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Bank linkages and international trade☆

Julian Caballero [a], Christopher Candelaria [b], Galina Hale [c,*]

[a] IADB, United States
[b] Vanderbilt University, United States
[c] Federal Reserve Bank of San Francisco, United States

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ABSTRACT

We uncover a new channel through which international finance is related to trade: the formation of international bank linkages increases exports. Bank linkages are measured for each pair of countries in each year as a number of bank pairs in these two countries that are connected through cross-border syndicated lending. Using a gravity approach to model trade for 66 countries over 24 years with a full set of fixed effects (source-year, target-year, source-target), we find that new connections between banks in a given country-pair lead to an increase in trade flows between these countries in the following year and to trade diversions from countries competing for similar imports. We conjecture that the mechanism for this effect is the role bank linkages play in reducing export risk and present evidence supporting this conjecture. In particular, using industry-level trade data and controlling for country-pair-year and industry fixed effects, we find that new bank linkages have larger impacts on trade in industries which tend to be subject to more export risk. For U.S. banks, we can show that bank linkages are positively associated with foreign letter of credit exposures.

1. Introduction

The Global Financial Crisis, which erupted as a result of the U.S. subprime mortgage crisis, brought international financial markets to a standstill and severely disrupted international trade (Alessandria et al., 2011; Bems et al., 2013; Chor and Manova, 2012). Since then, researchers have highlighted the importance of the relationship between finance and trade.[1] We contribute to this growing literature by testing whether bank linkages facilitate international trade. Although only a fraction of trade transaction payments are intermediated by banks via letters of credit,[2] we empirically demonstrate that there is a positive effect of bank linkages—that is, relationships formed through long-term interbank lending—on export flows. To explain this result, we conjecture that export risk is one of the mechanisms through which bank linkages affect trade. We test this conjecture and provide supporting

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* Corresponding author.
E-mail address: galina.b.hale@sf.frb.org (G. Hale).

[1] See surveys by Manova and Foley, 2015 and Contessi and de Nicola, 2012.

[2] Antràs and Foley (2015) estimate that the share of trade transaction payments intermediated by banks via letters of credit is about 17%, while Niepmann and Schmidt-Eisenlohr (2017a) measure it to be 10% of U.S. goods exports. However, banks might also be involved in additional trade transactions (for example, pre-export financing, pre-import financing, and factoring), so bank involvement is potentially larger.

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evidence that bank linkages reduce the risk that exporters face in international trade transactions.

Our empirical analysis is based on the standard gravity model framework outlined in Feenstra (2004). This approach, as opposed to a firm–level analysis, allows us to look for patterns that are common to many countries over a long period of time. We augment the model by adding an export risk factor, which we interact with our measure of bank linkages. This simple framework predicts that bank linkages are positively associated with trade and that the relationship becomes stronger when the export risk factor is larger. We test the model predictions by employing both aggregate- and industry-level regression specifications using bilateral trade data from COMTRADE for 66 countries for the period 1990–2013.

In an ideal world, we would perform our analysis using data on cross-border bank-to-bank lending relationships, focusing on instruments that directly reduce export risk (such as letters of credit). However, such datasets are not currently available for a cross-section of countries. We think the best available data are from the syndicated loans market and proxy for the tightness of bank linkages using individual loan-level data on syndicated loans extended to banks, which we obtain from Dealogic's Loan Analytics database. There are four reasons why syndicated loans are a suitable proxy for bank linkages: first, they tend to be large and are extended for a medium term (median is 3 years), which leads to substantial information acquisition by lenders; second, data are available on borrowers as well as lenders, which allows us to construct bilateral linkages; third, international syndicated loan markets are large and active (Cerutti et al., 2015); fourth, bank exposures to foreign countries via syndicated loans are a good proxy for their total exposures to these countries, explaining two thirds of the variation of total exposures across countries for the U.S. banks (Hale et al., 2016).

There are two other notable measures of bank linkages used in the literature. The BIS Banking Statistics of outstanding bank claims allow one to build proxies of cross-border stocks and flows of loans at an aggregate level. There are two issues, however. First, aggregate nature of the data is likely to mask cross-country heterogeneity in the intensity of bank relationships. Second, BIS data are only available for 812 country-pairs, which is a small subset of the 4290 country-pairs in our baseline sample. Notwithstanding, in robustness checks we control for BIS's measures of stocks and flows and show that our baseline results are robust to the inclusion of these measures. Another important measure is that of the bank ownership linkages introduced by Claessens and Van Horen (2014), which is available for a large cross-section of banks and country-pairs. It captures a rather different concept of interest: ownership relationships between majority shareholders of foreign banks, rather than lending relationships and the information flows they entail.[3]

We construct a global network of banks in which relationships are formed when banks extend syndicated loans to each other.[4] We take into account the direction of resulting linkages, but not the amount lent, because we are interested in these lending flows only as a proxy for banks' information acquisition.[5] This approach has two advantages: first, our constructed binary linkages are less affected by overall capital flows; second, we do not have to impute individual participants' shares of the syndicate loan when values are missing. We aggregate individual bank linkages by computing the sum of linkages formed by all bank loans extended by banks in country i to banks in country j. It is important to note that most linkages in our data do not have any direct or even indirect connections to trade activity.[6]

We begin our empirical analysis by testing whether new bank linkages formed between two countries in a given year affect trade between these two countries in the following year. We find that new bank linkages formed through bank lending in country i to country j in year $t - 1$ are positively associated with exports from i to j in year t. Increasing the change in the intensity of bank linkages due to new banking connections by 50% (about one standard deviation change) is associated with an increase in trade in the subsequent year of about 13%. This large effect, however, is partly due to common factors affecting both trade and bank linkages across country pairs and over time. Since bank linkages vary by country pairs and over time, we are able to control for a full set of fixed effects: country-pair, exporter-year, and importer-year in our preferred specifications. Once we control for the full set of fixed effects, we find the effect of the same change in bank linkages is associated with a 3.5% increase in international trade.[7]

The baseline result eliminates some alternative explanations and mitigates concerns of omitted variables bias by controlling for country-pair, time-varying covariates that may affect exports, such as the adoption of bilateral or regional trade agreements, the strength of financial ties, the occurrence of financial crises, and the adoption of a common currency. Furthermore, the result holds when we test for potential specification problems by estimating a Poisson pseudo-maximum-likelihood model following Santos Silva and Tenreyro (2006) and Santos Silva and Tenreyro (2010).

The causal interpretation of our baseline result, however, is potentially threatened by unobserved common factors that drive both bank linkages and exports and by reverse causality. To address the presence of common factors, we saturate our regression models with multiple sets of fixed effects, which include different combinations of country-specific effects, country-pair effects, and year fixed effects. In industry-level regressions, we are able to include country-pair-year fixed effects, which further alleviate common factor concerns by absorbing all dynamics of bank linkages and trade for each pair of countries. To assess the potential for reverse causality, we estimated a regression of new bank linkages on lagged exports. The regression evaluates the argument that banks from export markets might establish new linkages in import markets in anticipation of an increase in exports. While we found that the effect is positive and statistically significant, it is five times smaller than the effect of bank linkages on exports in our baseline regression,[8] which means that while reverse causality is a valid concern, it is unlikely to explain all of our results. Moreover, even if we believe reverse causality is an important driving factor, the positive result underscores the importance of bank linkages for facilitating exports, which aligns with the main argument of this paper.

We perform two sets of tests to further allay concerns about the presence of endogeneity induced by reverse causality. First, we demonstrate that formation of new bank linkages for a given country pair i,j creates trade diversion from countries k that compete with the destination country for similar imports (i.e., the formation of new bank linkages between i and j reduces exports from i to k). Assuming bank linkages between i and j are exogenous to exports from i to k, the trade diversion effect we find would not arise if our benchmark results were driven by reverse causality. However, it is possible that bank linkages may not be exogenous to exports from i to k if these exports are expected

[3] In fact, the results from Claessens et al. (2015) suggest the existence of a bank-ownership channel underpinning international trade; however, this is not necessarily related to export risk, which is the focus of our paper.

[4] In this regard, we differ from Garratt et al. (2011); Kubelec and Sá (2012); Minoiu and Reyes (2013); von Peter (2007), who construct banking networks at the aggregate level, using BIS data. See Hale (2012) for the discussion of advantages of the bank-level approach and Cerutti et al. (2015) for the comparison of the coverage of syndicated loan and aggregate data sets.

[5] Arguably, larger loans may warrant more information acquisition. However, in our sensitivity analysis we find that including loan amounts does not improve explanatory power of bank linkages, but rather adds noise to the estimates. One potential reason is that the loans we study are all large, and therefore, the difference in information intensity of lending is unlikely to be important.

[6] Only a negligible subset of loans in our data, 0.1% in terms of the number of loan tranches, is extended as back-up credit lines for trade credit, but most trade financing does not take the form of inter-bank syndicated loans.

[7] Note that this result is larger but in the same ballpark as that of Michalski and Ors (2012) who find that bank integration increases inter-state trade by 15% over 10 years after an increase in bank integration from 0 to the mean of the sample.

[8] Please see Online Appendix.

to be redirected to j and trigger demand for special financing services. While we cannot rule out this possibility, it does suggest a channel by which linkages matter for trade. Second, we test whether bank linkages formed by banks in country j lending to banks in country i affect exports from i to j, and find that there is no such effect.[9] This serves as a placebo test for our analysis: if our benchmark result was driven by spurious correlation between financial and trade flows, it would be as likely to show up in the reverse direction of flows as in the benchmark specification. Information acquisition, however, is only conducted by lenders about the borrowers and not vice-versa. Taken together, these tests place limits on the pervasiveness of reverse causality. In the absence of a natural experiment, we cannot fully rule out this fundamental endogeneity concern, but at a bare minimum, this paper provides strong evidence for the role that international bank linkages have on exports.

Having found evidence that increasing the change in the intensity of new bank linkages is associated with increases in trade, we examine whether export risk is one of the mechanisms that drive this result. We establish the plausibility of an export risk mechanism using three approaches. We examine whether bank linkages matter more for exports to riskier countries, whether they matter more for exports of riskier goods, and whether their effect is associated with the use of letters of credit, which are designed to address export risk. These tests also provide support for the causal interpretation of the effect of new bank linkages on exports, first through controlling for country-pair-year fixed effects, which absorb all overall dynamics of bank linkages and exports, and then by showing that even controlling for trade credit bank linkages still have effect on trade.

With respect to our first approach, we show that the effect of bank linkages on exports from country i to country j is larger for exports to countries with weaker contract enforcement and therefore higher export risk (Anderson and Marcouiller, 2002). Following Berkowitz et al. (2006) and Antràs and Foley (2015), we proxy for contract enforcement using the International Country Risk Guide (ICRG) index and find that the effect of bank linkages is twice as large for exports to countries with weak legal and political institutions. We confirm this finding using three additional proxies for contract enforcement and export risk: country credit ratings from S&P, insurance premium on exports to each country provided by the U.S. Exports-Imports bank, and OECD membership.[10]

For the second approach, we examine whether the effect of bank linkages on export flows is larger for differentiated goods than for either homogeneous goods or goods traded based on reference prices.[11] In interpreting these tests, we rely on the literature suggesting that trade involving more differentiated goods is subject to heightened export risk with respect to differentiated goods. There are several reasons why this may be the case. More differentiated products possess more complex characteristics rendering export contracts highly incomplete and more subject to contract enforcement and export risk (Berkowitz et al., 2006). This increases both the exporter's risk of not receiving payment and the importer's risk of receiving an inadequate shipment. Furthermore, the complexity of more differentiated products not only makes their quality uncertain to the buyer (Ranjan and Lee, 2007), but it also increases the probability that a dispute between exporter and importer arises, delaying the settlement. Finally, as argued by Nunn (2007), more complex goods involve higher levels of customization and relationship-specific investments. All these factors make trade of more differentiated or complex products more sensitive to export risk.[12]

To conduct our analysis based on differentiated products, we first compute total exports of homogeneous, reference, and differentiated goods for each country pair, and we repeat our baseline regression analysis separately for each goods category. We find that for differentiated goods, the estimated effect of having bank linkages is twice as large as what we estimate for reference goods, which in turn is twice as large as the effect for homogeneous goods. We then estimate the regressions at the country-pair-year-industry level. For these regressions, we can control for country-pair-year and industry fixed effects, which absorb any factor that varies over time by country-pair, including bank linkages. In this set-up, any common factor among goods categories that may create spurious correlation between formation of bank linkages and trade is absorbed by country-pair-year fixed effects. However, we can still identify the differential effect of bank linkages in different categories of goods. We find that, relative to homogeneous goods, bank linkages matter more for reference goods and even more for differentiated goods.

Finally, for the third approach, we examine whether the effect of bank linkages is associated with letters of credit, which is one avenue by which bank linkages can reduce export risk (Niepmann and Schmidt-Eisenlohr, 2017b; Olsen, 2015; Antràs and Foley, 2015). Letters of credit are typically issued by a bank in the importer's country and confirmed by a bank in the exporter's country, making bank linkages particularly important. We test for this channel using data on U.S. exports, the only country for which we can obtain data on banks' letters of credit exposures. We first document that bank linkages are much more important for U.S. exports than on average in our sample. We then demonstrate that letters of credit are positively associated with new bank linkages. However, not all of the effect of bank linkages on exports is due to the issuance of letters of credit. We find that bank linkages matter even when controlling for letters of credit. We understand these results as showing that issuance of the letters of credit is one, but not the only way by which bank linkages facilitate exports.

Overall, we provide ample evidence that the effect of bank linkages on trade likely works through the amelioration of export risk. Our identification strategy leverages the richness of bilateral trade data, which allows us to control for a full set of country-time and pair-time fixed effects in our benchmark results, as well as additional country-pair-time fixed effects in industry-level regressions. Our paper's main contribution, then, is twofold: first, we demonstrate a novel channel through which finance is important for trade; namely, a positive effect of bank linkages that are formed through syndicated loans extended to banks. Second, we demonstrate that this effect is likely due to the reduction of export risk.

The paper, thus, contributes to the growing body of literature on the relationship between finance and trade. In addition to the papers showing the importance of actual trade finance and trade guarantees cited earlier, Manova (2008) and Minetti and Zhu (2011) demonstrate the importance of credit constraints for exports.[13] Recently, Claessens et al. (2015) showed that foreign operations of international banks also play an important role in facilitating trade. Similarly, in the context of the U.S., Michalski and Ors (2012) leverage a natural experiment to show that bank integration across the 48 contiguous states causes inter–state trade to grow. Their analysis is closer to that of Claessens et al. (2015) in that they define banking integration as cross–state ownership of bank assets. Our paper extends these results to show that in

[9] The results are available upon request.

[10] We find that for exports to countries with credit rating of A+ or higher, bank linkages do not matter, while for countries with credit rating of BBB- or lower, they are twice as important relative to the average estimate for the sample. Bank linkages are only important for exports to countries for which export insurance premium exceeds the average by about 0.8 of a standard deviation. Similarly, using OECD membership as a crude proxy of contract enforcement, we find that for exports from any country to an OECD country, bank linkages have no effect; however, for exports from an OECD country to a non-OECD country bank linkages have a positive effect.

[11] Our product classifications come from Rauch (1999), who defines differentiated products as those for which trade is not based on reference prices, where these prices can be quoted either in organized exchanges or in trade publications. As highlighted by Rauch and Trinidade (2002), goods that possess reference prices are sufficiently homogeneous such that prices convey all relevant information for international trade; this is not the case for more differentiated goods which lack reference prices.

[12] Indeed, Hoefele et al. (2016) find that cross-border trade in more complex goods exhibit larger shares of cash-in-advance forms of payments relative to less complex industries.

[13] Paravisini et al. (2015), however, find that in the case of Peru a shortage of credit affects production rather than export-specific activities.

the international trade context not only ownership linkages, but also relationships between banks established via inter–bank syndicated lending contribute to trade.

In addition, our paper contributes to an understanding of border effects by showing that various proxies for export risk are associated with lower exports and that such risk is likely reduced through bank linkages between countries. Extant literature has found other avenues of export risk mitigation. For example, Rauch and Trinidade (2002) show the importance of ethnic networks; Guiso et al. (2009) show the role of trust in explaining international trade patterns; and Cristea (2011) and Poole (2012) show the importance of business relations. We add bank linkages formed through bank-to-bank syndicated lending to this list.

Moreover, by relating bank linkages to trade, our paper contributes to the literature on the role of financial flows in international business cycles. A more precise understanding of mechanisms through which financial flows affect economic relationships between countries can shed further light on this issue.[14] Finally, our paper also relates to the more general literature on effects of bank linkages which, not surprisingly after the Global Financial Crisis, focused predominantly on the risk and contagion aspects of bank linkages.[15] Our paper contributes to this literature by demonstrating that bank linkages also play a positive role in the global economy.

In the next section we present the theoretical background for our analysis in order to formulate the hypothesis and discipline our empirical approach. Section 3 discusses the empirical strategy and some identification issues that arise in our analysis. Section 3 describes our data. In Section 4 we present and discuss our main results. Section 5 presents robustness tests. Section 6 closes with concluding remarks and a brief discussion of the possible mechanisms by which bank linkages may reduce export risks in international trade.

2. Theoretical background

Our empirical analysis fits well in the general framework of the gravity model of trade. To show this, we review the basic microfoundations of the model following the textbook presentation of Feenstra (2004) and augment it with a simple set-up that relies on existing theory of trade and finance.

Assume that preferences of a representative consumer are isoelastic (CES) and that consumers in each country j consume goods produced in all other countries $i \in [1, C]$ so that the utility function is

$$U^j = \sum_{i=1}^{C} N^i \left(c^{ij}\right)^{\frac{\sigma-1}{\sigma}},$$

where N^i is the number of goods produced in country i and c^{ij} is country j's consumption of goods made in i, which also corresponds to the volume of exports from i to j, and $\sigma > 1$ is the elasticity of substitution. We assume that all goods produced in country i are sold in country j for the same price p^{ij}. We also assume balanced trade, which implies that the budget constraint for country j is given by its total output Y^j produced using constant return to scale technology as

$$Y^j = \sum_{i=1}^{C} N^i p^{ij} c^{ij}.$$

The optimization yields

$$c^{ij} = \left(\frac{p^{ij}}{P^j}\right)^{-\sigma} \frac{Y^j}{P^j},$$

where P^j is the CES price index

$$P^j = \left(\sum_{i=1}^{C} N^i \left(p^{ij}\right)^{1-\sigma}\right)^{\frac{1}{1-\sigma}}.$$

The value of exports is then

$$X^{ij} = N^i Y^j \left(\frac{p^{ij}}{P^j}\right)^{1-\sigma}.$$

Assuming labor to be the only input and full employment (Krugman, 1979), the zero-profit condition implies that $Y^i = yN^i p^i$, where y is the labor productivity, N^i is the labor supply in country i and p^i is the price of the domestically produced output in country i. Further assume that there is a wedge T^{ij} between the price of the good made in country i sold domestically, p^i, and the same good sold in country j, $p^{ij} = T^{ij} p^i$, with $T^{ii} = 1$, $T^{ij} > 1$.

Combining all of the above, we can express the value of exports from i to j in each period t as

$$X_t^{ij} = \frac{Y_t^i Y_t^j}{\left(p_t^i\right)^{\sigma} y} \left(\frac{T_t^{ij}}{P_t^j}\right)^{1-\sigma}.$$

The wedge T^{ij} between domestic and foreign prices has been given many interpretations in the literature, including transportation costs, trade barriers, and information costs. Here, we will focus on what we believe are two important components: geographical distance and export risk that may arise from asymmetric information and institutional factors that complicate payment enforcement. Our specific interpretation of export risk is related to the cost of payment or contract enforcement in cross-border deals, the importance of which is well documented in Anderson and Marcouiller (2002). This cost is likely to be increasing with distance because of longer shipping time,[16] and it will also be affected by the quality of institutions in country j and by how differentiated the traded good is, which we do not model explicitly.[17] Further assume that this cost can be reduced if banks in country i are closely linked with banks in country j, either through direct payment enforcement and guarantees as in Olsen (2015), through extending letters of credit as in Schmidt-Eisenlohr (2013), or through selection of creditworthy counterparties by banks in country j. In particular, Antràs and Foley (2015) and Schmidt-Eisenlohr (2013) show that financial costs take the form of iceberg costs and that banks can reduce them.[18] Thus, we assume

$$T_t^{ij} = D^{ij} \left(R_t^j\right)^{\left(1-a_t^{ij}\right)},$$

where D^{ij} is constant distance between countries i and j, R_t^j is the cost of contract enforcement in country j in the absence of bank linkages, and a_t^{ij} is the strength of bank linkages between countries i and j.

[14] While Imbs (2006) shows a positive cross-country correlation between financial flows and business cycle co-movements, Kalemli-Ozcan et al. (2013b) finds a negative within correlation.

[15] See, among others, Battiston et al. (2012); Cocco et al. (2009); Craig and von Peter (2014); Delli Gatti et al. (2010); Elliot et al. (2014); Giannetti and Leaven (2012); Haldane and May (2011); Imai and Takarabe (2011); Kalemli-Ozcan et al. (2013a); May and Arinaminpathy (2010); Nier et al. (2007); Sachs (2014) and von Peter (2007).

[16] Antràs and Foley (2015) and Niepmann and Schmidt-Eisenlohr (2017a) show that cash-in-advance or letter of credit, both of which are used as remedies for higher export risk, are more likely to be used for longer distance trade. Schmidt-Eisenlohr (2013) presents a model which rationalizes this result.

[17] As explained in the introduction, more differentiated products may be more subject to contract enforcement and export risk because of their complexity and hence incompleteness of their contracts (Berkowitz et al., 2006), or because buyers face larger uncertainty about their quality (Ranjan and Lee, 2007).

[18] In the model by Michalski and Ors (2012) banks that are present in both states can better assess trade–related risks.

Combining the above and taking logs, we obtain

$$\ln X_t^{ij} = \ln Y_t^i + \ln Y_t^j - (\sigma-1)\ln D^{ij} - (\sigma-1)\ln R_t^j \quad (1)$$

$$+(\sigma-1)a_t^{ij}\ln R_t^j - \sigma\ln p_t^i - \ln y + (\sigma-1)\ln P_t^j. \quad (2)$$

From this equation we can draw two main testable implications with respect to bank linkages:

1. exports are an increasing function of bank linkages a_t^{ij}, and
2. the effect of bank linkages is stronger the higher the export risk in country j, R_t^j.

In what follows, we will test these two predictions.

3. Empirical gravity model with export risk

The empirical gravity model is a direct application of the equations derived in the theoretical discussion. For our benchmark model, we estimate the following equation:

$$ex_{ijt} = \alpha + \beta al_{ijt-1} + GV'_{it}\delta_1 + GV'_{jt}\delta_2 + GC'_{ij}\gamma + \varepsilon_{ijt},$$

where *ex* is a log of exports, *al* is the measure of newly formed bank linkages, described in Section 3, vector *GV* (i.e., gravity variables) includes GDP per capita, population in both countries, vector *GC* (i.e., gravity constant variables) includes time-invariant country-pair gravity variables such as distance between countries' capitals, indicator of whether the countries are contiguous, have common language, and whether they share colonial past. We vary α from being a constant to a vector of fixed effects: (t, i, j), to (ij, it, jt) for a fully saturated model. In the regressions at the industry level we also include *ijt* fixed effects, which absorb all variables that vary at country-pair-year level. Our benchmark specification is a fully saturated model where the effects of all gravity controls listed above are absorbed by fixed effects:

$$ex_{ijt} = \alpha_{ij} + \alpha_{it} + \alpha_{jt} + \beta al_{ijt-1} + \varepsilon_{ijt},$$

with β being our coefficient of interest, which we expect to be positive. Note that we include changes in our aggregate bank proximity measures lagged by one year. Thus, we test how new connections formed between banks in year $t-1$ affect trade in year t. We also tested whether bank linkages formed by lending from j to i affect exports from i to j and found no effects. This is consistent with our interpretation of the measure of bank linkages as a proxy of information flow, since information flows from borrower to lender and the relevant direction is when payments have to flow in the same direction—from importer to exporter.

As shown in the theoretical discussion, if our hypothesis of bank linkages mitigating export risk is correct, the effect of bank linkages will vary with the export risk of country j. We first separate countries into OECD and non-OECD as a rough measure of contract enforcement and estimate our main specification for the four possible country-pair types. We then extend our analysis by including more direct proxies for export risk by estimating the model:

$$ex_{ijt} = \alpha_{ij} + \alpha_{it} + \alpha_{jt} + \beta_1 al_{ijt-1} + \beta_2 R_{it} al_{ijt-1} + \beta_3 R_{jt} al_{ijt-1} + \varepsilon_{ijt},$$

where R_{it} and R_{jt} are proxies for risk of exporting to i and j, with their main effects absorbed by α_{it} and α_{jt}. We use ICRG and S&P country ratings as well as export insurance premia as proxies for export risk. We expect both β_1 and β_3 to be positive, while we expect β_2 to be zero since there should be no effect of risk of exporting to i on exports from i to j. We include this measure, however, as a placebo test: if β_2 turns out to be positive and significant, it might indicate spurious correlation between bank linkages augmented by risk measures and trade.

As explained in the introduction, we also build on the insight that trade in more differentiated goods is expected to be subject to heightened export risk because the enforcement of contracts in these goods is more difficult (Berkowitz et al., 2006; Nunn, 2007; Ranjan and Lee, 2007). Thus, we expect the effect of bank linkages to be higher for more differentiated goods. We, therefore, estimate the model separately for exports of homogeneous and differentiated goods, expecting coefficient β to be higher for differentiated goods.

In addition, we estimate a specification with *ijt* fixed effects using the data disaggregated at SITC-4 level. For each industry k we measure exports from i to j in year t. Since everything that varies at country pair-year level, including the main effect of bank linkages, is absorbed by the fixed effect, we estimate the effect of the interaction between the index of how differentiated the goods are (D_k) and our measure of bank linkages. We expect bank linkages to matter more for more differentiated goods, that is, we expect β to be positive in the following model that includes *ijt* fixed effects:

$$ex_{kijt} = \alpha_{ijt} + \alpha_k + \beta D_k al_{ijt-1} + \varepsilon_{ijt}.$$

We conduct additional analysis and several robustness tests, which are described later in the paper.

4. Data

To test the hypotheses outlined above, we collect data on international trade flows, construct a measure of bank linkages using data on banks' syndicated loans market, and use several proxies for export risk, which are described in this section. To control for country-level variables in our gravity set up, we use data on GDP and population from the IMF's *World Economic Outlook* (WEO) database,[19] and use standard gravity-model variables, such as distance, colonial ties, common language and geographic size, from Head and Mayer (2013) (obtained through the website of CEPII).

4.1. International trade

As a measure of bilateral exports from country i to country j, we use reported imports by country j from country i in the UN-COMTRADE database. By privileging the importer's reports, instead of the exporter's, we follow Feenstra et al. (2005) and assume that the former are more accurate than the latter. We use the line "S2-TOTAL" as reported in nominal USD. We then deflate these values by the U.S. CPI (obtained from the U.S. Bureau of Labor Statistics). Our analytic trade data sample is a balanced panel consisting of 66 countries for the period of 1990–2013 at an annual frequency.[20] We conduct our analysis by applying the following logarithmic transformation to the data:

$$ex_{ij} = \log(1 + EX_{ij}),$$

which allows us to preserve the zeros. Out of $102{,}960 = 66 \times 65 \times 24$ observations in the period 1990–2013, only 10,403, or 10%, are zeros. In more recent years, the zeros in our data compose a smaller proportion of observations for a given year. As shown in Fig. A.1 in the Appendix, the proportion of zeros in our data went from 19% in 1995 to less than 1% in 2010. So, while there is a small mass of country pairs with no trade, especially in the early part of our sample, it is not large enough to influence our results. In the robustness tests section, we show that all results in the paper hold if we exclude country pairs without trade. We also demonstrate that our results hold if we use the Poisson pseudo-

[19] We choose WEO data because of a wider coverage. We have, however, estimated our regressions with World Bank's *World Development Indicators* (WDI) data instead and find that our results are basically unchanged, while our sample is about 3% smaller.

[20] See Fig. 1 for the list of countries in the sample. We include all countries for which all data series are available for the studied time period.

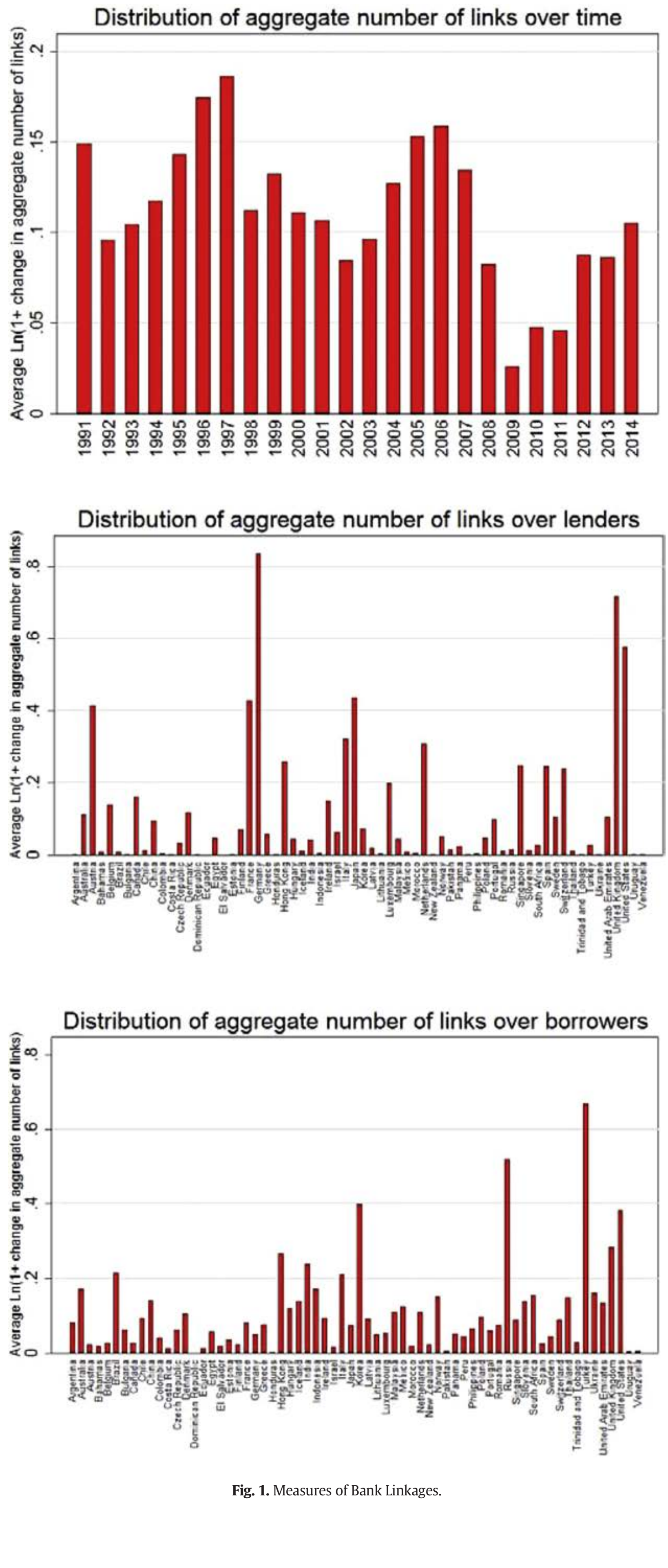

Fig. 1. Measures of Bank Linkages.

maximum likelihood method developed by Santos Silva and Tenreyro (2006) and Santos Silva and Tenreyro (2010).

We also use COMTRADE to obtain industry-level trade data at the 4-digit SITC (Rev.2) level of aggregation. We pair these data with the Rauch (1999) index of product differentiation.[21] These classifications sort SITC codes into three categories of goods: those traded on international exchanges, those with reference prices—both considered homogeneous goods, and differentiated goods for which branding information precludes them from being traded on exchanges or reference priced.

4.2. Bank linkages

We obtain deal-level data on syndicated international and domestic bank loans from Dealogic's Loan Analytics database (previously known as Loanware).[22] As our goal is to capture bank-to-bank lending activity, we obtain data on all loans extended to public and private sector banks between January 1, 1990 and December 31, 2014. There are three main reasons why we use these data: first, syndicated loans, as opposed to overnight loans, have a long maturity (median maturity in our data is about 3.5 years) and therefore are likely to establish relationships between borrowers and lenders[23]; second, unlike most bilateral loans, syndicated loans are large and are therefore likely to necessitate substantial amount of information acquisition by lenders; third, international syndicated loan markets are large and active (Cerutti et al., 2015; Ivashina and Scharfstein, 2010; De Haas and Van Horen, 2012), and data are readily available on a consistent basis across many countries.[24]

Ideally, we would like to ensure that each of the loans in our sample is a bank-to-bank loan, but the Dealogic database only allows us to identify borrower type (which we constrain to be either public or private sector banks); it does not allow us to place the same constraints on lenders, meaning that some of the lenders within a syndicate may not be banks.[25] Among the loans in our sample, over 60% are term credit, with the rest being revolving loans, CDs, and various credit facilities. We replicate syndicated loans as many times as there are lenders in the syndicate on the signing date of the loan. We link each borrower and each lender to a country on a locational basis.[26]

For each of the years in our sample, we construct a cumulative global banking network (GBN), where for each year t all loans between 1990 and t are included.[27] Thus, cumulative GBN expands every year through the addition of new connections as loans between bank pairs that have not engaged in lending previously. We rely on this cumulative network to measure the extent of newly formed bank linkages, a gross increase in bank linkages. By the end of our sample, we have 3393 banking institutions as lenders and 6018 banking institutions as borrowers.[28]

We compute a measure of bank linkages that only takes into account the number of direct connections between banks in country i and banks in country j. We refer to this measure as the aggregate number of linkages AL_{ij}, and it is simply the sum of bank pairs in countries i and j that are directly connected.[29] Note that the direction of connection matters: AL_{ij} is the number of banks in country i lending to banks in country j. Thus $AL_{ij} \neq AL_{ji}$. We use the log transformation of the one-period change in the aggregate number of linkages, which is the logarithm of a number of new connections formed between countries i and j:

$$al_{ijt} = \log\left(1 + \left(AL_{ijt} - AL_{ijt-1}\right)\right).$$

This one-period change measures the increase in number of linkages that is due to new connections that were formed in year t. In our regressions we use the first lag of this variable to measure the effect of new bank linkages formed between years $t-2$ and $t-1$ on exports in year t.[30] The distribution of bank linkages over years, over lenders, and over borrowers is shown in Fig. 1. Appendix Table A.3 provides summary statistics for the measure of bank linkages and its components.

By using al_{ijt-1} in our regression specification we make an implicit assumption that information benefits of bank linkage formed through new lending relationship only lasts one year. This is a judgment call on our part, because we are not aware of the literature that would guide us in this decision. It is quite possible that the effects are long-lasting. We could make another extreme assumption that the information effect of bank linkages lasts forever using AL_{ijt-1} in the regression instead. However, in this case we will be trying to explain fluctuating exports with a cumulative stock of linkages that is by construction only increasing, and slowly at that. Another alternative is to assume that information benefits of linkages last until the loans mature because of associated bank monitoring. While this assumption would be reasonable in general, in particular case of the syndicated loan market, only lead arrangers tend to be involved in monitoring and we find that the effects of bank linkages that are only restricted to lead arrangers are actually smaller.[31] Instead, we investigate empirically how long the effects of newly formed linkages on exports last in a special section following our baseline results.

4.3. Export risk

Our first three proxies are general country risk measures.[32] The first is the International Country Risk Guide (ICRG) index of political risk.

[21] We obtained the goods classification from Rauch's website: http://econweb.ucsd.edu/~jrauch/rauch_classification.html.

[22] See Miller and Chew (2011) for a detailed description of the syndicated loan market.

[23] In fact, anecdotal evidence suggests that establishing relationships is one of the main purposes of bank-to-bank syndicated lending on some occasions (see e.g., the media coverage of a syndicated loan to Turkish Garanti Bank in 2010, such as "Banks on Parade," IFR Turkey 2010.)

[24] Cerutti et al. (2015) discuss in detail the importance of syndicated loan market and show that the share of syndicated loans rose to as much as 30% of total cross-border bank lending in recent years. Moreover, according to Ivashina and Scharfstein (2010) over the last 30 years the syndicated loans market has evolved into a key vehicle through which banks lend to large corporations. Similarly, De Haas and Van Horen (2012) report that international syndicated loans made up 40% of all cross-border funding to firms in the U.S. and more than two thirds of cross-border flows to emerging markets in 2007. The interbank portion of this market is also large and active. In the late 1990s, syndicated bank loans extended to banks and reported in Loan Analytics amounted to over 30% of total bank claims on banks as reported by the BIS. This ratio fell to below 20% by the end of our sample as interbank lending ballooned prior to the global financial crisis. In 2007 alone, 4.7 trillion USD worth of syndicated loans extended to banks are reported in Loan Analytics.

[25] Upon detailed review of the lenders' names, we find that the non-bank lenders account for roughly 29% of all lenders in our sample and consist mostly of insurance companies and special purpose vehicles. We kept them in our sample because there was no way to systematically exclude them. The only way to limit the loans to those issued only by banks is to only consider term loans type A (Cerutti et al., 2015), which constitute less than 5% of the sample. We reconstructed the linkages measure using this limited list of loans and re-estimated our regressions. We find that the effect of this definition of linkages is about half the size of the our main results. For our benchmark specification of column (8) in Table 1, the corresponding coefficient is 0.047 with a P-value of 0.13.

[26] Mian (2006) shows that cultural and geographical distances between headquarters and local branches play an important role in lending practices.

[27] While Dealogic's data extends back to 1980, the loan coverage is substantially limited before 1990. The resulting network would be expanding due to expanding coverage, not increasing connectivity.

[28] We employ the Stata Graph Library (Miura, 2011) for network construction. For further detail see Hale (2012).

[29] We do not utilize loan amounts when constructing our banking network. If we construct a weighted network in which loan amounts proxy for the strength of bank linkages as in Michalski and Ors (2012), our estimated effect of bank linkages on export flows is positive and of about same magnitude as in our benchmark model, but not statistically significant. This suggests that loan amounts add noise to the data rather than information.

[30] We experimented with bank linkages from j to i and with higher order connections, but found that only direct linkages from i to j have an impact on exports from i to j.

[31] These results are available from the authors upon request.

[32] Summary statistics for all these measures are reported in Appendix Table A.3.

Table 1
Gravity regressions with aggregate linkages measure. benchmark.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
al_{ijt-1}	0.265*** (0.0209)	0.0719*** (0.0269)	0.249*** (0.0213)	0.226*** (0.0189)	0.221*** (0.0190)	0.143*** (0.0144)	0.133*** (0.0142)	0.0719*** (0.0101)	0.0719*** (0.0256)
N_i	0.910*** (0.0144)	0.875*** (0.0413)	0.915*** (0.0145)	0.611*** (0.0705)	0.301*** (0.0881)	0.646*** (0.0681)	0.336*** (0.0852)		
N_j	0.947*** (0.0156)	0.931*** (0.0292)	0.951*** (0.0156)	1.050*** (0.0730)	0.741*** (0.0853)	1.091*** (0.0733)	0.781*** (0.0857)		
Y_i	0.904*** (0.0145)	0.733*** (0.0322)	0.916*** (0.0146)	0.434*** (0.0233)	0.419*** (0.0294)	0.421*** (0.0233)	0.408*** (0.0294)		
Y_j	0.933*** (0.0145)	0.875*** (0.0444)	0.944*** (0.0147)	1.015*** (0.0249)	1.000*** (0.0278)	1.013*** (0.0246)	1.000*** (0.0272)		
Area_i	−0.0680*** (0.0118)	−0.0774*** (0.0250)	−0.0673*** (0.0118)	0.490*** (0.0638)	0.715*** (0.0787)				
Area_j	−0.124*** (0.0125)	−0.142*** (0.0260)	−0.123*** (0.0125)	−0.184*** (0.0682)	0.0405 (0.0769)				
Border	1.416*** (0.106)	0.889*** (0.102)	1.417*** (0.107)	1.124*** (0.0887)	1.124*** (0.0887)				
Language	0.491*** (0.0628)	0.443*** (0.0864)	0.493*** (0.0629)	0.404*** (0.0596)	0.404*** (0.0596)				
Colony	0.166 (0.103)	−0.386*** (0.118)	0.161 (0.103)	0.355*** (0.0808)	0.355*** (0.0807)				
Comm.col.	0.216 (0.226)	0.468 (0.338)	0.224 (0.226)	0.484** (0.190)	0.484** (0.190)				
Same cntry	0.733*** (0.208)	0.618** (0.241)	0.743*** (0.208)	0.677*** (0.175)	0.678*** (0.175)				
Distance$^{(a)}$	−0.121*** (0.004)	−0.114*** (0.011)	−0.120*** (0.004)	−0.163*** (0.004)	−0.163*** (0.004)				
Linear trend	−0.0114*** (0.00139)	0.0230*** (0.00202)							
Fixed effects	None	None	t	i and j	t, i and j	$i \times j$	t and $i \times j$	$i \times j$, $i \times t$, $j \times t$	$i \times j$, $i \times t$, $j \times t$
Observations	85,806	86,608	85,806	85,806	85,806	85,806	85,806	93,442	93,442
Adjusted R^2	0.761		0.764	0.832	0.834	0.341	0.363	0.954	0.954
Clusters	4032	4032	4032	4032	4032	4032	4032	4290	66; 66

Dependent variable $\log(1 + EX_{ijt})$, except in column (2) where it is EX_{ijt}. N is log population. Y is log per capita real GDP. al_{ijt}, measure changes in aggregate direct bank indirect linkages. Regressions in columns (1) and (2) also include controls for trend, geographical sizes of countries i and j and whether either country was a colony. Columns (1)–(4) also include distance between countries' capitals, indicator of whether the countries are contiguous, have common language, and whether they share colonial past. Linear regression except in column (2) where Poisson pseudo-maximum-likelihood is used following Santos Silva and Tenreyro (2010). Robust standard errors clustered on $i \times j$ in parentheses. In column (8) errors are two-way-clustered on i and j. $^{(a)}$Coefficient and standard error scaled up by a factor of 1000. *(P<0.10), **(P<0.05), ***(P<0.01).

This index has 12 components aimed at assessing the political stability of a country.[33] We do not have a prior on which of the components of the ICRG index would be the best proxy for the quality of contract enforcement in a given country. All of these components are highly correlated and would be difficult to interpret if included individually. For these reasons, we compute the first principle component, which explains 45% of variance in all the components for our sample and use this first principle component as a proxy for contract enforcement quality.[34] Higher values of this measure indicate lower country risk.

We expect that a sovereign's ability to repay its debts is correlated with payment enforcement costs, so the second measure we use is the sovereign credit rating histories from Standard and Poors (S&P). The S&P credit rating is alphabetical, but we assign numerical values to the ratings—with higher values indicating worse rating, or higher risk.[35] The third measure is even more crude—we split our sample into OECD and non-OECD countries, since OECD membership tends to be correlated with lower business risk environment.[36]

We also use a measure that is directly related to export insurance: export insurance cost measured as export insurance premia, obtained from the U.S. EXIM Bank. Per our request, the EXIM Bank compiled average insurance premia they charged on export insurance contracts by destination country, including the United States, for all the years in our sample. For the United States, data are only available starting in 1996.

Our final measure is at the industry level. The existing literature suggests that export risks are higher for goods that are more differentiated (Berkowitz et al., 2006; Nunn, 2007; Ranjan and Lee, 2007). If bank linkages help reduce export risk, this implies that for more differentiated goods the effects of bank linkages will be larger, as there are more risks to mitigate. We thus use the Rauch (1999) classification of industries into those with homogeneous, reference, and differentiated goods.

5. Effects of bank linkages on exports

5.1. Baseline result

Our benchmark results are presented in Table 1, where we test whether changes in our measure of new bank linkages formed during

[33] The components of ICRG's political risk index are described in Table A.4 in the Appendix. The components measure different aspects of political risk, ranging from government stability to risks to international investors.

[34] We computed the first three principal components (PC) for the 12 index components, those with eigenvalues over one, and found that only the first PC is useful for our analysis. The first PC loads positively on all the index components with loadings ranging from 0.12 on government stability to 0.37 for military in politics. The loading on investment profile, subcomponent of which is used in Antràs and Foley (2015), is 0.23. The first PC explains 45% of all variance and has eigenvalue of 5.3, while the next component only explains additional 12% and has eigenvalue of 1.5.

[35] We assign value of 1 for AAA+ rating, 2 to AAA rating, etc. Our results are robust to alternative codifications.

[36] We categorize as OECD countries only the 23 high-income economies that where members of this organization by 1990. All other current OECD members in our sample are categorized as non-OECD countries.

year $t-1$ affect exports in year t. In column (1) we include all common gravity regressors, but no fixed effects.[37] We find a positive correlation between newly formed bank linkages and the following year's exports. We test for potential specification problems by estimating a Poisson pseudo-maximum-likelihood model following Santos Silva and Tenreyro (2006) and Santos Silva and Tenreyro (2010). We find that, qualitatively, the results are very similar to the OLS specification, with the coefficient on bank linkages equal to 0.08 and significant at 1% level even when we cluster standard errors on country-pairs. This result is not surprising given the small number of zeros in our data (less than 10% as discussed in Subsection 4.1). For this reason we do not proceed with this specification in the rest of the paper.[38]

Because the correlation we find might be due to globalization trends, in column (3), we add time fixed effects. We find that the correlation is somewhat diminished, but is still positive and statistically significant. We worry, however, that the correlation can be explained by historically established country ties; therefore, we include country i and country j fixed effects in column (4) and (5), with and without year fixed effects. We find that the effect of bank linkages still remains statistically significant, although it is slightly smaller.

Time-invariant ties between countries, however, are better captured with country-pair fixed effects. We add this pair fixed effects in columns (6) and (7) of Table 1, with and without time fixed effects, respectively. We find that the within-pair portion of the correlation between newly formed bank linkages and trade in the following year is about half of the total correlation we observed in the previous columns. Nevertheless, it remains statistically significant.

An important potential source of spurious correlation is general economic and financial conditions in each country. Hale (2012) shows that bank linkages are less likely to form if a country is experiencing a recession or a banking crisis. Clearly, these conditions can also affect trade. Thus, in column (8) we estimate a fully saturated model, that is, we include country-pair, source-country-time, and destination-country-time fixed effects. These fixed effects capture any time-invariant country-pair characteristics that could lead to both higher trade volumes and more bank linkages between these countries as well as any time-varying country-specific dynamics that could account for target countries attracting both trade and bank lending and for source countries exporting both goods and bank funds. We find that the effect of bank linkages on trade remains positive and statistically significant, although it is further diminished. It remains statistically significant when we double-cluster standard errors on source and target countries as shown in column (9).[39]

Since all variables are in logs in the regressions, it is easy to interpret the magnitudes of the coefficients. Our most conservative results—those in columns (8) and (9)—suggest that doubling the change in intensity of bank linkages due to new banking connections is associated with a 7% increase in exports in the following year. In other words, when banks in country i extend loans to twice as many banks with whom they previously did not have a relationship in country j as in country k, other things being equal, exports from i to j increase in the following year by 7% more than exports from i to k. With the average number of new linkages being 1.1 and an standard deviation of 1.52,[40] doubling the number of new linkages is not an uncommon phenomenon—a two standard deviation change from the mean would correspond to tripling the number of new linkages and therefore translating into 10% increase in exports.

This impact is not very large, but it is not negligible either—the coefficient of variation of exports in the sample is about 30%. Note also that this is likely to be a lower bound on the effect of bank linkages on trade for two reasons: first, some of the true effect is absorbed along with the spurious correlation by fixed effects included in the regressions; second, some of the effects may manifest at longer time horizons than the one year lag effect we measure, a possibility we explore next; third, we only measure a subset of bank linkages, those established through syndicated bank lending, with other types of bank linkages possibly correlated with syndicated lending. On the other hand, even with a fully saturated model, we cannot fully rule out the effects of endogeneity and reverse causality, to which we turn after discussing dynamic effects.

5.2. Exploring dynamics

For our baseline results we made an assumption that new linkages formed by bank lending only impact exports in the following year. If the effect of linkages is more persistent, our baseline results might be suffering from the omitted variable bias. Given that we have no prior on how long the effect of new bank linkages may last, we test for different duration possibilities empirically. An obvious approach is to include distributed lags of new linkages. However, each lag we add would cut our sample by one year. Instead, we take the following approach.

Our data starts in 1990. We start cumulating bank linkages in 1990 as if there were no new linkages formed prior to this year. We compute cumulative linkages measures for up to 7 lags

$$cal_{ijt}^{s}=\sum_{\tau=1}^{s} al_{ij(t+1-\tau)},$$

so that $cal_{ijt}^{1}=al_{ijt}$. In this manner, we preserve the sample size, which makes it easier to compare model fit. We then estimate our baseline equation of column (9) of Table 1, replacing al_{ijt} with each of the cal_{ijt}^{s} measures individually. Fig. 2 shows coefficients on each cumulative linkage measure β_s, along with 5% and 95% confidence bounds, as well as the level of Bayesian Information Criterion (BIC) to compare model fit.[41] For each regression with $s>1$ the interpretation of the coefficient is an average effect of linkages formed in last s years. Since each new linkage has an effect that lasts s years, the total cumulative effect of each new linkage needs to be computed as $(1+\beta_s)^s-1$, which we also show on the chart.

We can make a few observations from these results. First, average effect of past linkages declines monotonically as we add more years, suggesting that there is a decay in the effect of new linkages over time. Second, at $s=7$ the coefficient is no longer statistically significant and the cumulative effect starts to decline after 5 years, which tells us that the effect of linkages lasts at most 6 years. Finally, the BIC is minimized at $s=3$, suggesting that export fluctuations are best explained with new linkages formed in three preceding years. The average effect of doubling the number of these bank linkages is 4.1% for a cumulative effect on total exports over the next three years amounting to 12.8%.

We explore the 3-lag specification a bit further, by estimating a distributed lag regression in order to estimate the extent of decay in the effect of a given bank linkage over time. Specifically, we estimate the same regression as in column (9) of Table 1 but include al_{ijt-1}, al_{ijt-2}, and

[37] In all regressions the estimating period is 1990–2013. Given that bank linkages enter lagged in the model, there are a total of $98{,}670=66\times65\times23$ possible observations. The actual number of country pairs in the data, however, is only 93,930 because there are countries in the sample that didn't become independent before 1990. In addition, there are some countries with no GDP or population data available for some of the early years. Thus, the regressions with those covariates are based on a smaller total number of observations.

[38] In fact, we find that the magnitude of our main effect from this specification is very similar to the one we find with a fully saturated model in columns (8) and (9) of Table 1.

[39] We report double-clustered results whenever we can. For some specifications, we do not have enough observations with ubiquitous fixed effects to also allow for double-clustering. With double clusters, there are only 66 clusters in each dimension, which means that within each of the 66 matrices all covariances need to be estimated. In specifications with fewer observations or with more fixed effects, we don't have the degrees of freedom to estimate all these covariances.

[40] As Appendix Table A.3 shows the average log change in linkages is 0.11 and the standard deviation is 0.42. Taking exponents, this means average one new bank linkage and standard deviation of 1.52 per year.

[41] Akaike Information Criterion produces the same results.

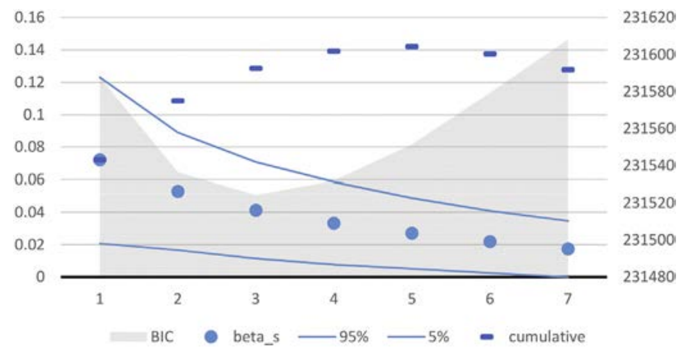

Fig. 2. Effects and fit of cumulative lags. Note: BIC is Bayesian Information Criterion depicted on the right axis. Each β_s is estimated from a separate regression specified in Section 4.2. Cumulative effect is a compounded effect over s years computed using the average annual effect given by β_s. 95% and 5% indicate confidence interval for β_s.

al_{ijt-3} as explanatory variables.

$$ex_{ijt} = \alpha_{ij} + \alpha_{it} + \alpha_{jt} + \beta_1 al_{ijt-1} + \beta_2 al_{ijt-2} + \beta_3 al_{ijt-3} + \varepsilon_{ijt}.$$

In this regression, we lose two years of data compared to our baseline specification.

We find the following estimates

$$ex_{ijt} = \alpha_{ij} + \alpha_{it} + \alpha_{jt} + 0.051^{***} al_{ijt-1} + 0.025^{**} al_{ijt-2} + 0.024^{*} al_{ijt-3} + \varepsilon_{ijt},$$

that is, there is quite a bit of a decay immediately following the first year of the impact — the coefficients on the second and third lags are half of that on the first lag.[42] Importantly, the sum of the three coefficients is 10%, not that different from the cumulative effect of the coefficient on cal^3_{ijt}.

Thus, we learn the following from our analysis of the dynamics. First, the effect of bank linkages on exports lasts about three years, and no longer than 6 years. Second, the impact effect is the strongest, with subsequent effects no larger than half of the impact effect. Third, doubling the number of bank linkages increases exports in the following years by 7 to 14% depending on the horizon.

With respect to our baseline results, our extreme assumption of the effect of linkages expiring after one year has two impacts on our interpretation of the magnitude of the effect. On the one hand, given positive auto-correlation in the effects of lagged linkages, omission of the linkages formed in the years prior to $t-1$ creates an upward bias in the coefficient on al_{ijt-1} in our baseline regression. That is, we are overestimating the impact effect. On the other hand, by not allowing for persistent effects, we under-estimate the total impact on new bank linkages, because we only allow for each newly formed bank linkage to affect export in one year that follows. Keeping this in mind, we proceed with our analysis relying on our baseline specification, for simplicity.[43]

5.3. Exploring reverse causality: trade diversion and placebo test

One logical concern with our identification strategy is reverse causality. Although Grosse and Goldberg (1991) suggests that banks may follow their clients to other countries, the "follow the customer" hypothesis has had limited empirical support (Seth et al., 1998). In the context of our study, reverse causality is plausible if banks operating in export markets anticipate an increase in exports and therefore choose to form connections with banks in import markets. We tested this hypothesis by regressing new bank linkages on lagged exports and found that lagged exports have a positive and statistically significant effect on new linkages; yet, this effect is 5 times smaller than our baseline result.[44] Under the assumption that export banks form these connections to facilitate trade and provide ways to alleviate export risk, the formation of bank linkages corresponds to the mechanism we have described in the paper; however, the small magnitude on lagged exports suggests that reverse causality is not the main driving factor for our baseline result.

To provide empirical evidence that reverse causality is not a primary concern for our analysis, we conduct a test assessing the presence of trade diversion. If the formation of bank linkages indeed increases exports from i to j, it is unlikely that this increase is entirely due to trade creation—some of it has to be trade diversion. Therefore, we should observe a decline in exports from i to countries that import similar set of goods from i to that of country j. Our story implies a decline in exports from i to k in response to new bank linkages formed between i and j, where ij and ik pairs have a similar trade composition.[45]

To conduct this test, we find a destination country k for each country pair ij in each year, so that exports from i to k closely match the 4-digit industry composition of exports from i to j. To do this, we rank industries by volume of exports for each country pair and year, compute the rank correlation, and pick the country pair with the highest correlation to ij in a given year. We then drop observations in which the highest correlation was below 0.4 (about 15% of the sample) and estimate the following regression:

$$ex_{ikt} = \alpha_{ij} + \alpha_{it} + \alpha_{jt} + \gamma al_{ijt-1} + \varepsilon_{ijt},$$

where we expect coefficient γ to be negative. Table 2 shows that this is indeed the case: the formation of new bank linkages between countries i and j reduces exports from i to country k that has most similar industrial composition of exports from i to j. The magnitude of the trade diversion effect is about half of the main effect of bank linkages.

We further test to see whether bank linkages formed by banks in country j lending to banks in country i affect exports from i to j. This serves as a placebo test for our analysis: if the effect was driven by spurious correlation between financial and trade flows, in a given country pair it would be as likely to show up in the reverse direction of flows as in our benchmark specification. Instead, we find that there is no effect.[46]

5.4. Assessing the export risk mechanism

In this section, we test whether the mechanism through which bank linkages affect exports is related to export risk. Using three different approaches, we find evidence supporting this hypothesis. First, we demonstrate that the effect of bank linkages is larger in magnitude for countries that have weaker contract enforcement and higher export risk using data on contract enforcement from the International Country Risk Guide, country credit ratings from S&P, and cost of export insurance from the U.S. Export-Imports Bank; we also leverage non-OECD membership, as a proxy for higher export risk. Second, we show that the effect of bank linkages is larger for differentiated goods, which have been shown in the literature to be associated with larger trade costs (Rauch, 1999) and may carry more export risk, relative to homogeneous goods; the result is robust to the inclusion of industry fixed effects. Finally, we show that bank linkages are positively associated with letters of credit, which are used as a means of reducing export risk. However, letters of

[42] This results is very robust to including any number of lags in the regression — there is always about 50% decline from first to second lag.

[43] All out results go through if we replace al_{ijt-1} with cal^3_{ijt} throughout our analysis, as shown in the Online Appendix.

[44] We provide the results of this reverse regression in the Online Appendix.

[45] To interpret this test as a test of causality, we have to assume that bank linkages between i and j are exogenous to exports from i to k. Of course, there is a possibility that banks anticipate trade diversion from k specifically to j and thus form linkages with banks in j. While we personally believe that this scenario is unlikely, there is no way for us to formally rule out this possibility. Regardless of the interpretation, we believe our trade diversion results are informative.

[46] Results available upon request.

Table 2
Gravity Regressions of exports to competing importers.

Cluster on:	$i \times j$	i and j
	(1)	(2)
al_{ikt-1}	−0.0345*** (0.0127)	−0.0345** (0.0152)
Observations	57,260	57,260
Adjusted R^2	0.855	0.855
Clusters	4021	66; 66

Dependent variable $\log(1+EX_{ikt})$. Full set of fixed effects ($i \times j, i \times t, j \times t$) included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages. Robust standard errors clustered as indicated are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

Table 3
Gravity regressions with aggregate linkages and risk measures.

Risk measure (ρ)	ICRG PC1	S&P rating	insurance cost
	(1)	(2)	(3)
al_{ijt-1}	0.0469*** (0.015)	−0.00888 (0.014)	−0.0313 (0.021)
$al_{ijt-1} \times \rho_{it}$	0.0102* (0.0058)	−0.00208 (0.0027)	
$al_{ijt-1} \times \rho_{jt}$	−0.017*** (0.0044)	0.00851*** (0.0015)	0.0625** (0.0307)
Observations	90,090	91,536	39,568
Adjusted R^2	0.956	0.957	0.963
Clusters	4290	4160	1885

Dependent variable $\log(1+EX_{ijt})$. Full set of fixed effects ($i \times j, i \times t, j \times t$) included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages. ICRG PC1 is the first principal component of the ICRG indexes which loads positively on all its components—see footnote 34 for details. Higher ICRG index is associated with lower risk. S&P ratings are coded in a linear fashion with 1 corresponding to AAA+, 2 to AAA, etc. 21 to CCC-, and 28 to SD. Robust standard errors clustered on $i \times j$, are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

credit is not the only mechanism of risk reduction: after controlling for letters of credit, we still find that bank linkages matter.

5.4.1. Do bank linkages matter more for exports to riskier countries?

One implication of the export risk reduction mechanism is that bank linkages should be more important for countries in which contract enforcement institutions are generally worse. We use two widely employed measures of country risk—ICRG indexes (Berkowitz et al., 2006; Antràs and Foley, 2015) and S&P sovereign credit ratings. These measures do not necessarily reflect export risk directly, but are widely accepted measures of overall contract enforcement and creditworthiness of the countries, so we expect them to be positively correlated with export risk. Because main effects of country risk variables are absorbed by country-time fixed effects, we only include lagged change in bank linkages as well as interactions of these changes with risk measures for source and target countries. The results are reported in columns (1) and (2) of Table 3, for ICRG index and sovereign credit rating, respectively.

We find that higher country risk (lower ICRG index or higher S&P risk measure) of the importer, country j, makes bank linkages more important for trade. This is consistent with our hypothesis—if bank linkages help alleviate export risk, they will have higher impact on exports to countries with higher risk. In fact, for exports to countries with good credit rating or high ICRG score, bank linkages don't matter at all. Specifically, F-tests show that for countries with ICRG index slightly above average and higher as well as for countries with sovereign credit rating better than A+, there is no statistical effect of bank linkages on trade. In contrast, bank linkages matter twice as much as for the full sample if the ICRG score is two standard deviations below the mean or if the credit rating is worse than BBB-. As expected, the country risk measure of the exporter does not affect the importance of bank linkages for trade. These results are consistent with the paper by Olsen (2015), which finds that only importer's risk characteristics matter for international trade.

We next turn to more direct measures of export risk. In column (3) of Table 3 we include interactions of export insurance premium (cost of export insurance) with our measure of bank linkages. Data on export insurance premia were obtained from the U.S. EXIM bank and are average premiums charged on actual export insurance contracts. We expect that higher premia are associated with higher export risk and therefore will make bank linkages more important. Because this variable comes specifically from insurance contracts on exports by importing country, we do not include its interaction with exporting country.[47] Our sample is reduced substantially here because export insurance premia are only available for a limited number of countries. We find, consistent with our previous analysis, that higher risk of the importers is associated with a bigger role of bank linkages in trade. F-tests show that bank linkages have a statistically significant effect on exports to countries where export premia exceed the average by more than 0.86 of standard deviation.

As an additional test, we follow the simple approach of splitting importers and exporters into groups by OECD membership, since by all measures of the rule of law, contract enforcement and the like, OECD countries score on average much better than non-OECD countries (Hall and Jones, 1999; Rodrik et al., 2004). While this is a crude way of proxying for export risk, OECD membership status has an advantage of being completely predetermined and unaffected by subsequent changes in bank linkages or trade. The results are presented in Table 4. We find that bank linkages are only important for exports to non-OECD countries and are twice as important for country pairs in which the exporter is also a non-OECD country, compared to pairs in which exporter is an OECD country.[48]

5.4.2. Do bank linkages matter more for exports of riskier goods?

As previously discussed, the existing literature suggests that export risks are higher for goods that are more differentiated (Berkowitz et al., 2006; Nunn, 2007; Ranjan and Lee, 2007). If bank linkages help reduce export risk, this implies that for more differentiated goods the effects of bank linkages will be larger because there are more risks to mitigate. We test this hypothesis by estimating our benchmark regressions for exports of differentiated, exchange-traded, and reference-priced goods, using COMTRADE data at the 4-digit SITC level sorted into Rauch (1999) categories.

The results are presented in Table 5. We find that the coefficient on bank linkages is small and marginally statistically significant for homogeneous goods traded on exchanges. For reference goods (goods with prices listed in reference catalogs), the effect is twice as large, and for differentiated goods it twice as large as for reference goods. Thus, bank linkages are more important for exports of differentiated goods, which is consistent with the interpretations of our results as bank linkages reducing export risk.[49]

All the above results are consistent with the mechanism we propose: bank linkages contribute to trade growth by reducing export risk. While they do not prove that the effect of bank linkages on trade is causal, one would be hard pressed to find an alternative story that would be consistent with all the evidence presented. Nevertheless, we are concerned about country-pair dynamics that could lead to both, growing trade

[47] If we do include it, we find that insurance premia of exporters do not matter.

[48] Another proxy for contract enforcement could be a legal origin of a country—that is common vs. civil law. Antràs and Foley (2015) show that in countries with legal origin other than common law cash-in-advance is more likely to be used. Consistent with their result, we find that bank linkages matter more for both exporters and importers from common law countries, countries which are more likely to rely on bank services for their international trade, but this is only the case for country pairs with both exporters and importers classified as OECD countries.

[49] We must note that exports of differentiated goods might also involve higher upfront expenses and therefore require more bank funding, which would be an additional mechanism through which bank linkages may affect trade.

Table 4
Gravity regressions with aggregate linkages by country group.

	$OECD_{11}$	$OECD_{10}$	$OECD_{01}$	$OECD_{00}$
	(1)	(2)	(3)	(4)
al_{ijt-1}	−0.00404 (0.00689)	0.0649*** (0.0113)	−0.00593 (0.0350)	0.0982*** (0.0262)
Observations	10,864	21,469	21,469	39,640
Adjusted R^2	0.988	0.964	0.965	0.918
Clusters	506	989	989	1806

Dependent variable $\log(1+EX_{ijt})$. Full set of fixed effects ($i\times j$, $i\times t$, $j\times t$) included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages. $OECD_{ij}$ indicates OECD membership (0,1) of i and j countries in each pair and year. Robust standard errors clustered on $i\times j$ are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

Table 5
Gravity regressions with aggregate linkages measure by Rauch (1999) (liberal) category of exports.

	Homogeneous	Reference	Differentiated
	(1)	(2)	(3)
al_{ijt-1}	0.0114* (0.0063)	0.0230*** (0.0049)	0.0534*** (0.0065)
Observations	93,442	93,442	93,442
Adjusted R^2	0.916	0.953	0.965
Clusters	4290	4290	4290

Dependent variable $\log(1+EX_{ijt})$. Full set of fixed effects ($i\times j$, $i\times t$, $j\times t$) included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages. Robust standard errors clustered on $i\times j$ are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

and acceleration in the formation of bank linkages. Trade data, disaggregated by industry, allow us to address this concern.

In Table 6 we present the results of the regressions where the dependent variable is exports from country i to country j of industry k goods in year t. We control for industry and country-pair-year fixed effects, which absorb any time-varying country-pair variables including any omitted variables that may generate spurious correlation between trade and bank linkages as well as our measure of changes in bank linkages. Our variable of interest is the interaction between change in bank linkages and an indicator of differentiated goods—homogeneous goods being a baseline category. We find, consistent with our hypothesis and with results in Table 5, that bank linkages matter more for trade in differentiated goods than for homogeneous goods. Effects are statistically significant whether we cluster standard errors on country pairs or double cluster them on both source and target countries.

Tying these findings together, we find strong support for our conjecture that bank linkages increase exports by reducing export risk. Six separate tests of the mechanism point towards this conclusion. We rule out the possibility of common factors driving the correlation between bank linkages and trade by finding different effect of bank linkages for different categories of goods in the regression with country-pair-year fixed effects.

5.4.3. Do bank linkages matter after controlling for Letters of Credit?

One of the ways in which bank linkages may help reduce export risk is by facilitating the issuance and underwriting of Letters of Credit. As described by Niepmann and Schmidt-Eisenlohr (2017a,b), Letters of Credit provide insurance for trade transactions and the use of Letters of Credit is associated with export risk. In this section we test whether our measure of newly formed bank linkages is associated with Letters of Credit, and whether the effect of bank linkages on trade matters beyond its effect on Letters of Credit. Using data on foreign transactions of U.S. banks available through form FFIEC 009a, we compute total Letter of Credit (LC) exposure of U.S. banks vis-à-vis other countries for years 1991–2013. Since data are only available for the U.S., this part of our analysis is limited to bank linkages formed by U.S. banks lending to foreign banks and U.S. exports. The unit of observation is now target country-year, so we can only control for year and target country fixed effects.

We begin by demonstrating that the effect of bank linkages on trade is still present in this restricted sample, and is in fact substantially larger, as shown in column (1) of Table 6. We next show, as has been established in the literature, that there is a contemporaneous correlation between the use of Letters of Credit and exports from U.S, as shown in column (2): doubling the Letter of Credit exposure is associated with 27% higher exports.

In column (3) of Table 7, we demonstrate that LC exposures are associated with lagged increase in bank linkages with an elasticity of 19%. These results are prima facie evidence of the sequencing implied by our previous findings: bank linkages formed at time t in the interbank syndicate loans market are correlated with LC exposures at $t+1$.[50] We then construct predicted values for the LC exposures using the results of this regression. In column (4), we show that the portion of the LC exposures that is due to the increase in bank linkages is important in explaining exports—the coefficient on predicted exposures is positive and statistically significant with the coefficient very close to 1. Results from columns (3) and (4) indicate that bank linkages formed through syndicated loan market may lead to higher instance of LC issuance and therefore to more trade.

There are of course other ways through which bank linkages may influence trade, such as facilitating payments, providing information on creditworthiness of counterparty, etc. In fact, when we control for the LC exposures, the effect of change in bank linkages remains positive and statistically significant (although diminished in magnitude—see column (5) of Table 7). These other avenues, however, are not observed to researchers. Thus, the evidence in Table 7 provides support for only a portion of the mechanism by which bank linkages may help increase trade, but together with recent work on Letters of Credit discussed previously, it strongly supports our hypothesis that bank linkages help reduce export risk.

6. Robustness tests

We conduct two sets of robustness tests on our benchmark specification.[51] The first set of tests addresses concerns about influential observations and the second set assesses the robustness of our results to the inclusion of country-pair time-varying covariates.

6.1. Influential observations

In Table A.1. we provide estimates of our benchmark specification with a modified sample. The most important influential time period is that of the global financial crisis; in 2008–09, banking activity came to a standstill while trade collapsed. A number of papers, in fact, relate this trade collapse to the lack of access to financing.[52] To make sure that our results are not entirely driven by this episode, however, we estimate our regression only using the data up to 2007. We find that the effect of bank linkages on trade is slightly smaller than in the benchmark, but it is still positive and statistically significant (see column (1)).

There are also influential countries in our analytic sample. During our sample period, China's trade expanded dramatically as did its financial linkages with the rest of the world. We find, however, that our results are not driven by China, as shown in column (2) of Table A.1. Our results are also not driven by the U.S., the world banker

[50] This results is not surprising in light of Bharath et al. (2007) finding that prior lending relationships are conducive to future lending and underwriting relationships.

[51] Robustness tests of other regressions are available from the authors upon request.

[52] For example, Ahn et al. (2011) demonstrate that financial factors contributed to trade the collapse, Amiti and Weinstein (2011) show a causal relationship between the health of banks providing trade finance and exports, while Chor and Manova (2012) show that the trade collapse was more severe for firms with limited access to finance.

Table 6
Industry-level regressions with aggregate linkages measure interacted with Rauch (1999) (liberal) category of exports.

Fixed effects:	k, i^*j^*t	k, i^*j^*t	k, i^*j^*t	k, i^*j^*t	k^*i^*t, k^*j^*t	i^*j, k^*i^*t, k^*j^*t
Cluster on:	$i \times j$	i and j	$i \times j$	i and j	$i \times j$	$i \times j$
	(1)	(2)	(3)	(4)	(5)	(6)
$al_{ijt-1}\times$ Reference goods	0.0078*** (0.0009)	0.0078*** (0.0021)	0.0084*** (0.0012)	0.0084*** (0.0025)	0.013*** (0.0023)	0.0003 (0.00047)
$al_{ijt-1}\times$ Differentiated goods	0.0181*** (0.0019)	0.0181*** (0.0049)	0.0186*** (0.0022)	0.0186*** (0.0047)	0.019*** (0.003)	0.0063*** (0.00097)
Observations	61,483,292	61,483,292	17,651,401	17,651,401	61,483,292	61,483,292
Adjusted R^2	0.185	0.185	0.227	0.227	0.276	0.372
Clusters	4290	66; 66	4278	66; 66	4290	4290

Dependent variable $\log(1 + EX_{ijkt})$. k is industry. al_{ijt} measures changes in aggregate direct bank linkages. First two columns include all observations. Second two columns exclude zeros. Robust standard errors clustered as indicated are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

Table 7
Gravity regression with aggregate linkages and letter of credit exposures.

Dependent variable	$\log(1 + EX_{USjt})$	$\log(1 + EX_{USjt})$	$\log(1 + LC_{USjt})$	$\log(1 + EX_{USjt})$	$\log(1 + EX_{USjt})$
	(1)	(2)	(3)	(4)	(5)
al_{ijt-1}	0.180*** (0.057)		0.185*** (0.043)		0.141*** (0.0501)
$\log(1 + LC_{USjt})$		0.253*** (0.081)			0.210*** (0.073)
Predicted $\log(1 + LC_{USjt})$				0.973*** (0.309)	
Observations	1452	1452	1452	1452	1452
Adjusted R^2	0.740	0.741	0.825	0.740	0.742
Clusters	65	65	65	65	65

Dependent variable $\log(1 + EX_{ijt})$. LC is Letter of Credit exposures in real U.S. dollars. j and t fixed effects included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages. Robust standard errors clustered on j are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

(Gourinchas and Rey, 2007), as shown in column (3). Our benchmark sample includes two offshore centers: Bahamas and Trinidad and Tobago. Column (4) shows that dropping these islands from the sample does not affect our results.

As noted before, our benchmark models are estimated with a strictly balanced panel. When there is no reported trade data between country pairs, the observations are treated as zeros. In column (5) we show that the results are robust to dropping the zeros from the estimation.

6.2. Series definitions

Our baseline results are obtained based on imports data from COMTRADE. However, we also run robustness checks using exports data from the IMF's Direction of Trade Statistics database (DOTS), as COMTRADE statistics may have unreported data.[53] Although both DOTS and COMTRADE databases are based on underlying statistics reported by national agencies, there are discrepancies in coverage between the two databases. This is due, in part, to the IMF practice of using imputation methods in the DOTS database (see e.g., Hummels and Lugovskyy, 2006). This affects the distribution of observations that are zeros in the two databases.[54] As column (6) of Table A.1 shows, the results are qualitatively comparable to our benchmark that is based on aggregation of COMTRADE data, although they are smaller in magnitude.

Throughout the analysis we relied on the measure of newly formed bank linkages that included a substantial number of zeros. Not surprisingly, given that our data is formulated at country pair-year level, in over 90% of the cases, no new linkages at all are formed for a given country pair in a given year. A natural question to ask, therefore, is whether our result is driven by a presence of any number of new linkages, regardless of how many. To test for this, in column (7) of Table A.1, we replace our measure of newly formed linkages with a 0/1 indicator, which takes a value of 1 when any positive number of new linkages are formed between country i and country j in year $t-1$. We find that indeed, our results are driven by the differences between observations with and without new linkages. The effect of 0/1 indicator is in fact larger than that of a continuous variable.

Finally, there are two distinct types of lenders in loan syndicates: those that actively participate in the origination of the loan (lead arrangers) and other participants that passively contribute funds.[55] We have estimated our model for linkages only formed by lead arrangers and for linkages only formed by non lead arrangers and find that both have a significant and positive effect on exports, with lead-arranger linkages having an effect that is nearly twice as larger as for other participants.[56] This is consistent with our mechanism, since lead arrangers likely obtain much more information about borrower than other participants.

6.3. Including country-pair, time-varying covariates

We now assess the stability of our results to the inclusion of additional controls that may explain trade and that vary over time within country pairs.[57] We first describe the additional data used to conduct these tests; then we present and discuss the results.

6.3.1. Additional data for robustness tests

Regional trade agreements, GATT/WTO membership, and common currency. We use data on trade agreements and common currency for years 1990–2006 from Head and Mayer (2013). We complemented the data on regional and bilateral trade agreements and GATT/WTO membership for years 2007–2013 using the World Trade Organization's Regional

[53] DOTS data, unfortunately, are not available at the industry level.

[54] Fig. A.2 in the Appendix shows the distribution of the DOTS data for different years. It is evident that it has fewer zeros than the UN-COMTRADE data, especially in the early years of our sample period.

[55] Ivashina and Scharfstein (2010), among others, discuss the importance of this distinction.

[56] In the interest of space these results are not reported, but are available from the authors upon request.

[57] All country-time variables are absorbed by country-time fixed effects.

Trade Agreements Information System (RTA-IS).[58] We complemented the data on common currency by hand and based on information from the IMF. In our country sample this basically meant to update in the data the year of eurozone membership for countries that adopted the euro after 2006. We also corrected Head and Mayer (2013) data to include the adoption of the U.S. dollar by Ecuador and El Salvador in 1999 and 2000, respectively.

Financial crises. We obtained data on systemic banking crises, currency crises and sovereign debt crises from Laeven and Valencia (2012).

6.3.2. Results with country-pair, time-varying covariates

The tests that use these data are presented in Table A.2. In column (1) we control for whether both countries i and j are in a regional or bilateral trade agreement. We find that while regional trade agreements (RTAs) do indeed increase trade, controlling for them does not change the impact of bank linkages.[59] In our fully saturated model, we find that common currency does not have a statistically significant impact on trade (column (2)). Most importantly for our purposes, however, we find that controlling for common currency does not change the effect of bank linkages. Similarly, changes in bilateral exchange rate also do not have an effect on trade and controlling for them does not change the effect of bank linkages (column (3)).[60]

While individual countries' crises are controlled for by country-time fixed effects, we can expect dynamics of trade and bank lending to be different if both countries are experiencing the same crisis. In column (4) we control for such situations, separating crises into banking system, debt, and currency crises. We find that a combination of currency crises has a negative effect on trade and that these controls do not alter our findings.

When we include all these controls together (column (5)), with the exception of common currency and exchange rate, which did not have any effect, we continue to find that doubling bank linkages increases trade in the following year by about 5.6%. This effect remains statistically significant.

A number of recent papers show the importance of financial linkages in explaining trade (Manova, 2008; Ahn et al., 2011; Antràs and Foley, 2015; Amiti and Weinstein, 2011; Minetti and Zhu, 2011; Chor and Manova, 2012). For this reason, and to ensure that our main results are not driven by financial linkages, we include measures of stocks of bank claims from BIS to proxy for financial integration,[61] and flows of bank claims from BIS to proxy for trade credit availability.[62] We find that controlling for these proxies of financial linkages does not alter our results.[63]

7. Conclusion

We present evidence that when banks in a given country pair become more closely connected, trade between these two countries tends to increase in the following year by an economically and statistically significant amount. We find this result controlling for gravity variables as well as for a full set of fixed effects: exporter-year, importer-year, and country pair. Moreover, we find that bank linkages are more important for exports of more differentiated goods. This result is statistically significant even when we control for exporter-importer-year fixed effects that absorb all potential factors that might jointly drive dynamics of bank linkages and trade for a given country pair. In addition, we show evidence of trade diversion from countries with similar import composition to countries experiencing increased imports due to new bank linkages.

We conjecture that the mechanism for this effect of bank linkages is related to the role banks play in reducing export risk. We show a variety of tests that support this conjecture. In addition, we find that export payment guarantees that banks provide through letters of credit are indeed one of the ways, but not the only way, in which banks help reduce export risk.

Besides the provision of finance, there are a number of other ways in which banks can alleviate export risk, for which we cannot test given currently available data. Because banks are particularly good at providing information on creditworthiness of potential buyers, they may help reduce information asymmetries hindering international trade in the way that is similar to social and other information networks.[64] For example, bank linkages mean better information for banks about contract enforcement in the destination country. This implies that bank linkages result in better information of banks in country i about the probability of banks and firms in country j to honor financial contracts, better information about the value of collateral in country j, and knowledge of the best ways to seize collateral and assure payment in country j. All this may induce firms in country i to transact with firms in country j, particularly if they can work with country i's banks that are more familiar with risk levels and risk management in country j (because such banks can offer cheaper finance and/or insurance for exporting to j). Similarly, the presence of bank linkages between two countries proxies for a broader exposure of banks to destination countries and thus signals a level of trust and reputation between banks from the two countries. Cross-border bank linkages then may result in more international trade by making firms more willing and able to engage in cross-border trade.

Our results are important for a number of reasons: first, they show a yet unexplored way in which finance is related to trade; second, they shed light on a mechanism that gives rise to the border effect of trade that is so far not fully understood; third, they provide a mechanism which links financial integration to real integration; fourth, they demonstrate positive effects of bank linkages that in the wake of the global financial crisis seem to get much less attention in the literature than the dangers of such linkages.

In this study we took an aggregate approach to analyzing effects of bank linkages instead of firm-level or product-level analysis more common in modern empirical trade literature. This aggregate approach allows us to uncover patterns that are common for a larger set of countries and over an extended time period. Exact mechanisms underlying these patterns could be better addressed at a micro level and we hope our findings will encourage further research in this direction.

[58] The RTA-IS system can be accessed through the web, at http://rtais.wto.org/ui/PublicAllRTAList.aspx. We use the list of all regional and bilateral trade agreements in force as of December 2013. Following Head and Mayer (2013), two countries are coded as having an RTA if the two countries belong to a regional trade agreement or if they have a bilateral one. We considered only agreements into force for over six months of the year (thus, RTAs such as Chile-Japan that entered into force in September 2007 are coded as entering into force in 2008).

[59] To determine whether RTAs are an important omitted variable in the rest of our regressions, we have reestimated all regressions that do not have $i \times j \times t$ fixed effects with additional control for RTA indicator. We found that while RTA has a positive and significant effect on exports in majority of regressions, it does not have a material effect on our coefficient of interest. That is, an omitted variable bias resulting from not including RTA indicator is minimal. These results are reported in the Online Appendix.

[60] We also tested whether an indicator that both countries have fixed exchange rate has an effect and found that it did not and including it did not affect our results.

[61] Stocks of assets are accumulated over time and are commonly used in the literature as a measure of financial integration—see, for example, Imbs (2006), Kalemli-Ozcan et al. (2013a), and Kalemli-Ozcan et al. (2013b).

[62] We use restricted version of the locational BIS bank claims of i on j and vice versa and financial flows that are measured as valuation–adjusted changes in these bank claims.

[63] The results are available upon request.

[64] Bank linkages can be similar to social network linkages in that they may provide channels of information flows and help match sellers to buyers in different countries. On the importance of social and information networks see the early survey by Rauch (2001) and the recent papers by Combes et al. (2005) and Baston and Silva (2012).

Appendix A. Additional charts and tables

Table A.1
Robustness tests 1: sample, data source, specification.

	Year <2008	No China	No US	No Islands	No zeros	DOTS data	$al_{ijt-1} \in 0, 1$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
al_{ijt-1}	0.071*** (0.011)	0.069*** (0.010)	0.074*** (0.011)	0.065*** (0.010)	0.024*** (0.007)	0.025*** (0.005)	0.085*** (0.011)
Observations	67,702	90,536	90,536	87,676	89,119	93,169	93,442
Adjusted R^2	0.958	0.953	0.951	0.955	0.964	0.969	0.954
Clusters	4290	4160	4160	4032	4277	4288	4290

Dependent variable $\log(1 + EX_{ijt})$ except in column (7), where it is $\Delta \log(1 + EX_{ijt})$. Full set of fixed effects ($i \times j$, $i \times t$, $j \times t$) included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages, in column (8) it is set to 1 for any positive value and 0 0 otherwise. Robust standard errors two-way clustered on i and j are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

Table A.2
Robustness tests 2: time-varying pair-level controls.

	(1)	(2)	(3)	(4)	(5)
al_{ijt-1}	0.0708*** (0.0101)	0.0717*** (0.0101)	0.0650*** (0.00946)	0.0245*** (0.00481)	0.0558*** (0.00945)
i and j in common RTA	0.121*** (0.0213)				0.145*** (0.0220)
i and j have same currency		−0.0327 (0.0343)			
$\%\Delta E_{ij}$			0.0000901 (0.0000603)		
Bank crises in both i and j				0.0264* (0.0157)	0.0567** (0.0284)
Debt crises in both i and j				0.0530 (0.0786)	0.353 (0.349)
Currency crises in both i and j				−0.116 (0.0746)	−0.257*** (0.0710)
Observations	93,442	93,442	93,088	83,395	83,580
Adjusted R^2	0.954	0.954	0.955	0.971	0.958
Clusters	4290	4290	4290	4029	4029

Dependent variable $\log(1 + EX_{ijt})$. Full set of fixed effects ($i \times j$, $i \times t$, $j \times t$) included in all regressions. al_{ijt} measures changes in aggregate direct bank linkages. RTA is regional trade agreement. E_{ij} is the ij exchange rate, year average. bs_{ijt}, bs_{jit}, bf_{ijt}, bf_{jit} are measures of stocks and flows of bank claims from BIS. Robust standard errors two-way clustered on i and j are in parentheses. *(P<0.10), **(P<0.05), ***(P<0.01).

Table A.3
Summary statistics.

Variable	Obs	Mean	Std. Dev.	Min	Max
Country-pair-year level					
Log real exports	97,732	3.85	2.61	0	12.19
Bank linkages:					
Share of directly linked bank pairs	97,732	0.22	0.40	0	1
Number of bank pairs	97,732	6.11	33.4	0	1037
Number of banks directly linked *AL*	97,732	5.33	30.7	0	1037
Log change in number of direct links *al*	97,732	0.10	0.41	0	5.31
– in regression sample	93,442	0.11	0.42	0	5.31
Control variables (in regression sample)					
Regional trade agreement	93,442	0.23	0.42	0	1
Common currency	93,442	0.025	0.16	0	1
Percentage change in exchange rate	93,088	8.43	318	−0.999	16,312
Country-pair bank crisis	83,580	0.0055	0.074	0	1
Country-pair debt crisis	83,580	0.00005	0.007	0	1
Country-pair currency crisis	83,580	0.00014	0.012	0	1
Country-year level					
N_i	91,924	2.85	1.69	−1.36	7.22
Y_i	91,924	8.52	1.27	5.17	10.9
ICRG PC1	91,714	0.064	2.21	−7.30	4.03
S&P credit rating	91,989	9.36	5.39	2	28
Export insurance premium	39,633	0.58	0.20	0	1.79

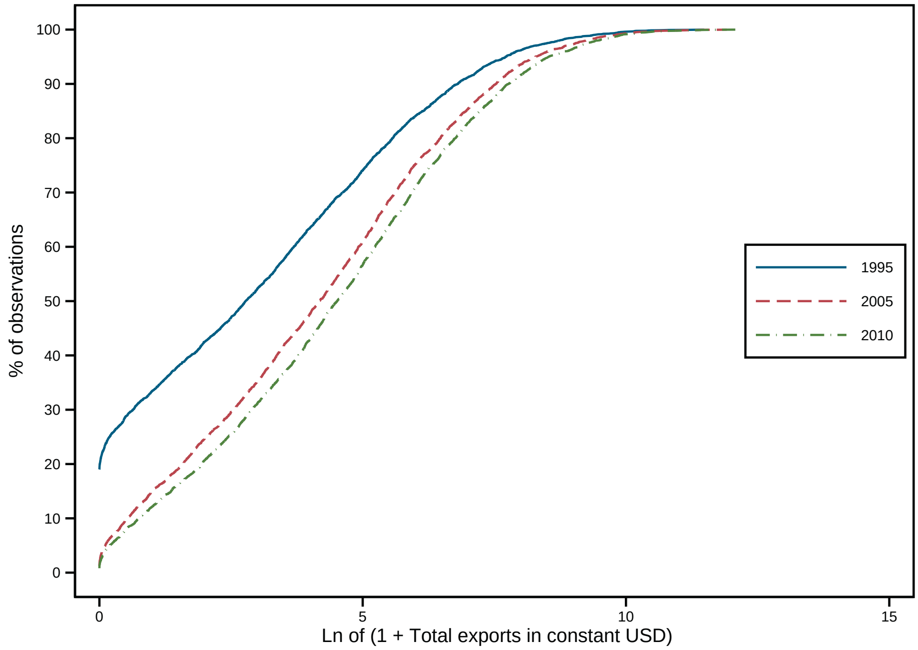

Fig. A.1. Distribution of Exports. UN-COMTRADE aggregate data.

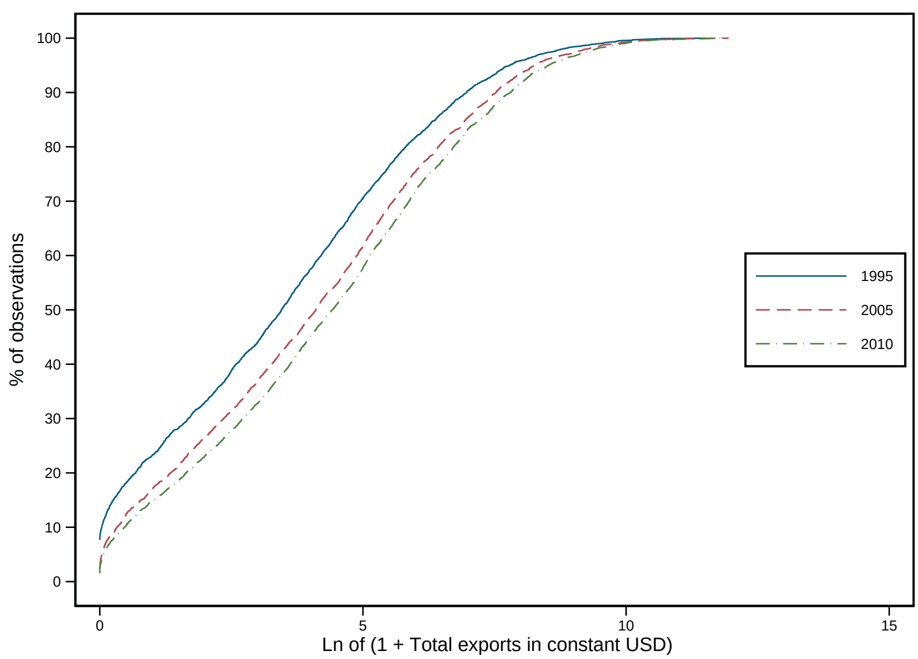

Fig. A.2. Distribution of Exports. IMF DOTS data.

Table A.4
Components of ICRG Political Risk Index.

Component	Description
Government Stability	This is an assessment both of the government's ability to carry out its declared program(s), and its ability to stay in office. The risk rating assigned is the sum of three subcomponents (Government Unity, Legislative Strength and Popular Support).
Socioeconomic Conditions	This is an assessment of the socioeconomic pressures at work in society that could constrain government action or fuel social dissatisfaction. The risk rating assigned is the sum of three subcomponents Unemployment, Consumer Confidence and Poverty.
Investment Profile	This is an assessment of factors affecting the risk to investment that are not covered by other political, economic and financial risk components. The risk rating assigned is the sum of three subcomponents Contract Viability/Expropriation, Profits Repatriation and Payment Delays.
Internal Conflict	This is an assessment of political violence in the country and its actual or potential impact on governance. The highest rating is given to those countries where there is no armed or civil opposition to the government and the government does not indulge in arbitrary violence, direct or indirect, against its own people. The lowest rating is given to a country embroiled in an on-going civil war. The risk rating assigned is the sum of three subcomponents Civil War/Coup Threat, Terrorism/Political Violence and Civil Disorder.
External Conflict	The external conflict measure is an assessment both of the risk to the incumbent government from foreign action, ranging from non-violent external pressure (diplomatic pressures, withholding of aid, trade restrictions, territorial disputes, sanctions, etc) to violent external pressure (cross-border conflicts to all-out war). External conflicts can adversely affect foreign business in many ways, ranging from restrictions on operations to trade and investment sanctions, to distortions in the allocation of economic resources, to violent change in the structure of society. The risk rating assigned is the sum of three subcomponents War, Cross-Border Conflict and Foreign Pressures.
Corruption	This is an assessment of corruption within the political system. Such corruption is a threat to foreign investment for several reasons: it distorts the economic and financial environment; it reduces the efficiency of government and business by enabling people to assume positions of power through patronage rather than ability; and, last but not least, introduces an inherent instability into the political process. The most common form of corruption met directly by business is financial corruption in the form of demands for special payments and bribes connected with import and export licenses, exchange controls, tax assessments, police protection, or loans. Such corruption can make it difficult to conduct business effectively, and in some cases may force the withdrawal or withholding of an investment.
Military in Politics	The military is not elected by anyone. Therefore, its involvement in politics, even at a peripheral level, is a diminution of democratic accountability. However, it also has other significant implications. The military might, for example, become involved in government because of an actual or created internal or external threat. Such a situation would imply the distortion of government policy in order to meet this threat, for example by increasing the defense budget at the expense of other budget allocations. In some countries, the threat of military take-over can force an elected government to change policy or cause its replacement by another government more amenable to the wishes of the military. A military takeover or threat of a takeover may also represent a high risk if it is an indication that the government is unable to function effectively and that the country therefore has an uneasy environment for foreign businesses. A full-scale military regime poses the greatest risk. Overall, lower risk ratings indicate a greater degree of military participation in politics and a higher level of political risk.
Religion in Politics	Religious tensions may stem from the domination of society and/or governance by a single religious group that seeks to replace civil law by religious law and to exclude other religions from the political and/or social process; the desire of a single religious group to dominate governance; the suppression of religious freedom; the desire of a religious group to express its own identity, separate from the country as a whole. The risk involved in these situations range from inexperienced people imposing inappropriate policies through civil dissent to civil war.
Law and Order	Law and Order are assessed separately, with each sub-component comprising zero to three points. The Law sub-component is an assessment of the strength and impartiality of the legal system, while the Order sub-component is an assessment of popular observance of the law.
Ethnic Tensions	This component is an assessment of the degree of tension within a country attributable to racial, nationality, or language divisions. Lower ratings are given to countries where racial and nationality tensions are high because opposing groups are intolerant and unwilling to compromise. Higher ratings are given to countries where tensions are minimal, even though such differences may still exist.
Democratic Accountability	This is a measure of how responsive government is to its people, on the basis that the less responsive it is, the more likely it is that the government will fall, peacefully in a democratic society, but possibly violently in a non-democratic one. The points in this component are awarded on the basis of the type of governance enjoyed by the country in question.
Bureaucracy Quality	The institutional strength and quality of the bureaucracy is another shock absorber that tends to minimize revisions of policy when governments change. Therefore, high points are given to countries where the bureaucracy has the strength and expertise to govern without drastic changes in policy or interruptions in government services.

Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jinteco.2018.08.006.

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