2003) use the establishment-level 1997 CFS data (which are not publicly available) and find that the calculated home-bias in inter-state trade observed by Wolf (2000) is significantly lower when (i) wholesalers' shipments are accounted for and (ii) shipment distances are properly measured, dimensions which are not observable in the publicly available version of CFS data. Although we rely on the same dataset, our paper differs from Wolf (2000) and Hillberry and Hummels (2003) in two major ways. First, we do not examine the home-bias in intra-state trade. In fact, we ignore within-state trade in our data as our focus is on financial and real integration across states. Second, we exploit the changes in trade shares across state-pairs and over time (using 1977 and 1993 CFS data) for identification purposes and do not focus on a cross-sectional analysis of the data as Wolf (2000) and Hillberry and Hummels (2003) do with the 1993 and 1997 CFS data, respectively. [6]

[6] Despite these important differences, our results, discussed in detail in section 5 below, may point to another reason why Wolf's (2000) results differ from those of Hillberry and Hummels (2003): inter-state financial integration was greater in 1997 compared to 1993 following the enactment of the Riegle-Neal Inter-state Banking and Branching Efficiency Act (IBBEA) in 1994 which came into effect in 1995. Therefore, our finding that more financial integration between 1977 and 1993 leads to higher inter-state trade flows suggests that the financial barriers to inter-state trade in 1997 were likely to be lower than what they were in 1993. This may explain, at

Next, we propose a theoretical model to formalize our conjecture regarding the link between financial and real integration.

3. A Simple Theoretical Model

In this section we use a standard, stylized monopolistic competition model of trade with heterogenous firms (Melitz, 2003) into which we introduce financial intermediaries that are the sole providers of capital. This set-up allows us to obtain a theoretical gravity equation with specific implications for trade shares across regions given the integration of banking systems across the same regions. We keep the model simple enough to be tractable, yet detailed enough for it to provide us with the gravity equation that we estimate using the U.S. inter-state trade data in Section 5 to test our main hypothesis.

Consider a standard monopolistic competition model of trade in a manufactured (differentiated) good between three states (or regions) *i, j* and *k*. Consumers have Dixit-Stiglitz utility over varieties of a differentiated good, $U(c) = \left(\sum_{n=1}^{N} (c_n)^{\sigma} \right)^{\frac{1}{\sigma}}$. The measure of elasticity of substitution between the said varieties is $\frac{1}{1-\sigma} > 1$. When a variety is offered to the market, there is some probability (unknown *a priori* to a firm, discussed in detail below) that it will be rejected by the consumers and firm's sales are going to be zero. With such a preference structure, a firm located in state *i* faces in any other state *m* (with $m = \{j, k\}$) the firm-level demand c_{im} (i.e., consumption) for its product if the consumers wish to buy:

$$c_{im} = \frac{I_m}{P_m} (p_{im})^{-\frac{1}{1-\sigma}} \tag{1}$$

where p_{im} is the price the firm charges for its variety on market *m*, I_m is the state *m* income, and P_m is the price index (level) of all varieties of the manufactured good sold in state *m*.

Capital is the only factor of production.[7] Firms differ in terms of productivity and each manufactures its own variety of the differentiated good. Upon entry, they need to pay a fixed cost in order to set up a manufacturing facility and conduct R&D to invent a variety of the good in question and then face a constant marginal cost of production. Firms are also cash constrained: both the fixed costs needed to set up production and all variable costs are financed solely through bank loans.

least in part, as to why the results of Wolf (2000) on inter-state trade barriers with 1993 data are stronger than those of Hillberry and Hummels (2003) with 1997 data.

[7] We could introduce more factors of production, such as labor. The model remains solvable, and its qualitative implications unchanged, as long as the production function has constant returns to scale.

After conducting R&D, a firm obtains a technology (and a productivity a that is common knowledge) to produce a variety of the manufactured good that it can sell either to state j (a j-type project), to state k (a k-type project), or both if the bank accepts to further financing.[8] The sales of this variety can be successful (i.e., consumers in state $m = \{j, k\}$ will buy) with independent probabilities. Given the preference structure, constant returns to scale in production, and the assumption of monopolistic competition, each firm will command a constant mark-up over the cost of production and quote a price p_{im}:

$$p_{im}(a) = \frac{\tau_{im}\psi_{im}(a)}{\sigma} \tag{2}$$

where τ_{im} is a measure of the unit (iceberg) trade cost between regions i and m, and ψ_{im} is the marginal cost of production of one unit of the good by the firm located in region i targeting destination m having productivity a.[9] It should be noted that the marginal cost of production will vary across destinations, given that the cost of capital charged by the bank will vary with the risk and costs of information acquisition for the evaluation of the destination-specific project.

We are particularly interested in the expected value of exports from state i to m, which are given by:

$$E\left(N_{im}p_{im}c_{im} \mid \psi_{im} < \tilde{\psi}_{im}\right) = E\left(N_{im}\left(\frac{\psi_{im}(a)}{\sigma}\right)^{-\left(\frac{\sigma}{1-\sigma}\right)} T_{im}\frac{I_m}{P_m} \;\middle|\; \psi_{im}(a) < \tilde{\psi}_{im}(\tilde{a})\right) \tag{3}$$

where N_{im} is the mass of firms located in state i exporting to destination m, c_{im} is the firm-level demand in state m at prices p_{im}, and T_{im} ($= \tau_{im}^{-\sigma/(1-\sigma)}$) is a measure of trade barriers (for example, distance) between states i and m. Only firms that will have a marginal cost (productivity) lower (higher) than some cutoff value for the particular destination $\tilde{\psi}_{im}$ will have loan applications accepted by the bank.

Next, we link the manufacturer's need for financing through loans with the representative bank's informational advantage in evaluating loan requests given its presence (or absence) in different markets that the producer targets. Prior to entry each firm requires borrowing capital from a bank to finance their fixed costs. There are N_i firms that enter.[10] Next, each firm applies for a loan to finance its variable costs

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[8] Without loss of generality, in this partial equilibrium setup we disregard any sales that may occur in the local state i's market.

[9] Given the constant-returns-to-scale production function with capital as the only factor of production, the marginal cost of production ψ_{im} will be equal to the marginal cost of the input (that is, the rate of the bank loan). In other words, ψ_{im} will be equal to R_m/a (to be defined below), the cost of capital specific to the project aiming to export to state m times the unit capital requirement to produce a unit of the good given by the productivity parameter a.

[10] It is difficult with the data we have to provide a measure of banking conditions for entire states and total trade outgoing from a particular state. Hence, we compare the flows between different destinations originating from the same state. For this it suffices to determine the fraction of existing firms that export to each destination.

for each destination separately. If accepted, the bank processes the loan request (at a cost g_l) and then may learn more about the probability of success of the project and quotes the firm an appropriate loan rate.

There are many projects that can be funded by a bank located in region *i* that will involve exports from region *i* to regions *j* or *k*. Suppose that a particular bank in region *i* has a presence in region *j* (through subsidiaries or branches) but not in region *k*. In our set-up the presence (absence) in market *j* (*k*) gives this bank an informational advantage (disadvantage) in evaluating projects that target market *j* (*k*). One can easily think of a simple scenario in which the presence in both regions *i* and *j* gives the bank a better ability to evaluate the future economic conditions in those regions, as opposed to region *k* where it has no presence. This informational advantage would allow the bank to better assess of the potential consumer demand c_{ij} (i.e., expected sales) in region *j* for the producer's variety of the manufactured good at prices p_{ij}. For example, a North Carolina bank with presence in Ohio may be in a better position to evaluate loan requests for manufacturing projects in the former state that target the latter state. Equivalently, the bank's presence in both regions *i* and *j* may help it in assessing the potential success of projects linked with industries to which it has already made loans. For example, a car parts producer in Missouri might be expected to have larger exports to Michigan than to Virginia due to a larger car industry that is located in Michigan. We conjecture that a Missouri bank with presence in Michigan (but no presence in Virginia) is better able to assess the success of such a project than a Missouri bank with a presence in Virginia (but no presence in Michigan). In other words, we assume that the bank is more apt in discerning economic conditions in a particular sector in state *j* where it is already present than in a region *k* from which it is absent. One can say that it is less costly for a bank to verify the state of the world (for example, as in Townsend, 1979) regarding projects that are destined for region *j,* where it has branches or subsidiaries, than those destined for region *k* where it does not have a presence.[11]

We model three channels through which the informational advantage of banks can manifest itself.[12] The first one is what we call the loan-pricing channel, affecting the cost of the loans that projects directed to different states with different bank linkages can have. Suppose that a project is undertaken by a firm located in state *i* and targeting market *m* (with $m = \{j, k\}$ as before). The project will be successful (i.e., sales will take place) with probability q_m or it will fail to deliver any sales with probability $(1 – q_m)$. The said project can be one of two types. Type-1 projects involve some risk $q_{m,1} > 0$ for generating sales in

Therefore, we consider a partial equilibrium setup here and do not solve for the mass of firms entering into production in each state.

11 Alternatively, the bank could be ambiguity averse, as in Klibanoff et al. (2005), and want to grant more loans for projects to destinations where it can assess probabilities of success better.

12 There may be other channels such as credit rationing, skimming of valuable projects (those with higher probability of success) by banks, etc., that we do not model here.

state m while type-2 projects are completely unsuccessful ($q_{m,2}= 0$).[13] A fraction χ of projects targeting market m is of type-1 and a fraction $(1-\chi)$ is of type-2. Neither the entrepreneurs located in i, nor banks that are not operating in the region m can know the type of the project at hand *ex ante*, even though they know *a priori* the fraction of type-1 and type-2 projects (i.e., they know χ). Importantly, in our model a bank located in region i, evaluating the loan application for a project targeting region j knows the distribution of q_j if it already has a presence (either through subsidiaries or branches) in state j. On the other hand, a bank that does not have a presence in state k can only form expectations about the probability of success q_k.

Banks in our model raise deposits for which they are price takers.[14] The representative bank always requires some margin over its cost of funds r, i.e., it requires at least r/η (with $0<\eta<1$) in expectations from a project. In other words, similar to the manufacturers, the banks in our model also operate in a monopolistic competition fashion, i.e., they have some pricing power over their loans.[15] Then, for accepted projects in state i aiming to sell to consumers in state j, the bank learns, thanks to its presence in region j, whether the project is of type-1 or type-2, as well as the related probability of success q_j prior to lending. As a result, the bank with presence in region j charges $R_{j1}=r/(q_1\eta)$ (commensurate with the risk of the type–1 project) but will not lend if the project is of type-2. For projects that target state k on which the bank does not have such information, it will charge the interest rate $R_k = r /[E(q)\eta]$ where $E(q)= \chi_1 q_1$. In effect, the existence of a bank with superior information about j-type projects that would price loans more accurately would lower the (financial) barriers of trading with that state in our model and cause higher flows to occur between states i and j (see below). We call this effect the loan-pricing channel, which would also work if there is be no firm heterogeneity and fixed costs of project processing. We also note that such expertise about target market demand and supply conditions is important in the financing of international trade related projects or capacity expansions.

The second channel may come from the differences in the costs of processing loans for a project going to state m, $g_m>0$, which may be lower for destinations where the bank has branches. Given the expected loan pricing, the bank also has an earlier decision to make whether to accept or reject a project. This creates the third channel, the project selection channel. A bank will accept a project whenever the

[13] This assumption is for the sake of simplicity. A model with $q_{m,1} > q_{m,2} > 0$ gives the same qualitative implications on the difference between trade flows across regions (available upon request).

[14] Here banks are price-takers in their inputs, which are deposits in our model. In reality, banks may have pricing power over certain types of deposits (such as interest paying checking (NOW) accounts and savings accounts), even though they do not have similar pricing power over other types of deposits (such as Certificates of Deposits (CDs) and negotiable- or large-CDs). Since our focus is on loans (banks' output) we abstract from banks' potential price setting power in the deposit market.

[15] An assumption of a perfectly competitive market for loans, in the absence of fixed costs of loan processing, does not alter our model's implications, with the caveat that firm heterogeneity cannot be easily modeled with the current preference structure.

firm in expectations will be able to repay the loan (including the fixed cost of loan application processing). This means that banks are going to accept projects of firms that have high productivity, and hence will enjoy higher expected profits relative to firms with lower productivity. As there is a large mass of firms, N_i, with potential projects that can obtain financing, the bank's decision constitutes effectively an entry condition for firms exporting to a particular state. The bank will in expectations earn zero profits on the firm with the lowest productivity $\tilde{a}_{im}$ which loan is accepted to destination m:

$$E\left((qR_{im} - r)\frac{c_{im}(\tilde{a}_{im})}{\tilde{a}_{im}} \right) = g_m r \tag{4}$$

After substituting for the loan rates and sales of successful firms, the productivity cutoff[16] is $\tilde{a}_{ij} = \left(\frac{\tau_{ij}}{\sigma}\right)^{\frac{1}{\sigma}} \left(\frac{P_j}{I_j}\frac{\eta}{1-\eta}\frac{g_j}{\chi}(R_1)^{\frac{1}{1-\sigma}}\right)^{\frac{1-\sigma}{\sigma}}$ for the projects destined to state j and $\tilde{a}_{ik} = \left(\frac{\tau_{ik}}{\sigma}\right)^{\frac{1}{\sigma}} \left(\frac{P_k}{I_k}\frac{\eta}{1-\eta} g_k R_{ik}^{\frac{1}{1-\sigma}}\right)^{\frac{1-\sigma}{\sigma}}$ for the projects destined to state k.[17] The decrease in expected prices set by firms from state i for products targeting state j's market, or a fall in loan processing costs g, or both, will increase the number of accepted (and hence financed) projects from state i to state j relative to the projects targeting market k.

Following the recent literature on heterogeneous firms in international trade we posit that productivities are drawn from a Pareto distribution with parameter $z > \frac{\sigma}{1-\sigma}$ and the lowest productivity $a=1$. This allows us to obtain a tractable solution for the expected flows between two states although we can obtain the same qualitative conclusions without this specialization of the distribution of productivities.

The expected flows from state i to a destination state m are then the following (where $g(a)$ is the probability density function of the productivity a):

$$EX_{im} = N_i \int_{\tilde{a}_{im}}^{\infty} \left[\sum_k \chi_k q_k p_{im}\left(R_{im}(q_k)\right) c_{im}\left(R_{im}(q_k)\right) \right] g(a)da \tag{5}$$

Substituting for R, p, c we obtain

[16] We implicitly assume in this partial equilibrium setting that both these cutoffs are above the minimum possible productivity $a=1$, *i.e.* in expectations there will be some selection of projects to both destinations.

[17] In our partial equilibrium setup, we assume that the changes in the mass of firms, prices etc. in state i do not to affect the costs, income or the price levels in other states.

$$EX_{ij} = N_i\Gamma_{ij}\left(\chi(R_1)^{-\frac{1}{1-\sigma}}\right)^{\frac{z(1-\sigma)}{\sigma}}\left(g_j\right)^{1-\frac{1-\sigma}{\sigma}z} \tag{6}$$

where $\Gamma_{ij} = \dfrac{z}{z-\dfrac{\sigma}{1-\sigma}}\left(\dfrac{\tau_{ij}}{\sigma}\right)^{1-\frac{z}{\sigma}}\eta^{-\frac{1-\sigma}{\sigma}z}(1-\eta)^{\frac{1-\sigma}{\sigma}z-1}\left(\dfrac{I_j}{P_j}\right)^{z\frac{1-\sigma}{\sigma}}$. The expected flows from state i to state k are

$$EX_{ik} = N_i\Gamma_{ik}R\left(E(q)\right)^{-\frac{z}{\sigma}}\left(g_k\right)^{1-\frac{1-\sigma}{\sigma}z} \tag{7}$$

where Γ_{ik} is defined similar to Γ_{ij} above.

Under the strong assumptions that (i) $\Gamma_{ij}=\Gamma_{ik}=\Gamma$, (ii) $g_j=g_k=0$ and that (iii) all financing comes from banks that are present in region j but absent in region k, we can compare the two trade flow expressions. We find that $EX_{ij} > EX_{ik}$ as $R_l < R$. Therefore, expected flows in terms of value between states i and j would be higher than those between states i and k.[18]

The effects of informational advantage on expected flows may manifest itself not only in the price of the loans (and indirectly through the number of accepted projects due to this reason) but also through the lower costs of loan processing. Assuming that (i) $\Gamma_{ij}=\Gamma_{ik}=\Gamma$, (ii) the banks do not learn about success probabilities of projects even if they are present in the states and that (iii) all financing comes from banks that are present in region j but absent in region k, $EX_{ij} > EX_{ik}$ if $g_k > g_j$.

Recapitulating, the informational advantage of banks may affect flows through lower loan rates (and hence lower costs for served firms and their higher sales) and these together with lower loan processing costs are going to affect the number and marginal costs of projects that are going to be accepted by the bank. The ratio of expected the flows (equivalent to the ratio of expected trade shares of these states in state's i trade) to state j and k (from (6) and (7)) then will be:

$$\frac{EX_{ij}}{EX_{ik}} = \left(\frac{g_j}{g_k}\right)^{1-\frac{1-\sigma}{\sigma}z}\left(\frac{\chi^{(1-\sigma)}R_{ik}}{R_{1ij}}\right)^{\frac{z}{\sigma}} \tag{8}$$

Next, we conduct a simple calibration exercise to assess the impact of the loan-pricing channel on trade shares. This allows us to obtain plausible benchmarks for the results of our empirical analysis.

A conservative calibration exercise

The loan price channel offers a conservative lower bound of the strength of the effects of bank-entry deregulation on trade flows that we should expect. We leave out calibrating the other two effects

[18] In the case when $q_{m,1}>q_{m,2}>0$ so that the state-j projects receive funding also for Type-2 projects, the same conclusion follows from the Jensen's inequality.

(existence of which will further increase the flows between states with more bank integration), as we do not know what are the differences in the costs of loan processing g to different destinations nor the shape parameter z of the Pareto distribution of firm productivities.[19] Hence, we show the results of our calibration of the loan-price channel in Table 1 after setting $g_j = g_k = 0$ and assuming no firm heterogeneity. The rows of Table 1 correspond to different assumptions on the implied constant mark-up $\mu=(1-\sigma)/\sigma$ (and the elasticity of substitution $1/(1-\sigma)$), whereas the columns correspond to different scenarios regarding decreases in costs of lending caused by bank-entry liberalization. The cells of Table 1 show the percentage increases in expected trade flows – and hence trade shares - based on equation (8) under different mark-ups and decreases in costs.

For the calibration, we need estimates of the markups and the cost decreases resulting from bank-entry deregulation. There are many studies that tried to uncover the markups in various industries for the U.S. while estimating the standard monopolistic competition model using different methodologies. For example, Hanson (2005) finds for most industries markups in the range 15.1-25.4%, Head and Ries (2001) find 9.6-14.5%, Lai and Trefler (2002) claim on average 23.2%. The references pointed in these papers show most estimates of markups lie between 10-25% for the manufacturing sector.

There are few estimates of the effects of U.S. banking deregulations on business loan rates. Jayaratne and Strahan (1998) find that *intra-state branching* deregulation leads to a 15 to 33 basis points decrease in business loan rates, but find no impact from *inter-state bank-entry* deregulation. Note that this estimate applies for all transactions in a state and may not capture differences in loan rates for trade-specific projects as in our model. Rice and Strahan (2010) point to a 21 to 88 basis points decrease in small-business loan rates following the relaxing of *inter-state branching* restrictions, some of which were put in place following the passing of the IBBEA as this legislation allowed states to control out-of-state branching. These rate decreases amount to 2.5% to 17.3% drop in small-business loan rates depending on the year and the extent of branching deregulation. As capital is not the only factor of production and assuming, as in factor accounting exercises, that it constitutes only 1/3 of the costs, this implies a fall in the marginal costs anywhere from 0.8% to 5.7%. For these reasons, in Table 1 we present calibration results for different assumptions on the markups in manufacturing (from 10 to 25 percent) and a fall in the total marginal costs due to improved information in the range of 1 to 5 percent.

The potential effects of improved financial conditions even with small changes in loan pricing can be easily of the order of 50%, depending on the level of competition in the industry (as measured by the elasticity of substitution) and the fall in the cost of credit and the ensuing fall in total marginal costs

[19] This calibration exercise does not include other effects that can be potentially at work, such as credit rationing (due to asymmetric information problems) or trade diversion (since bank integration may be considered here as a fall in trade barriers), because we do not include them in our model for tractability.

(which equal to the average variable costs in our model). Hence, the modeled informational advantage that the presence in region *j* confers to the representative loan-granting bank could generate large differences in trade flows (and trade shares) from region *i* to region *j* compared to those from region *i* to *k*.

Empirical models

According to our stylized theory model, controlling for other factors that might affect trade, the integration of the banking sectors across two regions would have a positive impact on the trade between them. In our theoretical set-up, controlling for other factors amounts to presuming that $\Gamma_{ij}=\Gamma_{ik}$, which in its turn implies that regions *j* and *k* are identical in terms of income levels, price indexes of the manufactured good, and their respective trade costs with region *i*. This is clearly a highly improbable set of assumptions. In the empirical specifications detailed below, we control for the differences that these variables exhibit at the state level. Unfortunately, in the data that are available to us, we do not observe the mass of exporting firms, the interest rates charged on loans that they requested, or the costs of processing of their loans. Hence, even though we are able to test for changes in trade shares across regions as our theory model predicts, we are unable to test for the strength of particular channels through which the informational advantage of banks may operate on state-pair trade flows.

We reformulate our model to obtain flows between state-pairs in terms of region *i*'s trade shares with all the other states. This step is needed in order to obtain an equation that can be estimated with the data that are at our disposal. Note that we are not interested in explaining the variation in the total, or aggregate, trade flows out of a given state over time following bank entry deregulation. There are not only more data points available with state-pairs (as opposed to data at the level of the 50 states), but importantly, there is more variation in trade flows and bank-entry deregulation among state-pairs. Our empirical estimation strategy relies on exploiting the variation in bank-entry deregulation across states, prior to the federal deregulation of banking entry, put together with the variation over in the trade data over the only two CFS surveys (in 1977 and 1993) that are available over the same period.

The expected share of exports to destination *m* in total exports of state *i* (including exports to state *m*) is defined as $S_{im} = EX_{im} \Big/ \sum_l EX_{il}$. After taking logarithms and some algebra we obtain:

$$\ln S_{im} = -\Theta_i + \beta_1 \ln\left(I_m\right) - \beta_2 \ln\left(P_m\right) + \Xi_{im} + \beta_3 \ln\left(T_{im}\right) \tag{9}$$

where $\Xi_{im} = \left(\chi(q_1)^{\frac{1}{1-\sigma}}\right)^{\frac{z(1-\sigma)}{\sigma}} (g_m)^{1-\frac{1-\sigma}{\sigma}z}$ if *m* is a state that shares the presence of banks with the exporting region *i*, $\Xi_{im} = \left(E(q)\right)^{\frac{z}{\sigma}} (g_m)^{1-\frac{1-\sigma}{\sigma}z}$ otherwise; and $\Theta_i = -\ln\left(\sum_m N_i \Gamma_{im} \Xi_{im}\right)$ is the state *i* fixed effect. Our variable of interest is always a measure of Ξ_{im}, i.e., the effect of inter-state bank integration. To test our hypothesis about bank integration and trade, we estimate two variants of (9), namely difference-in-differences models and instrumental variable-models, with different specifications and estimators.

Difference-in-Differences Models

In a first step, we assume that the state-level bank-entry deregulations were exogenous to trade flows and specify difference-in-differences models. A number of arguments that can be advanced to support the argument that inter-state trade was not the driver of bank-entry deregulation.

The first set of arguments concerns the way deregulation occurred. There were three principal modes of bank-entry deregulation at the state level. First, numerous states opened up their banking systems non-reciprocally (unconditionally) towards Multi-Bank Holding Companies (MBHCs) from all other states. This means that they did not require the other states to grant the same privilege to their banks. In such a general deregulatory approach trade with specific states could not have played a role in the decision to liberalize entry to all other states' banks. Second, if deregulation were to follow trade flows, then we would have observed states opening up in a non-reciprocal (unconditional) fashion to *selected* trade partners. But only Oregon initially opened up its banking system to some states *non-reciprocally* in 1986 before extending this privilege to banks from all states of the union in 1989. In fact, 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 difference-in-differences 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).

Second, the political economy explanations put forward to explain inter-state banking deregulation do not include trade flows (Kane, 1996, and Kroszner and Strahan, 1999). We would argue that states were likely to deregulate at similar periods under the political pressure of similar constituencies, and not as a result of higher trade flows between them. States in particular regions of the country would have similar political economy drivers that shape the deregulation. These characteristics include the importance of lobbying groups such as small banks that were against deregulation for fear of loss of local market power, insurance industry 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). These characteristics were likely to be shared by states in a given region, such as the Midwest or the Southeast. Kroszner and Strahan (1999) results would suggest that similar constituencies might have been lobbying for protection from out-of-region bank competition in a regional compound, resulting in region-level inter-state deregulation at first. Kane (1996) argues further 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. These regional compounds are also likely to have higher inter-state trade flows, although trade flows did not cause deregulation *per se*. 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. These arguments support our assumption that inter-state banking deregulation process was exogenous to trade flows and shares. This, in turn, allows us to specify difference-in-differences models:

$$\begin{aligned}\ln(TRADE\_SHARE_{imt}) = {} & \alpha_{it} + \beta_1 \ln(GDP\_DEST_{mt}) + \beta_2 \ln(WAGE\_DEST_{mt}) \\ & + \beta_3 D\_1993_t + \beta_4 D\_DEREG_{im} \\ & + \beta_5 \left(D\_1993_t \times D\_DEREG_{im}\right) \\ & + \mathrm{X}\ \ GEOGRAPHIC\_CONTROLS_{im} + \varepsilon_{imt}\end{aligned} \tag{10}$$

where, subscript i denotes the origin-state, m the destination-state, and t indexes time in years; ln(*TRADE_SHARE*), ln(*GDP_DEST*) and ln(*WAGE_DEST*) correspond to ln(*S*), ln(*I*) and ln(*P*) of model (9), respectively; the *GEOGRAPHIC_CONTROLS*, which corresponds to ln(*T*) in (9), includes *ADJACENCY*, *D_BORDER_DEST*, *D_COASTAL_DEST*, ln(*DISTANCE_CAPS*), *D_RIVERS*, *D_SAME_COAST* variables. All of these variables are described in detail below in Section 4 on data. We include *D_DEREG* to capture the potential differences between the deregulating state-pairs (the treatment group) and the non-deregulating state-pairs (the non-treatment group), *D_1993* to capture the changes in the level of trade shares through time since 1977 (it should be remembered that we only have two years of data due to CSF availability in the pre-federal deregulation period). Given the origin-state-and-year fixed effects (α_{it}), the remaining variation that would be due to deregulation (treatment) would be captured by β_5, the coefficient of the interaction of these two indicator variables, which correspond to Ξ_{im} in (9) in difference-in-differences models. We estimate equation (10) using pooled-OLS (with fixed-effects) and *within* (fixed-effects) estimators.

Santos Silva and Tenreyro (2006) observe that log-linear gravity models estimated using OLS are likely to be biased and inconsistent, and propose that Poisson regressions, which do not suffer from these problems, be used instead. We follow their suggestion and estimate the Poisson regression version of the difference-in-differences model (10):

$$
\begin{aligned}
TRADE\_FLOW_{imt} = \exp[\ & \alpha_{it} + \beta_1 \ln(GDP\_DEST_{mt}) + \beta_2 \ln(WAGE\_DEST_{mt}) \\
& + \beta_3 D\_1993_t + \beta_4 D\_DEREG_{im} + \beta_5 (D\_1993_t \times D\_DEREG_{im}) \\
& + \mathrm{X}\ \ GEOGRAPHIC\_CONTROLS_{im}\ ] + \upsilon_{imt}
\end{aligned} \tag{11}
$$

Note that time-varying origin-state fixed effects *(α_{it})* and a year-effect (*D_1993*) mean that the coefficient estimates measure the effect of one unit change in the explanatory variable on trade flows relative to the total origin-state trade flows in 1977 and 1993, separately. In other words, because of *time-varying* origin-state fixed effects, the coefficient estimates on the variables of interest are the same as those that are obtained when trade share is used as a dependent variable.[20]

In alternative specifications of models (10) and (11), we include origin-destination state-pair fixed-effects, i.e. α_{im}. In the presence of origin-destination state-pair fixed-effects the proxies for trade barriers ($GEOGRAPHIC\_CONTROLS_{im}$) are not included, as α_{im} controls not only for all the geographic features concerning the origin-destination state-pair, but also for other unobservable state-pair characteristics that we cannot otherwise account for.

Instrumental Variables (IV) models

While difference-in-differences models look at the impact of inter-state deregulation on trade, they do not allow us to examine the impact of *actual* bank entry. In a second set of regressions we examine the impact of actual banking integration, as measured by the fraction of banking assets that are common for an origin-destination state pair, on trade:

$$
\begin{aligned}
\ln(TRADE\_SHARE_{imt}) = & \ \alpha_{it} + \gamma_1 \ln(GDP\_DEST_{mt}) + \gamma_2 \ln(WAGE\_DEST_{mt}) \\
& + \gamma_3 D\_1993_t + \gamma_4 D\_BANK\_INTEGRATION_{imt} \\
& + \Gamma\ GEOGRAPHIC\_CONTROLS_{im} + \phi_{imt}
\end{aligned} \tag{12}
$$

Here, however, our test variable (*BANK_INTEGRATION*), which corresponds to Ξ_{im} in equation (9), is potentially endogenous to trade. We estimate (12) with IV-regressions where we follow MRS (2003) and instrument the potentially endogenous $BANK\_INTEGRATION_{imt}$ with the number of years since

[20] This argument also applies to pooled-OLS regressions with time-varying origin-state fixed-effects and a year dummy.

deregulation for the origin- and destination-state and a "dummy" variable indicating whether origin- and destination-state deregulated entry as of 1993. Finally, we also estimate the Poisson-IV version of (12) following Santos Silva and Tenreyro (2006):

$$\begin{aligned} TRADE\_FLOW_{imt} = \exp\Big[& \alpha_{it} + \gamma_1 \ln(GDP\_DEST_{mt}) + \gamma_2 \ln(WAGE\_DEST_{mt}) \\ & + \gamma_3 D\_1993_t + \gamma_4 D\_BANK\_INTEGRATION_{imt} \\ & + \Gamma\, GEOGRAPHIC\_CONTROLS_{im} \Big] + \omega_{imt} \end{aligned} \tag{13}$$

The data sources that we use to construct the variables and the proxies in models (10) through (13) are described in the next section.

4. Data

In order to estimate models (10) through (13), so that we can test the hypothesis of a positive link between banking sector integration and trade across regions, we combine inter-state U.S. trade data with inter-state bank entry deregulation and actual bank entry data together with the data for the control variables.

Our dependent variables, the value (in dollars) of trade flows ($TRADE\_FLOW_{imt}$) from state i to state m and the logarithm of the share of trade ($\ln(TRADE\_SHARE_{imt})$) from state i to state m with respect to the sum of all trade originating from state i, are based on shipment data available in the Commodity Flow Survey (CFS). This survey, originally conducted by the Department of Transportation's Bureau of Transportation Statistics, is now jointly administered with the U.S. Census Bureau.[21] CFS, which is typically administered with five-year intervals, is stratified across industries, geography, and firm size. During the inter-state banking deregulation period, however, only two surveys were conducted: one in 1977 and another in 1993. For our purposes it is fortunate that the 1977 precedes the first inter-state banking deregulation by Maine in 1978. The timing of the 1993 survey, a year before the enactment of the IBBEA legislation that deregulated inter-state banking and branching at the federal level as of 1995, is also fortunate: there is no confounding of state-level deregulations up to 1993 that are central for our identification strategy with the federal banking deregulation when IBBEA came into effect in 1995. Moreover, NAFTA, which could impact the surveyed flows, did not come to existence until 1994. The 1977 CFS survey covered approximately 19,500 manufacturing establishments (plants) in the 50 states plus the District of Columbia with one or more employees using a stratification scheme based on the population of such establishments. The 1993 survey, on the other hand, covered approximately 100,000

[21] http://www.bts.gov/programs/commodity_flow_survey/ and http://www.census.gov/svsd/www/cfsmain.html.

mining, manufacturing, wholesale and retail establishments, with a similar stratification scheme. Both CFS surveys exclude establishments involved in crude oil and natural gas extraction, farming and services, as well as direct imports and exports across U.S. borders. Participating establishments reported the value, weight, type, origin, destination and the mode of transportation (land, water, air) of sample shipments over four one-week periods during the calendar year, with one week per each quarter.

The CFS data that we use have four major drawbacks. First, we are dealing with survey data as opposed to actual trade data on inter-state exports, which we cannot observe since the U.S. states do not collect this information. Moreover, survey sampling methods and sizes change over time. As a result we have to resort to trade *shares* as opposed to trade *flows*, the latter being the more commonly used variable in the empirical trade literature. Nevertheless, we also use trade flows as a dependent variable because in empirical models with time-varying origin-state fixed effects, using trade shares or trade flows are equivalent (because the denominator of trade share, total trade flows out of the origin-state becomes a origin-state level fixed-effect for a given year). Second problem that we face is that the publicly available version of the CFS that is available to us aggregates the surveyed shipments data at the state level. As a result our dependent variable is at the state level (and not establishment, firm, county or metropolitan area level, any of which would have allowed us to conduct more precise tests). The third difficulty is that, as indicated above, the number of sampled establishments is more than five times larger in the 1993 survey than the 1977 survey, leading to even larger sampling errors for the latter dataset, especially for the smaller states. These larger sampling errors engender higher standard errors in our regressions, which would render our estimates less precise. As a result, even though we account for the resulting heteroskedasticity, the survey data that we use make it more difficult for us to find any evidence that would support our hypothesis. The fourth drawback is that starting with the 1993 survey CFS includes wholesalers' shipments alongside manufacturers' data (but not in the 1977 survey).[22] Unfortunately, the publicly available version of CFS that we have access to does not disaggregate shipments by the nature of establishments even though it breaks them down by industry segments starting with 1993 (not available in the 1977 survey). As a result, we cannot exclude wholesalers' shipments from the 1993 survey to make them conform to 1977 data. The inclusion of wholesalers' data in 1993 can inflate sampled-trade flows in that year compared to 1977 data, but the trade *shares* that we focus on are less likely to be affected by this

[22] Using establishment-level CFS data (which are not publicly available) Hummels and Hillberry (2003) show that the presence of wholesalers in the data from 1997 biases the estimations of the intra- versus inter-state trade as the wholesalers serve predominantly local (within-state) destinations. They criticize Wolf's (2000) results on the home bias in U.S. trade based on the fact that the latter used the public version of aggregated CFS data that includes wholesalers' shipments. These concerns do not apply to our work since we do not use within-state shipment data in our empirical tests. In our theoretical set-up manufacturing firms and wholesalers face equal financial constraints while exporting from a state.

inclusion. Despite these four drawbacks, we rely on the CFS as it is the only source of data that is available for inter-state trade in the United States.

Before we construct our dependent variables, $TRADE\_FLOW_{imt}$ and ln($TRADE\_SHARE_{imt}$), we exclude Alaska and Hawaii from the CFS dataset because a majority of the flows from these two states to the rest of the U.S. are missing in the dataset. Alaska and Hawaii are also special because they are geographically detached from the other 48 states and the physical trade barriers between them and the rest of the U.S. are considerably larger and harder to account for properly in an empirical set-up. We also drop the District of Columbia because it has very few manufacturing establishments. In other words, we focus on the 48 contiguous states of the Union. As a result, our full-sample includes 2,256 (= 48 × 47) state-pairs on an annual basis, which gives us 4,512 state-pair-year observations for 1977 and 1993 combined.

We combine the CFS data with two types of banking data. First, we collect inter-state banking entry deregulation dates from Amel (2000) to create the effective dates (years) of deregulation. The effective deregulation years are established to make sure that the deregulation by a given state *i* could possibly lead to integration of its banking sector with that of state *j*. For example, if state *i* allows entry by all the other states on a reciprocal basis, entry of state *j*'s banks into state *i*'s banking sector cannot occur unless state *j* allows access of state *i*'s banks into its own banking sector. Using effective deregulation years (the earliest of which are reported in Table 2 for each state)[23], we create a deregulation indicator variable (D_DEREG_{im}) that equals one if banks from at least one of the states *i* or *m* in a state-pair could enter the other state's market as of 1993, and zero otherwise. Second, we use data from bank financial statements (the so-called *Call Reports*), which all U.S. banks have to report to their federal financial regulators, to calculate a measure of banking systems' integration at the state-pair level.[24] For this we collect the total assets of banks and data on bank holding company (BHC) structures. We account for the holding company structure since, prior to the IBBEA that was signed into law in 1994, inter-state bank entry occurred mostly the acquisition of banks in a given state by BHCs from other states. We define the integration variable ($BANK\_INTEGRATION_{imt}$) for a given state pair *i* and *m* to be the ratio of banking assets in state *i* owned by banks in state *m* plus the banking assets in state *m* owned by banks in state *i* divided by the sum of state *i* and *m*'s total banking assets.[25] Given that the bank holding company (BHC) structure was the primary instrument of banking market entry prior to the enactment of the IBBEA in

[23] Note that, for a given state, the effective opening dates to other states' banks vary as states typically deregulated in waves and not at once. See Amel (2000) for details.

[24] The U.S. federal commercial-banking regulators are the Office of the Comptroller of the Currency, the Federal Reserve and the Federal Deposit Insurance Corporation.

[25] Two states' banking systems may also be integrated through tertiary links: ownership of assets by state *n*'s banks in states *i* and *j* would create an additional channel of integration. We leave out such tertiary channels, and as a result our $BANK\_INTEGRATION_{imt}$ variable provides a *lower* estimate of existing bank links, thus making it more difficult for us to detect any effects empirically.

1995, we attribute total assets by the headquarter state of the highest-level institution in the BHC structure if the institution belongs to a multi-bank holding company.

One could argue that banks could follow trade flows and enter the banking markets of states with which their home state has the strongest economic ties. If so, as mentioned above, our banking market integration variable would be endogenous to trade flows and its use would yield biased and inconsistent estimates. We use the instruments suggested by MRS (2003) for $BANK\_INTEGRATION_{imt}$: inter-state banking deregulation indicator variables for the origin and destination states ($D\_DEREG\_ORIG_{it}$, $D\_DEREG\_DEST_{mt}$) and variables that keep track of years since deregulation ($YEARS\_DEREG\_ORIG_{it}$, $YEARS\_DEREG\_DEST_{mt}$). The latter are equal to zero in 1977 since none of the states had deregulated inter-state bank entry by that year. Maine is the first state to do so by 1978.

The theoretical gravity equation (9) requires that we account for destination-state's income and producer-price index, $\ln(I_m)$ and $\ln(P_m)$, respectively. GDP figures for the destination state, GDP_DEST_{mt}, are taken from the U.S. Bureau of Economic Analysis. We expect the coefficient estimate of $\ln(GDP\_DEST_{mt},)$ to be positive and close to 1.0 in line with our theory model and with the estimates of gravity equations in the empirical trade literature. As a proxy for state price levels, which are not available at the state level, we follow the standard practice and use destination-state manufacturing wage-index ($WAGE\_DEST_{mt}$), based on the payroll-hours worked, from the U.S. Commerce Department Annual Survey of Manufactures.[26] In line with the theoretical model, we expect the coefficient estimate for the price-index proxy to be negative (a higher price index at the destination market implies a lower demand).

The gravity equation also requires us to control for trade barriers between origin-destination state-pairs, $\ln(T_{im})$. Even though the U.S. constitution bars states from levying tariffs on interstate trade, differences in state and local taxes and regulations engender barriers to interstate trade. Given the difficulty of obtaining data on trade barriers between U.S. states, and in line with the trade economics literature, we use "great-circle" distance between the capital cities for each state-pair ($\ln(DISTANCE\_CAPS_{im})$).[27] We complement the latter variable with a set of variables that account for the characteristics of the origin-destination state-pair's geography that could affect trade. Trade barriers are likely to be lower between adjacent states. The variable $ADJACENCY_{im}$ is meant to capture the effects of the origin-state having one or more neighboring states on its trade flows toward the destination state. $ADJACENCY_{im}$ is equal to the inverse of the number of states with which the origin-state has a common

[26] Therefore our estimates do not suffer from the critique of Anderson and Van Wincoop (2003) who strongly criticize the lack of control for price indices in many earlier studies employing gravity equations.

[27] We also construct an alternative measure and designate the largest city by population in each of the U.S. states, and then calculate the *great circle* distance between the designated largest city-pairs that correspond to the state-pairs. Our results are not affected by the substitution of one variable for the other.

border *if* the origin-destination state pair has a common border, and 0 otherwise.[28] States that have access to a common waterway are likely to enjoy lower transportation costs. With the indicator variable D_RIVER_{im} we account for the fact that a pair of states might be on the Mississippi or Columbia river systems as well as the Great Lakes shoreline. Through control variables, we also account for whether the destination state is a U.S. border or coastal state for two reasons. First, due to the presence of "border effects" such states may be more remote than others and hence have naturally a lower trade share.[29] Second, even if the survey participants are asked to exclude shipments specifically destined to another country, some of the shipments to the border-states captured by the CFS may in fact be destined for international exports, a factor that could potentially inflate trade shares to states on the U.S. borders. Thus, indicator variable $D\_COASTAL\_DEST_{im}$ is equal to one for states on the Atlantic and Pacific Oceans, and zero otherwise; indicator variable $D\_BORDER\_DEST_{im}$ is equal to one for states on the U.S. national frontier, and zero otherwise. These controls are only used in the time-varying origin-state fixed-effects (α_{it}) models, since origin-destination state-pair fixed-effects (α_{im}) in *within* or IV regressions account for all the geographic characteristics that concern the particular states in a given pair.

The summary statistics for our sample are presented in Table 3. The average trade flow between the exporting-importing state-pairs is 930.85 million dollars, with a standard deviation of 2.19 billion dollars. For 120 state-pair observations in 1977 and 167 in 1993 the observed trade flow, and as a result the trade share, is zero. Of course, this does not mean that there is actually no trade between these state-pairs, but that the CFS sampling scheme either (i) did not detect any shipments (presumably due to actual low trade flows emanating from smaller states), or (ii) the shipments were small and rounded to zero when reported, or (iii) the estimate was not published by CFS administrators because of high survey sampling errors. These 287 state-pair-year observations will drop out of our sample when the logarithm of trade share is used as a dependent variable, a well-known problem in the empirical trade literature. As explained in more detail in Section 5 below, we estimate Poisson regression based models to deal with this potential source of bias. The trade share for the average state-pair is 2.13% (which is an expected statistic (=100%/47) given that each state has 47 partners, after excluding Alaska, Hawaii and District of Columbia from the sample, and a sum of trade shares of 100%), with a standard deviation of 3.53%, a minimum of zero and a maximum of 47.70%. The deregulation indicator variable (to be used in our difference-in-differences estimation) has a mean of 0.7190, suggesting that, as of 1993, 28.10% of state-pairs had not opened their markets to one another (this despite the fact that up to that point all of the 48

[28] Overall empirical estimates and conclusions were not affected by the use of a simple indicator variable that equals 1 if two states in a pair are adjacent, and 0 otherwise.

[29] For example, Redding and Sturm (2008) documented how the imposition of a border in Germany after WW II and the division of the country into two separate states caused industries and factors of production to migrate away from the new border regions.

contiguous states had deregulated bank entry to some extent). The adjacency variable, which is equal to the inverse of the number of neighboring states the state of origin has if a particular state-pair are adjacent and zero otherwise, has a mean of 0.0213 for the full sample. Of our 4,512 state-pair observations one third have a destination-state that is on the U.S. borders ($D\_BORDER\_DEST_{im}$ has a mean of 0.3333), and approximately 44% have a destination-state that is on the Eastern or Western seaboards ($D\_COASTAL\_DEST_{im}$ has a mean of 0.4375). For the 4,512 state-pairs in our sample the mean distance between state capitals ($DISTANCE\_CAPS_{im}$) is 1,677.23 thousand miles, 22.70% share a common river system, and 13.83% are on the same coast.

The summary statistics for $BANK\_INTEGRATION_{imt}$, the endogenous (instrumented) variable in the IV regressions, indicate that on average only 0.26% of state-pairs' banking assets were integrated with the maximum level of integration of 18.45% in 1993 (the state-pair in question is Florida and North Carolina). The low average for $BANK\_INTEGRATION_{imt}$ is due to the fact that many state-pairs in the U.S. had still not formed any actual banking links as of 1993 (only 14.98% of them did) , and fewer still (2.75%) had any links in 1977.[30]

The indicator variables $D\_DEREG\_ORIG_{it}$ and $D\_DEREG\_DEST_{mt}$, which we use as IVs for our endogenous variable *BANK_INTEGRATION*, point that almost two-thirds of states had deregulated their market as of 1993 towards other states (the former variables' mean is 0.6450 in 1993 and 0 in 1977). However, most of this deregulation took place in the latter years in our sample: the mean for the variables tracking the number of years since deregulation ($YEARS\_DEREG\_ORIG_{it}$ and $YEARS\_DEREG\_DEST_{mt}$) is 3.4907 years in 1993 (and 0 in 1977).[31]

As mentioned above, one problem with the CFS data is that they are subject to sampling errors. Another problem is rounding of the reported flows. The smallest unit of measure in the CFS is $1 million, which means that for sampled-shipments that are very small, the errors in the calculated trade shares between 1993 and 1977 can be easily more than 10% for reported flows below $10 million.[32] To check the robustness of our estimation results to sampling and rounding errors that are potentially inherent in the

[30] The non-zero bank links in 1977 are due to the fact that some states had allowed bank entry before the passage of the 1956 Douglas Amendment to the Bank Holding Company Act that forbade any further inter-state banking expansion. The inter-state banking links existing by then were grandfathered by the 1956 Act. $BANK\_DEREG_{im}$ for those pairs would be still zero in 1977, since no inter-state banking deregulation took place before 1978.

[31] The summary statistics for $D\_DEREG\_ORIG_{it}$ and $D\_DEREG\_DEST_{mt}$ on the one hand, and $YEARS\_DEREG\_ORIG_{it}$ and $YEARS\_DEREG\_DEST_{mt}$ on the other, are equal to each other. This is due to the fact that our full sample covers all possible 4,512 origin-destination state-pair combinations over two years. However, for a given state-pair the observations need not be equal to each other for $D\_DEREG\_ORIG_{it}$ and $D\_DEREG\_DEST_{mt}$ or for $YEARS\_DEREG\_ORIG_{it}$ and $YEARS\_DEREG\_DEST_{mt}$: even though summary statistics are equal, the variation across paired-observations are not.

[32] The standard errors on the estimate of *global* (i.e., total at the state-level) shipments reported in the 1977 CFS documentation are especially high for smaller states: for example, the reported standard error is 23% for Vermont, 16% for Maine, and 11% for New Hampshire, whereas it is 5% for New York and 4% for California. The measurement errors in state-pair trade flows that we use here are likely to be even greater.

trade survey data, we also examine a sub-sample 3,512 state-pair-year observations for which trade flows are $10 million or more (in constant 1977 dollars) in 1977 or 1993.[33] Admittedly arbitrary, the threshold of $10 million was chosen so as to minimize the influence of observations with large measurement errors while minimizing the number of observations that would be dropped.[34]

5. Empirical Model Estimates

The estimates of the gravity equations (10) through (13) are presented in tables 4 through 7, respectively. In all of our estimations we use robust standard-errors clustered by the state of origin to account for the heteroskedasticity, with the exception of Poisson-IV models where we have robust errors because we could not account for the clustering of errors by the state of origin.[35]

Before we focus on our test variables, we first check the validity of our gravity equation by examining its estimates and compare them with the findings in the literature. The coefficient estimates for ln(GDP_DEST_{mt}), ln($WAGE\_DEST_{mt}$), and geographical controls in tables 4 through 7 are in line with those found in the empirical trade literature. The logarithm of destination state's GDP has a coefficient estimate that is statistically significant and typically slightly above (below) but close to 1.0 in the OLS (Poisson) regressions presented in tables 4 and 6 (tables 5 and 7). This result is consistent with those of Santos Silva and Tenreyro (2006) that compares OLS and Poisson estimates of gravity equations. The coefficient estimates for ln($WAGE\_DEST_{mt}$), which proxies for manufacturing price index in the destination-state, typically enters with the expected negative sign in the regressions, although it is not statistically significant with the exception of two regressions where it is negative and marginally statistically significant at the 10%-level. The year indicator variable *D_1993* has a negative (positive) and statistically significant coefficient estimate in the OLS (Poisson) regressions. This apparently contradictory result is explained by the fact that we have ln($TRADE\_SHARE_{imt}$) as a dependent variable in the OLS regressions of tables 4 and 6, and $TRADE\_FLOW_{imt}$ in Poisson regressions of tables 5 and 7. Between 1977 and 1993 the average origin-destination trade shares have slightly decreased, possibly due to faster growth in trade shares of states for which the flow was unobserved or zero in 1977. Over the same period the average sampled-trade flows have increased, which is likely to be due to both CFS' larger

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[33] Due to the availability of the CFS data and the $10 million threshold that we impose, the number of trade pairs for exporting states differs in the final sample: for example we have 47×2 observations for Illinois, New Jersey, Pennsylvania and Wisconsin, whereas only 9×2 for Wyoming. This feature of the sub-sample does not affect our model estimates given that we include state fixed-effects and account for heteroskedasticity in the calculation of the standard errors.

[34] In unreported estimations, we also used thresholds of $5 and $20 million (in constant 1977 dollars). These thresholds would limit rounding (reporting) errors to 20% and 5% of the surveyed value, respectively.

[35] We also considered, but decided against, using origin-destination state-pair clustered errors, given that we only have two observations per state-pair in our two period panel.

sampling scheme in 1993 as well as higher trade flows, though we cannot differentiate which effect dominates.

The coefficient estimates for geographic characteristics presented in Table 4, 5, and 7 (state-pair fixed effects in Table 6 precludes geography variables), which proxy for trade barriers, are also in line with those found in the literature. The adjacent states have higher and always statistically significant (at the 1%-level) trade shares in all of our estimates in Table 4, 5 and 7. The log of distance between state capitals has a negative and always statistically significant (at the 1%-level) coefficient estimate in all columns of tables 4 through 7: the higher the distance between two state capitals, the lower the trade shares between the two states concerned. Being linked through a river system (which includes the Great Lakes) has a positive and statistically significant effect on trade in all estimates except for the OLS estimates with the full-sample where the coefficient estimates are positive but not statistically significant. Trade towards destination states that are on the U.S. international borders is higher, but statistically significant only in three out of five columns of Table 4, and with varying signs and no statistical significance in other tables. Trade towards destination states that are on the coasts is lower, but statistical significance is typically lacking except in two columns out of five in Table 4. Being on the same shoreline ($D\_SAME\_COAST_{im}$) does not appear to have any impact on trade. These coefficient estimates for the typical gravity equation terms are in line with those found in the literature. Next we focus on the coefficient estimates of our test variables.

Difference-in-Differences Estimates

Table 4 columns 1 through 3 present the estimates of the empirical equation (10) using the "full" sample of 4,225 observations after dropping 287 observations with zero trade shares, columns 4 and 5 using the sub-sample of 3,512 observations with trades of $10 million of more. Empirical models of columns 1, 2 and 4 include origin-state-and-year fixed-effects (α_{it}), and those of columns 3 and 5 origin-destination state-pair fixed effects (α_{im}). All models are estimated with pooled-OLS with indicator variables for various fixed effects, except columns 3 and 5 that are estimated using a *within* estimator.

For the testing of our hypothesis, the coefficient estimate of interest is β_5 for the interacted-indicator variables $D\_1993_t \times D\_DEREG_{im}$, which captures the treatment effect of the banking deregulation. In Table 4 column 2 the coefficient estimate for the interacted-indicator variables is 0.0752 (statistically significant at the 10% level). This means that for a state-pair that has deregulated entry to each other's banks before or on 1993, their trade share was greater by 7.52% on average going from 1977 to 1993 compared to the trade-share of a state-pair where no such deregulation occurred. In column 3, we include origin-destination state-pair fixed-effects, α_{im}, which controls not only for state-pair's geographic characteristics, but also for other unobservables that we could not account for with our proxies. The coefficient estimate for $D\_1993_t \times D\_DEREG_{im}$ is 0.0598 but not statistically significant at the conventional levels (it has a p-value of 0.165). The lack of statistical significance with our preferred estimator is potentially disappointing. One possible explanation for the observed discrepancy in the difference-in-differences coefficient estimates between columns 2 and 3 (both with the "full" sample) may be due to the survey sampling errors that are, as noted above, larger for small states because a fewer establishments has been surveyed among the more limited set populating a smaller state. To check for this possibility we re-estimate empirical models of columns 2 and 3 using the sub-sample of trades of $10 million or more and report the results in columns 4 and 5 of Table 4. Irrespective of the fixed-effects specification chosen (i.e., whether origin-state-and-year fixed-effects or origin-destination state-pair fixed-effects), β_5 for the interacted indicator variables ($D\_1993_t \times D\_DEREG_{im}$) is estimated to be 0.092, which is statistically significant at the 1%-level. In other words, we find that, compared to state-pairs that did not deregulate, out-of-state bank-entry deregulation leads to a statistically significant 9.2% increase in trade shares by 1993. Importantly, with origin-destination state-pair fixed-effects the β_5 estimate is statistically significant. When compared with the results of columns 2 and 3, in which the estimates were based on the "full" sample (after dropping out zero trade share observations), estimates in columns 4 through 5 suggest that survey sampling errors do have an impact on model estimates, at least with the OLS estimates.

Another possible explanation for the discrepancy in the estimates for β_5 in Table 4 may be the choice of the log-linear gravity equation. Santos Silva and Tenreyro (2006) point out that OLS estimates of log-linear models would lead to biased estimates because of the Jensen's inequality: the expected value of a logged variable is not equal to the logarithm of the expected value. This engenders heteroskedasticity that is exacerbated in our case given that CFS sampling design results in higher sampling errors for smaller states. Further, the log-linear gravity model rules out the 287 observations for which the trade share is zero.[36] One argument is that these observations are subject to even larger sampling errors than those with less than a given, albeit arbitrary, threshold such as $10 million. However, a valid counter-argument would be that zero trade share observations nevertheless carry information without which the estimation results would be biased or worse inconsistent. To deal with the problem of zero trade flows, and hence zero trade shares, we follow Santos Silva and Tenreyro (2006) and estimate equation (11) using pooled-Poisson and *within*-Poisson regressions and present the results in Table 5.

[36] Alternative solutions for dealing with zeros in a log-linear model, all of which are arbitrary, include adding 1 million dollars to all $TRADE\_FLOW_{imt}$ observations before calculating trade shares, inserting a very small positive but non-zero number (say, 1 dollar) to trade flows before calculating any trade share, or estimating a Tobit model. We rule out these solutions, as they generate biased and potentially inconsistent estimates (Santos Silva and Tenreyro, 2006).

The first three columns of Table 5 present Poisson regression estimates with the full sample of 4,512 observations (that include zero trade flows), the last two columns repeat the exercise with the sub-sample 3,512 observations with trade sizes of \$10 million or more. The coefficient estimate of β_5 for the interacted indicator variables ($D\_1993_t \times D\_DEREG_{im}$) in column 2 (with origin-state-and-year fixed effects) suggests a statistically significant 19.86% increase in trade shares by 1993 for state-pairs that deregulate bank entry to each other's institutions as opposed to state-pair that do not deregulate such entry. This coefficient estimate is more than twice the size of the similar coefficient estimate obtained using pooled-OLS fixed-effects estimator in Table 4. However, Table 5 results also suggest that it is important to control for origin-destination state-pair effects. The estimate of β_5 in column 3, with origin-destination state-pair fixed-effects (α_{im}) with a *within*-Poisson estimator, is 0.1434.[37] This estimate suggests that it is important to control for unobservables involving the destination state. Otherwise, the difference-in-differences coefficient estimate may pick-up some of the variation that is due to unobservables that are left unaccounted for.

In the last two columns of Table 5, we check whether CFS sampling errors affect these Poisson estimates using the sub-sample of 3,512 observations with trades larger than or equal to \$10 million. The estimate of β_5 is 0.1865 in column 5 with the model including α_{it} and 0.1414 with the model including α_{im}. These coefficient estimates are very close to those in columns 2 and 3, respectively, whereas this was not the case in Table 4 with OLS regressions. This is likely to be due to the fact that the Poisson maximum likelihood estimation procedure gives equal weights to all observations given the first order condition on which it is based (see for example, Santos Silva and Tenreyro, 2006), whereas the OLS, which minimizes the sum of the squared-errors, gives more weight to outliers, which, in our case, also contain the observations with the higher sampling errors.

Based on the *within*-Poisson state-pair fixed-effect version of the difference-in-differences model using the full sample of observations, we conclude that there is on average a 14.34% increase in trade over the decade that ends in 1993 for state-pairs that deregulated entry to each other's banks. However, following deregulation bank entry differs across state pairs, which suggests that entry's impact on trade may differ based on the level of actual financial integration, a question that we examine next.

Instrumental Variables Estimates

To examine the effects of actual bank entry on trade, we estimate equations (12) and (13). Our test variable is $BANK\_INTEGRATION_{imt}$, which is potentially endogenous if, following deregulation,

[37] Note that the number of observations used in the estimation of the *within*-Poisson model with state-pair fixed-effects is 4,450 instead of 4,512, the number of observations in the full sample. This is due to the fact that the *within*-Poisson estimator drops 62 state-pair observations for which the dependent variable, *TRADE_FLOW*, is zero *both* in 1977 and 1993.

banks chose to enter states with which their home state's economy is integrated. We follow MRS (2003) and use as instruments two indicator variables that keep track of whether origin- and destination-states have experienced liberalization, and two other variables that measure the number of years since banking deregulation occurred for the same states. The estimates of the log-linear model (12) with state-pair fixed-effects are presented in Table 6. In the following table, we provide the estimates of the Poisson model (13) with origin-state-and-year effects (the reasons for using α_{it} instead of α_{im} with the Poisson estimator is explained below).

In Table 6 column 1, we present the estimates of the base-case *within* state-pair fixed-effects log-linear model without the endogenous test variable. In column 2, we present the *within* (fixed-effects) log-linear model estimates with $BANK\_INTEGRATION_{imt}$ but without IV. Our focus is on column 3, where we present the IV estimates for the log-linear model.

In the uninstrumented regression presented in Table 6 column 2, $BANK\_INTEGRATION_{imt}$ has a coefficient estimate of 0.5469, which is not statistically significant. However, as explained above, the fixed-effects estimates presented in column 2 are potentially biased and inconsistent if $BANK\_INTEGRATION_{imt}$ is endogenous to trade. In the IV-GMM2S regression presented in column 3, the coefficient estimate for the instrumented $BANK\_INTEGRATION_{imt}$ is 7.3626 (statistically significant at the 1% level). This indicates that for the state-pair experiencing one standard deviation increase in the banking integration variable (an increase of 0.0118, or 1.18%), the trade share increases by 0.0869, that is by 8.69%. Observing that the average integration in the full sample reported in Table 3 was low (0.0019), we consider this to be a high estimate of the impact of banking integration on real integration through trade. Given the preponderance of zeros in $BANK\_INTEGRATION_{imt}$ one standard deviation increase may not be the most appropriate statistic. Another way to interpret this result is to use the move from zero to the mean of the explanatory variable. In 1993, the average bank integration for states that had established bank links was 0.0228, or 2.28% of their combined banking assets. The coefficient estimate of 7.3626 would suggest that if for a state-pair, whose banks were not integrated before, the bank integration would suddenly increase to this mean value (say, due to a merger or acquisition between two large depository institutions), the trade between these two states would increase by 0.1679, that is, by 16.79%. This result is our preferred one as it is based on state-pair fixed effects and the full sample. It is well within the range of values in obtained from our calibration exercise and presented in Table 1. More than 13 times higher coefficient estimate for $BANK\_INTEGRATION_{imt}$ in column 3 compared to the one observed in column 2 suggests that the uninstrumented *within* estimator results are likely to suffer from endogeneity bias and are potentially inconsistent.

To check the validity of our instruments we conduct a series of identification tests, which are presented at the bottom of column 3 of Table 6. 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 5% or 10% of the OLS bias. The Staiger-Stock (1997) rule-of-thumb indicates that our results do not suffer from weak-instruments bias: the partial F-statistic of 14.20 from the first-stage regression is higher than the rule-of-thumb-threshold of 10, indicating that the maximal bias arising from finite sampling inherent in the IV estimates in column 3 of Table 6 is less than 10% of the OLS bias in column 2. Indeed, the more formal *weak identification* test with the same test statistic rejects the null hypothesis of weak instruments with a Stock and Yogo (2005) critical value that corresponds to a 10% maximal IV relative bias. This critical threshold value suggests that the finite-sample bias of IV estimates is less than 10% of the OLS bias, that is, low. 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 Hansen-J statistic reported at the bottom of column 3 of Table 6 indicates that we 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.

Next, we check to what extent these results may be affected by the CFS sampling-error problems mentioned above. In Table 6, column 4, we re-estimate the log-linear model of column 3 using the sub-sample 3,512 observations with a minimum trade size of $10 million. The results indicate that, as in the case of difference-in-differences estimates, log-linear model estimates are affected by sampling error. The coefficient estimate for $BANK\_INTEGRATION_{imt}$ is 11.0970, indicating that a one standard deviation increase in bank integration between the origin-destination states would increase their trade by 13.09% (=11.0970×0.0118). Alternatively, for a state-pair whose banks were not integrated before, if the bank integration were to increase to its mean value in 1993, i.e. 2.28%, the trade between these two states would increase by 25.03%. One more time, the results of the identification tests indicate that our endogenous variable is properly instrumented: in column 4 we reject the *under-identification* and *weak-identification* tests as we should (and at the same significance level as in column 3), and we cannot reject the *over-identification* test. And the results with this sub-sample are still in line with our calibration exercise. It seems that CFS sampling errors lead, as in the case of difference-in-differences estimates, to underestimation of the impact of bank integration on trade.

Next, we check whether the use of the log-linear IV-model, and hence the exclusion of zero-trade observations, may be affecting our estimates. As in the case of the difference-in-differences case, we estimate Poisson models with IV using the full sample of 4,512 observations, however with two caveats. The first caveat is that we could only estimate equation (13) with time-varying origin-state fixed-effects, but we could not estimate it with origin-destination state-pair fixed-effects. The reason is that, to the best of our knowledge, a "*within*"-Poisson-IV estimator does not exist. This by itself is not a concern, given that in the specific case of Poisson regression, estimating a pooled-Poisson regression with many indicator variables posing as fixed effects is equivalent to estimating a *within* Poisson regression with the corresponding fixed-effects (see for example, Cameron and Trivedi, 1998, pp. 280-282). The problem that we face is that the maximum likelihood Poisson estimator fails to converge when we introduce a large number of indicator variables (2,256 to be exact) in equation (12) in the form of α_{im}. As a result, our Poisson-IV estimates in Table 7, whose columns otherwise follow the outlay of Table 6's columns, are limited to origin-state-and-year fixed-effects. The second caveat is that for Poisson-IV estimates we could not conduct identification tests, since we could find no references for such tests in the Poisson estimator setting.

With these caveats in mind, we observe in column 3 of Table 7 that the coefficient estimate for $BANK\_INTEGRATION_{imt}$ is equal to 6.5961 and statistically significant at the 1%-level. This suggests that if a state-pair that was not financially integrated would see its $BANK\_INTEGRATION_{imt}$ rise to the mean of the data (0.0228), the origin-destination trade share would increase by 15.04% (=6.5961×0.0228), an economically significant increase that would take place presumably over a number of years. We also check, as we did in the case of difference-in-differences estimates, to what extent Poisson-IV regressions are affected by the presence of sampling errors in the CFS data. In column 4 we re-estimate the model of column 3 with the 3,512 observations for which trade size is at least $10 million. The coefficient estimate of $BANK\_INTEGRATION_{imt}$ is 6.8093, a value that is very close to that obtained using the full sample of 4,512 observations. Similar to results obtained in Table 5, Poisson-IV results in Table 7 do not appear to be affected by the sampling error problems given the specific nature of the Poisson estimator that gives equal weights to all observations. Our IV estimates suggest that, using information on actual bank entry, moving from zero to sample-mean banking integration (0.0228) would increase trade in a state pair in the range of 15% to 25%.

Robustness Checks

To check the robustness of our results, we conduct two sets of estimations to respond to potential concerns about endogeneity of bank-entry deregulation to trade. First, we re-estimate our main difference-in-differences and IV-models using a sub-sample of state-pairs for which the trade share was less than 5%

as of 1977. If bank deregulation were endogenous to trade as one may fear, one would expect that the effect would be most pronounced for state pairs with the highest level of trade shares prior to the start of the deregulatory process in 1978. By excluding state-pairs with the highest level of trade-shares as of 1977, i.e., by removing data points where the potential concern of endogeneity would be the most valid, our first robustness exercise aims to check whether we obtain similar results to those of the full sample. Because the resulting sub-sample would more prominently consist of small trade shares, which are also more likely to incorporate sampling related errors, we restrict ourselves to trade flows of 10 million or more (in constant 1977-dollars). This helps us avoid the possibility that sampling error related issues mentioned above drive these robustness-checks. After removing 510 state-pair observations for which the 1977 trade share was higher than 5% and 996 observations for which the trade flows were less than 10 million (in 1977 dollars), we are left with 3,006 state-pair-and-year observations (almost two-thirds of our initial sample) with which replicate our main models. The results are presented in Table 8 where the first two columns present the state-pair fixed-effect difference-in-differences model using the *within* OLS and Poisson estimators, respectively. As in the previous regressions, the coefficient estimates for ln(GDP_DEST_{mt}) are statistically significant and close to 1.0, and the ones for ln($WAGE\_DEST_{mt}$) are negative but not statistically significant, comforting us regarding the validity of our gravity equation estimates. The coefficient estimate for our interacted test variables, $D\_1993_t \times D\_DEREG_{im}$, is equal to 0.1043 in the case of the log-linear model and 0.1920 in the case of the non-linear Poisson model, both of these estimates are statistically significant at the 1%-level. Comparable coefficient estimates in our previous regressions were equal to 0.0924 (Table 4, column 5) for the log-linear model, 0.1414 (Table 5, column 5) for the Poisson model, with both estimates being statistically significant at the 1%-level. It is not surprising that we obtain higher estimates here than previously, since by limiting our first robustness check sub-sample to trade shares less than 5% as of 1977, we have, in effect, only kept those state-pairs that could benefit the most from the inter-bank entry deregulation.

Next we compare IV regression results that we obtain within the frame of our first robustness check with those that we obtained in Tables 6 and 7. Specifically, IV-GMM-2S results with state-pair fixed effects in column 3 of Table 8 indicate that the coefficient estimate for the instrumented $BANK\_INTEGRATION_{imt}$ is equal to 22.3229 (statistically significant at the 1%-level), which would suggest that for a state-pair whose banking markets were not linked in 1977, if the bank integration were to increase suddenly to 2.28% - the mean value for integration in 1993 - trade between these two states would increase by 50.90%. However, this result is probably biased. The coefficient estimate of 22.3229 is twice as large as the comparable coefficient estimate in column 4 of Table 6, and based on weak-identification tests (not reported to conserve space) the former is also potentially twice as biased as the latter (which, in its turn has, 10% of the OLS bias). Although other identification-diagnostics come out clean[38], we nevertheless cannot have much reliance on this particular robustness test. Reassuringly, we do not appear to face the same problem with estimates obtained from the Poisson-IV regressions. In column 4 of Table 8, the coefficient estimate for $BANK\_INTEGRATION_{imt}$ is equal to 7.4196 (statistically significant at the 1%-level), a slightly higher number than the comparable estimate of 6.8093 (statistically significant at the same level) in column 4 of Table 7. If the bank integration between two previously non-integrated states were to increase to 2.28%, trade between these two states would increase by 16.92% in the period 1977-1993, compared to a 15.53% increase in trade if one were to use the coefficient estimate of 6.8093 of column 4 in Table 7. In unreported regressions, we estimated the models of Table 8 using state pairs with trade shares less than 10%, 4%, and 3% (with a state-pair trade size restriction of 10 million in 1977-dollars always in place), and obtained similar results, with the caveat that lower the trade share threshold worse our IV estimates became. Thresholds lower than 3% of trade share yield statistically insignificant results for most coefficient estimates, probably because with such a restriction we throw out most of the variation in the data.

As a second robustness check against the potential endogeneity of bank-entry deregulation to trade and any impact that this may have on our estimates, we examine an event that is truly exogenous to the individual states' decision making: the Interbank Banking and Branching Efficiency Act (IBBEA), also known as the Riegle-Neal Act, that was signed into legislation in 1994 and became effective in 1995. This act deregulated bank entry at the federal level for those states that had not opened their market to *all* other states' banks. Prior to the enactment of IBBEA all states had opened up their banking markets to at least some other states' banks. But the reciprocity requirements imposed by a non-negligible number of states made it so that 500 (out of 2,256) state-pairs had not deregulated bank entry as of 1994.[39] Importantly for us, for these 500 state pairs the decision to deregulate was dictated from Washington, DC, and could not be possibly driven by any trade links these may or may not have had with the other state in the pair. Luckily for us, a new CFS was conducted in 1997, three (two) years after IBBEA became law (effective). We create a new database by combining the 1993 CFS data that we used until this point, with the 1997 survey. The short period between these two surveys works against us finding any effect (though many banks used quickly the opportunity for mergers and acquisitions in liberalizing state-pairs), as does the introduction of NAFTA in 1994 that may have changed the geography of interstate trade flows.

In Table 9, we present a series difference-in-differences models that we obtain using OLS and Poisson estimators with the 1993-1997 data. Even though the sampling issues were better handled

[38] The null hypothesis of under-identification is rejected at the 1%-level (with an under-identification test statistic of 17.29), whereas the joint-null hypothesis of valid instruments and correct exclusion restrictions for the instruments cannot be rejected (Hansen-J statistic equal to 1.34 with a p-value of 0.7186).

[39] It should be remembered that in our setting deregulation takes place for a state pair once the two states' reciprocity requirements match if both states impose such requirements. Bank entry becomes also deregulated if one or both states open their banking markets on a non-reciprocity basis.

(compared to the 1977 survey) and more homogenously treated in the 1993 and 1997 CFS surveys, we nevertheless restrict ourselves to the sub-sample with trade sizes of 10 million or more (in 1977-dollars to be consistent with the previous estimates).[40] Very importantly, even though we will be comparing coefficient estimates between Table 9 on the one side and Tables 4 and 5 on the other, the bases of comparison for difference-in-differences models are very different. In the previous difference-in-differences estimations (i.e., Tables 4 and 5), we measured the effect of the *treatment* (the opening of the banking market) on trade shares, by comparing the state pairs that opened their respective markets to each others' banks as of 1993 (i.e., the *treated*) to those that did not as of the same date (the control group). In our second robustness check, we are comparing those state pairs that were forced to open their respective banking markets through federal deregulation (the *new* treatment group) to those state pairs that had already opened up bank entry prior to 1994. This second set of robustness-check estimates should give us an idea as to how do the *treated* in this *new* round fare in terms of trade shares *relative* to the *already treated* earlier. Another important point regarding Table 9 is that only state-of-origin-and-year fixed-effect models are presented. This is because in our attempts to estimate origin- and destination-state fixed effects models or state-pair fixed-effects models we obtained for ln(GDP_DEST_{mt}) and ln($WAGE\_DEST_{mt}$) unreasonable estimates. These models were discarded, as they could not be justified as proper empirical gravity models.[41]

The log-linear difference-in-differences model estimates are presented in column 1 of Table 9. The coefficient estimate of interest for the interacted indicator variables, $D\_1997_t \times D\_FEDERAL\_DEREG_{im}$, is equal to 0.0404 (statistically significant at the 10%-level), suggesting that the trade shares for the state-pairs that were forced open up their markets to each others' banks has increased by 4.04% over the two years that followed the coming into effect of IBBEA. However, in the same column the coefficient estimate for $D\_FEDERAL\_DEREG_{im}$ is equal to -0.0849 (statistically significant at the 10%-level): in 1993 the state-pairs for which the federal deregulation would lead to an opening of their market to their respective banks had trade shares that were lower by -8.49% compared to other state-pairs that had deregulated bank-entry earlier. In other words, even though trade shares have increased post-IBBEA for the 500 affected state-pairs on average, the improvement over the short period of time post-Riegle-Neal was not enough to catch up with the group of state pairs that had opened their banking markets earlier. In terms of trade shares, the 500 state pairs affected by IBBEA for

[40] We obtained very similar results after estimating the empirical models in Table 9 with (i) the full sample, or (ii) the sub-sample with trade sizes equal to or higher than 10 million in constant 1993 dollars.

[41] One possible explanation as to why state-pair fixed effects or origin- *and* destination-state fixed effects models fail may be due to much lower amount variation in trade shares per state-pair in the 1993-1997 data compared to 1977-1993 data. In fact, the standard deviation of the first-differenced trade-shares over 1993-1997 is half as large as that of 1977-1993. This could result in most of the variation in the dependent variable being "soaked up" by the state-pair or individual origin- and destination state pair effects.

out-of-state bank entry still had *lower* trade shares than other state pairs by 4.45% in 1997. These coefficient estimates are comparable in terms of their general magnitude (and taking into account that the bank liberalization was at work for 2 years) to those presented in column 4 of Table 4, where the coefficient estimate for $D\_1993_t \times D\_DEREG_{im}$ is equal to 0.0921 (statistically significant at the 1%-level) and that for D_DEREG_{im} is equal to 0.0430 (*not* statistically significant). The latter estimate and its lack of statistical significance indicates that, as of 1977, there was no difference between the trade shares of state pairs that eventually and by their own actions liberalized bank market entry and those that did not.

In column 2 of Table 9, we present the difference-in-differences model obtained with the Poisson estimator. The coefficient estimate of interest for the interacted indicator variables, $D\_1997_t{\times}D\_FEDERAL\_DEREG_{im}$, is equal to 0.1079 (statistically significant at the 1%-level), and that for $D\_FEDERAL\_DEREG_{im}$ is equal to -0.1942 (statistically significant at the 1%-level). We are essentially interested in the *similarity* of these estimates with those presented in column 4 of Table 5. An increase in trade of 0.1079 over a two-year period strikes us as large, but the origin-state-and-year fixed-effect Poisson-model provided higher difference-in-differences estimate before as well: in Table 5, the coefficient estimate for $D\_1993_t{\times}D\_DEREG_{im}$ is equal to 0.1865 in column 4 (origin-state-and-year effects Poisson-regression model) as opposed to 0.1414 (state-pair fixed-effects Poisson-regression model).[42] As in the case of the log-linear model, the estimates from the Poisson model confirm the pattern observed in column 1 of Table 9: even if by 1997 the trade shares increase by 10.79% for the *newly* treated group, subject to federal deregulation, the same group was still *behind* the *previously* treated group by 8.63%.

Ideally, we would like to examine the effect of actual bank entry on trade post-IBBEA. Unfortunately, we cannot estimate IV-regressions with the 1993-1997 dataset since in this case the federal legislation on bank-entry deregulation applies to all of the 500 state pairs at the same time. There is no instrument that we can come up with that could possibly provide across state-pair variation and serve as a legitimate instrument for $BANK\_INTEGRATION_{imt}$, the endogenous variable. Instruments similar to those we used for the 1977-1993 such as the number of years deregulation, would simply fail us since they would have the same value for each of the concerned state pairs.

Our second set of robustness checks suggest that when we examine the effect of an incontestably exogenous change in the legislation imposed by the federal legislature in Washington, DC, over a different time period, we obtain results that are in essence very similar to those we obtained before. The observed patterns point out that bank liberalization has level effects on trade shares (and flows) that need

[42] In fact, in the *within*-Poisson state-pair fixed-effects results that we chose not to report because the coefficient estimates for $\ln(GDP\_DEST_{mt})$ and $\ln(WAGE\_DEST_{mt})$ made little economic sense, the coefficient estimate for $D\_1997_t{\times}D\_FEDERAL\_DEREG_{im}$ is equal to 0.0458 (statistically significant at the 10%-level).

not be persistent: state-pairs that liberalized later started to catch up with regards to these pairs that liberalized earlier though due to the short period after the IBBEA when we observe the flows in 1997 the convergence may not have yet occurred fully. Combined with the results of the first set of robustness checks, these additional tests give us further confidence in our empirical findings.

6. Conclusion

We estimate the size of financial barriers to inter-state trade stemming from lack of free inter-state banking in the U.S. states prior to 1995. Our results suggest that the barriers to trade stemming from the banking channel can be in fact economically important, even within such a homogenous economic area as the 48 contiguous states of the Union. Using different estimation methods, we find that the removal of banking barriers to trade increased trade volumes by 14.1% between state pairs undergoing banking liberalization in the period 1977-1993 relative to state-pairs that did not give such access to its banking markets. Basing on actual bank entry we find that moving from zero to sample-mean banking integration (0.0228) would increase trade in a state pair in the range of 15% to 25%. These estimates fall within the range predicted by a simple calibration of a standard model of an economy with monopolistic competition and Dixit-Stiglitz love-of-variety preferences if the marginal costs of production would decrease by 2.5% as a result of bank integration and the markups in the economy being between 10% and 20%. The results are robust to problems posed by sampling errors, zero trade flows or shares, or estimation methods, and do not appear to be driven by the potential problem of endogeneity.

Note that we estimated the impact of banking deregulation on trade shares between states. Our results say nothing about the potential increase in aggregate trade flows from a particular state as a result of increase in credit (i.e., capital) availability following bank entry deregulation and the corresponding, firm entry, growth of that state's GDP, etc. What we observe is probably the lower bound estimate for trade barriers coming from the lack of a unified banking system that occur in international trade.

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Table 1. Calibration Exercise

This table presents the results of the calibration exercise that uses the loan-pricing channel. The exercise assumes that there is no firm heterogeneity, i.e., the assumption is that $g_j = g_k = 0$. The table presents the percentage increase in trade flows between states (i and j) that enjoy a bank link in comparison to a pair where no link is present (i and k) for different levels of decreases in marginal costs (across the columns) and for given levels of markups (across the rows).

Assumption: $g_j = g_k = 0$, no firm heterogeneity (loan-pricing channel only)		Fall in Marginal Costs		
		1%	2.5%	5%
Markup	10%	10.5	28.8	67.0
	15%	6.9	18.4	40.7
	20%	5.1	13.5	29.2
	25%	4.1	10.6	22.7

Table 2. Inter-State Banking Deregulation Dates

This table presents the earliest date at which a state agreed to allow acquisitions or presence of Multi-Bank Holding Companies (MBHCs) from at least one other state since the passage of 1956 Bank Holding Company Act that had ruled all such entry. We focus on the 48 contiguous states of the U.S.: the states of Alaska and Hawaii are excluded from our analysis due to their isolated geographical locations. Note that a limited number MBHC ties established prior to the BHC Act of 1956 were grandfathered with the 1956 Act: in other words few state pairs had already established banking sector integration prior to 1977. The column "start of deregulation" gives the earliest year in which the state agreed to open the banking system. Typically, such legislation by state i required reciprocity, i.e., that a counter-party state j also allow entry to state i's banks. As a result, the effective dates of opening within each state pair differ substantially and cannot be compactly shown. Given that in all cases, except three, the reciprocity requirement was met by at least one state, the start of deregulation date also corresponds to the effective opening of the state to interstate banking after the 1956 Act. The exceptions are Kentucky (that legislated interstate banking deregulation in 1984, with reciprocity requirements therein being met by at least one state in 1985), Maine (that legislated interstate banking deregulation in 1978, with reciprocity requirements therein being met by at least one state in 1982), and Minnesota (that legislated interstate banking deregulation in 1986, with reciprocity requirements therein being met by at least one state in 1987).

State	Start of deregulation (effective opening date if any different)	State	Start of deregulation (effective opening date if any different)	State	Start of deregulation (effective opening date if any different)	State	Start of deregulation (effective opening date if any different)
AL	1987	IA	1991	NE	1990	RI	1984
AZ	1986	KS	1992	NV	1985	SC	1986
AR	1989	KY	1984 (1985)	NH	1987	SD	1988
CA	1987	LA	1987	NJ	1986	TN	1985
CO	1988	ME	1978 (1982)	NM	1989	TX	1987
CT	1983	MD	1985	NY	1982	UT	1984
DE	1988	MA	1983	NC	1985	VT	1988
FL	1985	MI	1986	ND	1991	VA	1985
GA	1985	MN	1986 (1987)	OH	1985	WA	1987
ID	1985	MS	1988	OK	1987	WV	1988
IL	1986	MO	1986	OR	1986	WI	1987
IN	1986	MT	1993	PA	1986	WY	1987

Table 3. Descriptive Statistics

This table presents the summary statistics for the variables used in the analysis. The dependent variables come from the Commodity Flow Surveys of 1977 and 1993 on 48 states. Our sample consists of the 48 contiguous states, which results in 4,512 state-pair-year observations (=48×(48–1)×2), as we exclude Alaska, Hawaii, and the District of Columbia. *TRADE_FLOW* is the trade size (in millions of dollars) as sampled by the Commodity Flow Survey from the origin state to a destination state. ln(*TRADE_SHARE*) is the trade share of the destination state among origin state's exports using the previous variable for volume of trade. The explanatory variables are as follows (indicator variable names are preceded by the prefix D_): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; *D_1993* is equal to 1 if year is equal to 1993, and 0 if it is equal to 1977; *D_DEREG* is equal to 1 if the state deregulated inter-state banking entry as of 1993, and 0 otherwise (as none of the states had deregulated inter-state banking entry as of 1977); *ADJACENCY* is equal to the inverse of the number of states with which the origin-state has a common border if the origin-destination state pair have a common border, and 0 otherwise; *D_BORDER_DEST* is equal to 1 if the destination state is on the U.S. national border, and 0 otherwise; *D_COASTAL_DEST* is equal to 1 if the destination state has a maritime border; *DISTANCE_CAPS* is the distance (in thousand miles) between an origin-destination state-pair's capitals; *D_RIVER* is equal to 1 if the origin-destination state pair is part of the Missouri or Columbia river systems or if both states have shores on the Great Lakes, and 0 otherwise; *D_SAME_COAST* is equal to 1 if origin-destination state pair are both on the Eastern or Western seaboards of the U.S. The endogenous variable BANK_INTEGRATION is the fraction of banking assets owned by out-of-state banks that belong to the other state in a given state pair (i.e., it is the total banking assets owned by state *m*'s banks in state *i* plus the total banking assets owned by state *i*'s banks in state *m*, divided by the sum of the banking assets of state *i* and *m*). IVs are as in Morgan, Rime and Strahan (2004): *D_DEREG_ORIG* (*D_DEREG_DEST*) is an indicator variables that equals 1 if the origin (destination) state has deregulated entry by 1993, and 0 otherwise; and *YEARS_DEREG_ORIG* (*YEARS_DEREG_DEST*) is the number of years the origin (destination) state has deregulated inter-state entry.

Variable	Number of observations	Mean	Std. Dev.	Min	Max
Dependent Variables:					
TRADE_FLOW	4,512	930.85	2,188.75	0	34,463.00
TRADE_SHARE	4,512	0.0213	0.0353	0	0.4770
Explanatory Variables:					
ln(*GDP_DEST*)	4,512	10.6958	1.1787	8.1244	13.6336
ln(WAGE_DEST)	4,512	2.0973	0.3931	1.4455	2.7709
D_1993	4,512	0.5000	0.5001	0	1
D_DEREG	4,512	0.7190	0.4496	0	1
ADJACENCY	4,512	0.0213	0.0730	0	1
D_BORDER_DEST	4,512	0.3333	0.4715	0	1

D_COASTAL_DEST	4,512	0.4375	0.4961	0	1
DISTANCE_CAPS	4,512	1,677.23	994.41	77.78	4,286.96
D_RIVER	4,512	0.2270	0.4189	0	1
D_SAME_COAST	4,512	0.1383	0.3453	0	1
Endogenous (Instrumented) Variable					
BANK_INTEGRATION	4,512	0.0019	0.0118	0	0.1845
Instrumental (Excluded) Variables (IVs)					
D_DEREG_ORIG	4,512	0.3225	0.4675	0	1
D_DEREG_DEST	4,512	0.3225	0.4675	0	1
YEARS_DEREG_ORIG	4,512	1.7453	2.7794	0	12
YEARS_DEREG_DEST	4,512	1.7453	2.7794	0	12

Table 4. Difference-in-Differences Regressions

This table presents the results of difference-in-difference regressions. The dependent variable, ln(*TRADE_SHARE*), is the trade share of the destination state among origin state's exports. The explanatory variables are as follows (indicator variable names are preceded by the prefix *D_*): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; D_1993 is equal to 1 if year is equal to 1993, and 0 if it is equal to 1977; *D_DEREG* is equal to 1 if the state deregulated inter-state banking entry as of 1993, and 0 otherwise (as none of the states had deregulated inter-state banking entry as of 1977); *ADJACENCY* is equal to the inverse of the number of states with which the origin-state has a common border if the origin-destination state pair have a common border, and 0 otherwise; *D_BORDER_DEST* is equal to 1 if the destination state is on the U.S. national border, and 0 otherwise; *D_COASTAL_DEST* is equal to 1 if the destination state has a maritime border; ln(*DISTANCE_CAPS*) is the log of the distance (in thousand miles) between an origin-destination state-pair's capitals; *D_RIVER* is equal to 1 if the origin-destination state pair is part of the Missouri or Columbia river systems or if both states have shores on the Great Lakes, and 0 otherwise; *D_SAME_COAST* is equal to 1 if origin-destination state pair are both on the Eastern or Western seaboards of the U.S. In all columns below difference-in-difference model is estimated using pooled-OLS with time-varying origin-state fixed-effects (i.e., with *separate* origin-state fixed effects for 1977 and 1993); except in columns (3) and (5) where it is estimated using *within* estimators using origin-destination state-pair fixed-effects. In all columns, standard-errors account for clustering of observations by the state of origin. t-stats are reported below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	"Full-sample"						Trade Flows ≥ $ 10 million			
	1		2		3		4		5	
ln(*GDP_DEST*)	1.0548	***	1.0568	***	1.1938	***	0.9485	***	1.0254	***
	(48.30)		(48.81)		(12.49)		(38.20)		(9.98)	
ln(*WAGE_DEST*)	-0.1383		-0.1561		-0.3065		-0.1522		-0.1059	
	(1.15)		(1.30)		(1.19)		(1.34)		(0.46)	
D_1993	-0.9581	***	-0.9869	***	-1.1362	***	-0.9509	***	-1.1983	***
	(11.59)		(12.17)		(5.42)		(13.07)		(6.29)	
D_DEREG			0.0430				0.0116			
			(0.85)				(0.33)			
D_1993 × *D_DEREG*			0.0752	*	0.0598		0.0921	***	0.0924	***
			(1.80)		(1.41)		(2.91)		(2.94)	
ADJACENCY	2.5026	***	2.3975	***			2.3792	***		
	(7.28)		(7.11)				(7.08)			

D_BORDER_DEST	0.0684 **	0.0669 **		0.0758 **	
	(2.12)	(2.11)		(2.42)	
D_COASTAL_DEST	-0.1093 **	-0.1035 **		-0.0124	
	(2.51)	(2.37)		(0.38)	
ln(*DISTANCE_CAPS*)	-0.8598 ***	-0.8614 ***		-0.7508 ***	
	(16.04)	(16.10)		(16.87)	
D_RIVERS	0.0269	0.0391		0.1035 **	
	(0.52)	(0.74)		(2.56)	
D_SAME_COAST	0.0715	0.0639		0.0000	
	(1.14)	(1.03)		(0.00)	
Estimation:					
Pooled-OLS	**yes**	**yes**	no	**yes**	no
Within (fixed-effects) estimator	no	no	**yes**	no	**yes**
Fixed-Effects (FE):					
Origin-state FE	**yes**	**yes**	no	**yes**	no
Origin-state FE x D_1993	**yes**	**yes**	no	**yes**	no
Origin-Destination state pair FE	no	no	**yes**	no	**yes**
Number of observations	4225	4225	4225	3512	3512
Number of groups (fixed effects)	96	96	2225	96	1756
Number of standard error clusters	48	48	48	48	48
Adjusted-R^2 (“Within”-R^2 in cols. 3 and 5)	0.7767	0.7771	0.0838	0.8066	0.0915

Table 5. Difference-in-Differences using Poisson Regressions

This table presents the results of difference-in-difference regressions. The dependent variable, *TRADE_FLOW*, is the trade size (in millions of dollars) as sampled by the Commodity Flow Survey from the origin state to a destination state. The explanatory variables are as follows (indicator variable names are preceded by the prefix *D_*): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; *D_1993* is equal to 1 if year is equal to 1993, and 0 if it is equal to 1977; *D_DEREG* is equal to 1 if the state deregulated inter-state banking entry as of 1993, and 0 otherwise (as none of the states had deregulated inter-state banking entry as of 1977); *ADJACENCY* is equal to the inverse of the number of states with which the origin-state has a common border if the origin-destination state pair have a common border, and 0 otherwise; *D_BORDER_DEST* is equal to 1 if the destination state is on the U.S. national border, and 0 otherwise; *D_COASTAL_DEST* is equal to 1 if the destination state has a maritime border; ln(*DISTANCE_CAPS*) is the log of the distance (in thousand miles) between an origin-destination state-pair's capitals; *D_RIVER* is equal to 1 if the origin-destination state pair is part of the Missouri or Columbia river systems or if both states have shores on the Great Lakes, and 0 otherwise; *D_SAME_COAST* is equal to 1 if origin-destination state pair are both on the Eastern or Western seaboards of the U.S. In all columns below difference-in-difference model is estimated using pooled-OLS with time-varying origin-state fixed-effects (i.e., with *separate* origin-state fixed effects for 1977 and 1993); except in columns (3) and (5) where it is estimated using *within* estimators using origin-destination state-pair fixed-effects. In all columns, standard-errors account for clustering of observations by the state of origin. t-stats are reported below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	"Full-sample"						Trade Flows ≥ $ 10 million			
	1		2		3		4		5	
ln(*GDP_DEST*)	0.9172	***	0.9164	***	0.9143	***	0.8881	***	0.9222	***
	(40.35)		(40.40)		(7.53)		(37.39)		(7.55)	
ln(*WAGE_DEST*)	-0.2426		-0.2780	*	-0.1537		-0.2562		-0.0999	
	(1.36)		(1.77)		(0.54)		(1.55)		(0.35)	
D_1993	0.3851	***	0.2934	***	0.2840		0.3203	***	0.2441	
	(2.88)		(2.85)		(1.51)		(2.88)		(1.30)	
D_DEREG			0.0350				0.0481			
			(0.95)				(1.32)			
D_1993 × *D_DEREG*			0.1986	***	0.1434	***	0.1865	***	0.1414	***
			(4.30)		(4.11)		(3.98)		(4.06)	
ADJACENCY	2.2421	***	2.0119	***			2.0555	***		
	(9.83)		(9.12)				(9.52)			

D_BORDER_DEST	0.0128	-0.0010		0.0087	
	(0.27)	(0.02)		(0.19)	
D_COASTAL_DEST	-0.0308	-0.0081		0.0112	
	(0.74)	(0.21)		(0.26)	
ln(*DISTANCE_CAPS*)	-0.6130 ***	-0.6119 ***		-0.5844 ***	
	(14.03)	(14.71)		(14.65)	
D_RIVERS	0.1600 ***	0.1742 ***		0.1627 ***	
	(3.71)	(4.18)		(4.00)	
D_SAME_COAST	0.0977	0.0637		0.0429	
	(1.55)	(1.02)		(0.70)	
Estimation:					
Pooled-Poisson regression	**yes**	**yes**	no	**yes**	no
Within (fixed-effects) Poisson	no	no	**yes**	no	**yes**
Fixed-Effects (FE):					
Origin-state FE	**yes**	**yes**	no	**yes**	no
Origin-state FE x D_1993	**yes**	**yes**	no	**yes**	no
Origin-Destination state pair FE	no	no	**yes**	no	**yes**
Number of observations	4512	4512	4450	3512	3512
Number of groups (fixed effects)	96	96	2225	96	1756
Number of standard-error clusters	48	48	48	48	48

Table 6. Fixed-Effects IV Regressions

This table presents the results of origin-destination state pair fixed-effects regressions without and with IV using two-stage generalized method of moments (IV-GMM-2S). The dependent variable, ln(*TRADE_SHARE*), is the trade share of the destination state among origin state's exports. The explanatory variables are as follows (indicator variable names are preceded by the prefix *D_*): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; D_1993 is equal to 1 if year is equal to 1993, and 0 if it is equal to 1977. The endogenous variable *BANK_INTEGRATION* is the fraction of banking assets owned by out-of-state banks that belong to the other state in a given state pair (i.e., it is the total banking assets owned by state m's banks in state *i* plus the total banking assets owned by state *i*'s banks in state *m*, divided by the sum of the banking assets of state *i* and *m*). IVs are as in Morgan, Rime and Strahan (2004): indicator variables that equal 1 if the origin (destination) state has deregulated entry by 1993 and 0 otherwise; and the number of years the origin (destination) state has deregulated inter-state entry. 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 level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	"Full-sample"						Trade Flows ≥ $ 10 million	
	1		2		3		4	
ln(*GDP_DEST*)	1.2035	***	1.2017	***	1.1484	***	1.0126	***
	(12.86)		(12.90)		(13.58)		(10.83)	
ln(*WAGE_DEST*)	-0.3138		-0.3178		-0.4177	*	-0.2150	
	(1.22)		(1.23)		(1.67)		(0.93)	
D_1993	-1.0999	***	-1.0966	***	-0.9852	***	-1.0803	***
	(5.35)		(5.34)		(4.91)		(5.49)	
BANK_INTEGRATION			0.5469		7.3626	**	11.0970	***
			(0.61)		(2.14)		(3.97)	
Estimation:								
Within (fixed-effects) estimator	**yes**		**yes**		no		no	
Fixed-effects IV with GMM 2-stage	no		no		**yes**		**yes**	
Origin-Destination state pair FE	yes		yes		yes		yes	

Number of observations	4225	4225	4000	3512
Number of FE groups	2225	2225	2000	1756
Number of standard-error clusters	48	48	48	48
F-statistic	56.88 ***	43.45 ***	48.83 ***	40.29 ***
Within-R^2	0.0835	0.0836	0.0654	0.0281
Identification diagnostics:				
First-stage F-stat			14.20 ***	15.27 ***
Partial-R^2			0.1058	0.1146
Under-identification test:				
Kleibergen-Paap rk LM-stat			24.49 ***	24.01 ***
Weak-identification test:				
Kleibergen-Paap Wald F-stat			14.20 *	15.27 *
Stock-Yogo (2005)				
critical values for:				
maximal IV relative bias …			10.27	10.27
…of			10%	10%
Over-identification test:				
Hansen-J stat			1.63	0.42
Hansen-J stat p-value			0.6521	0.9365

Table 7. Poisson IV Regressions

This table presents the results of Poisson regressions with and without IV using specifications that include origin-state fixed-effects. *TRADE_FLOW*, the dependent variable, is the trade size (in millions of dollars) as sampled by the Commodity Flow Survey from the origin state to a destination state. The explanatory variables are as follows (indicator variable names are preceded by the prefix *D_*): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; D_1993 is equal to 1 if year is equal to 1993, and 0 if it is equal to 1977; *D_DEREG* is equal to 1 if the state deregulated inter-state banking entry as of 1993, and 0 otherwise (as none of the states had deregulated inter-state banking entry as of 1977); *ADJACENCY* is equal to the inverse of the number of states with which the origin-state has a common border if the origin-destination state pair have a common border, and 0 otherwise; *D_BORDER_DEST* is equal to 1 if the destination state is on the U.S. national border, and 0 otherwise; *D_COASTAL_DEST* is equal to 1 if the destination state has a maritime border; ln(*DISTANCE_CAPS*) is the log of the distance (in thousand miles) between an origin-destination state-pair's capitals; *D_RIVER* is equal to 1 if the origin-destination state pair is part of the Missouri or Columbia river systems or if both states have shores on the Great Lakes, and 0 otherwise; *D_SAME_COAST* is equal to 1 if origin-destination state pair are both on the Eastern or Western seaboards of the U.S. The endogenous variable *BANK_INTEGRATION* is the fraction of banking assets owned by out-of-state banks that belong to the other state in a given state pair (i.e., it is the total banking assets owned by state *m*'s banks in state *i* plus the total banking assets owned by state *i*'s banks in state *m*, divided by the sum of the banking assets of state *i* and *m*). IVs are as in Morgan, Rime and Strahan (2004): indicator variables that equal 1 if the origin (destination) state has deregulated entry by 1993 and 0 otherwise; and the number of years the origin (destination) state has deregulated inter-state entry. Standard-errors and identification tests account for clustering of observations at the origin-state level. t-stats are reported in parentheses below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	Full-sample						**Trade Flows ≥ $ 10 million**	
	1		**2**		**3**		**4**	
ln(*GDP_DEST*)	0.9172	***	0.9101	***	0.9212	***	0.8865	***
	(40.35)		(41.25)		(49.81)		(50.12)	
ln(*WAGE_DEST*)	-0.2426		-0.1732		-0.1070		-0.0628	
	(1.36)		(1.20)		(0.87)		(0.52)	
D_1993	0.3851	***	0.3149	***	0.2341	*	0.2458	*
	(2.88)		(2.73)		(1.66)		(1.74)	
BANK_INTEGRATION			3.9169	***	6.5961	***	6.8093	***
			(5.91)		(5.55)		(5.86)	

ADJACENCY	2.2421	***	1.9839	***	1.7855	***	1.8319	***
	(9.83)		(9.19)		(9.68)		(10.16)	
D_BORDER_DEST	0.0128		0.0126		0.0151		0.0267	
	(0.27)		(0.29)		(0.50)		(0.88)	
D_COASTAL_DEST	-0.0308		-0.0141		-0.0151		0.0124	
	(0.74)		(0.35)		(0.41)		(0.35)	
ln(*DISTANCE_CAPS*)	-0.6130	***	-0.6039	***	-0.6006	***	-0.5677	***
	(14.03)		(14.97)		(20.92)		(20.29)	
D_RIVERS	0.1600	***	0.1709	***	0.1761	***	0.1731	***
	(3.71)		(4.55)		(4.60)		(4.76)	
D_SAME_COAST	0.0977		0.0453		0.0229		-0.0060	
	(1.55)		(0.72)		(0.38)		(0.10)	
Estimation:								
Pooled-Poisson regression	**yes**		**yes**		no		no	
Pooled-Poisson-IV	no		no		**yes**		**yes**	
Fixed-Effects (FE):								
Origin-state FE	**yes**		**yes**		**yes**		**yes**	
Origin-state FE x D_1993	**yes**		**yes**		**yes**		**yes**	
Number of observations	4512		4512		4512		3512	
Number of groups (fixed effects)	96		96		96		96	
Robust standard errors	yes		yes		yes		yes	

Table 8. Robustness Checks: Model Estimates with State-pair Trade-shares Less than 5% in 1977

This table presents the results of difference-in-difference and IV regressions for the sub-sample of state-pairs with trade shares less than 5% as of 1977 as a robustness check. ln(*TRADE_SHARE*), the dependent variable for the log-linear models in columns (1) and (3), is the trade share of the destination state among origin state's exports. *TRADE_FLOW*, the dependent variable for the Poisson regression models in columns (2) and (4), is the trade size (in millions of dollars) as sampled by the Commodity Flow Survey from the origin state to a destination state. The explanatory variables are as follows (indicator variable names are preceded by the prefix *D_*): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; D_1993 is equal to 1 if year is equal to 1993, and 0 if it is equal to 1977; *D_DEREG* is equal to 1 if the state deregulated inter-state banking entry as of 1993, and 0 otherwise (as none of the states had deregulated inter-state banking entry as of 1977); *ADJACENCY* is equal to the inverse of the number of states with which the origin-state has a common border if the origin-destination state pair have a common border, and 0 otherwise; *D_BORDER_DEST* is equal to 1 if the destination state is on the U.S. national border, and 0 otherwise; *D_COASTAL_DEST* is equal to 1 if the destination state has a maritime border; ln(*DISTANCE_CAPS*) is the log of the distance (in thousand miles) between an origin-destination state-pair's capitals; *D_RIVER* is equal to 1 if the origin-destination state pair is part of the Missouri or Columbia river systems or if both states have shores on the Great Lakes, and 0 otherwise; *D_SAME_COAST* is equal to 1 if origin-destination state pair are both on the Eastern or Western seaboards of the U.S. The endogenous variable *BANK_INTEGRATION* is the fraction of banking assets owned by out-of-state banks that belong to the other state in a given state pair (i.e., it is the total banking assets owned by state *m*'s banks in state *i* plus the total banking assets owned by state *i*'s banks in state *m*, divided by the sum of the banking assets of state *i* and *m*). IVs are as in Morgan, Rime and Strahan (2004): indicator variables that equal 1 if the origin (destination) state has deregulated entry by 1993 and 0 otherwise; and the number of years the origin (destination) state has deregulated inter-state entry. In all columns, standard-errors account for clustering of observations by the state of origin. t-stats are reported below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	Difference-in-Differences Regressions (Trade Flows ≥ $ 10 million)				IV Regressions (Trade Flows ≥ $ 10 million)			
	1		2		3		4	
ln(GDP_DEST)	1.0401	***	0.7964	***	1.0201	***	.8559	***
	(9.49)		(7.42)		(10.07)		(40.97)	
ln(WAGE_DEST)	-0.1819		-0.2519		-0.3261		-0.2003	
	(0.77)		(0.88)		(1.37)		(1.33)	
D_1993	-1.1410	***	0.5220	***	-0.9827	***	0.4228	***
	(5.78)		(2.64)		(4.85)		(2.58)	
D_1993 × D_DEREG	0.1043	***	0.1920	***				
	(3.33)		(4.99)					

D_BANK_INTEGRATION					22.3229	***	7.4196	***
					(3.20)		(3.32)	
ADJACENCY							1.8999	***
							(6.89)	
D_BORDER_DEST							-0.0052	
							(0.14)	
D_COASTAL_DEST							-0.0336	
							(0.85)	
ln(*DISTANCE_CAPS*)							-0.5952	***
							(14.74)	
D_RIVERS							0.1408	***
							(3.16)	
D_SAME_COAST							-0.0338	
							(0.50)	
Estimation:								
Within (fixed-effects) estimator	**yes**		**yes**		no		no	
IV-GMM-2S estimator with FE	no		no		**yes**		no	
IV-Poisson estimator	no		no		no		**yes**	
Fixed-Effects (FE):								
Origin-Destination state pair FE	**yes**		**yes**		**yes**		no	
Origin-state FE x D_1993	no		no		no		**yes**	
Number of observations	3006		3006		3006		3006	
Number of groups (fixed effects)	1503		1503		1503		96	
Number of standard error clusters	48		48		48		48	
F-statistic [Wald-chi^2]	41.26	***	[5395.30]	***	31.50	***		
Within-R^2	0.0947							

Table 9. Robustness Checks: Difference-in-Differences with Federal Deregulation (IBBEA) of 1994

This table presents the results of difference-in-difference regressions with 1993-1997 CFS data. The dependent variables are (i) ln(*TRADE_SHARE*), the trade share of the destination state among origin state's exports (in columns (1) and (3)), and (ii) *TRADE_FLOW,* the trade size (in millions of dollars) as sampled by the Commodity Flow Survey from the origin state to a destination state.. The explanatory variables are as follows (indicator variable names are preceded by the prefix *D_*): ln(*GDP_DEST*) is the destination state's GDP; ln(*WAGE_DEST*) is the destination state's wage index; *D_1997* is equal to 1 if year is equal to 1997, and 0 if it is equal to 1993; *D_FEDERAL_DEREG* is equal to 1 if the state had not deregulated inter-state banking entry as of 1994 and was subject to opening of its banking market through the Interstate Banking and Branching Efficiency Act (IBBEA, also called the Riegle-Neal Act) which into effect in 1995, and 0 otherwise; *ADJACENCY* is equal to the inverse of the number of states with which the origin-state has a common border if the origin-destination state pair have a common border, and 0 otherwise; *D_BORDER_DEST* is equal to 1 if the destination state is on the U.S. national border, and 0 otherwise; *D_COASTAL_DEST* is equal to 1 if the destination state has a maritime border; ln(*DISTANCE_CAPS*) is the log of the distance (in thousand miles) between an origin-destination state-pair's capitals; *D_RIVER* is equal to 1 if the origin-destination state pair is part of the Missouri or Columbia river systems or if both states have shores on the Great Lakes, and 0 otherwise; *D_SAME_COAST* is equal to 1 if origin-destination state pair are both on the Eastern or Western seaboards of the U.S. In all columns below the difference-in-differences model is estimated using with time-varying origin-state fixed-effects (i.e., with *separate* origin-state fixed effects for 1993 and 1997). In columns (1) and (3) the estimator is pooled-OLS, in columns (2) and (4) the estimator is pooled-Poisson regression. In all columns, standard-errors account for clustering of observations by the state of origin. t-stats are reported below coefficient estimates. *, **, *** denote statistical significance at 1%, 5%, and 10% levels.

	Trade Flows ≥ $ 10 million (in 1977-dollars)			
	Pooled-OLS		Pooled-Poisson	
	1		2	
ln(GDP_DEST)	0.9698	***	0.8905	***
	(41.30)		(35.32)	
ln(WAGE_DEST)	-0.5632	***	-0.4142	**
	(4.58)		(2.13)	
D_1997	-0.2027	***	-0.0818	***
	(10.48)		(2.94)	
D_FEDERAL_DEREG	-0.0849	*	-0.1942	***
	(1.91)		(3.23)	
D_1997 × D_FEDERAL_DEREG	0.0404	*	0.1079	***
	(1.94)		(3.02)	

ADJACENCY	2.8185	***	2.2491	***
	(8.66)		(10.49)	
D_BORDER_DEST	0.1286	***	0.0332	
	(4.38)		(0.83)	
D_COASTAL_DEST	-0.0531	*	-0.0065	
	(1.76)		(0.14)	
ln(DISTANCE_CAPS)	-0.7923	***	-0.5882	***
	(18.76)		(14.60)	
D_RIVERS	0.1150	**	0.1746	***
	(2.54)		(3.52)	
D_SAME_COAST	-0.0378		0.0108	
	(0.80)		(0.18)	
Estimation:				
Pooled-OLS regression	**yes**		no	
Pooled-Poisson regression	no		**yes**	
Within-(OLS)-estimator	no		no	
Within-Poisson-estimator	no		no	
Fixed-Effects (FE):				
Origin-state FE	**yes**		**yes**	
Origin-state FE × D_1997	**yes**		**yes**	
Origin-destination state-pair FE	no		no	
Number of observations	3688		3688	
Number of groups (fixed effects)	96		96	
Number of standard-error clusters	48		48	
Adjusted-R^2 [pseudo-R^2]	0.8290		[0.9124]	