RESHAPING GLOBAL TRADE: THE IMMEDIATE AND LONG-RUN EFFECTS OF BANK FAILURES*

CHENZI XU

I show that a disruption to the financial sector can reshape the patterns of global trade for decades. I study the first modern global banking crisis originating in London in 1866 and collect archival loan records that link multinational banks headquartered there to their lending abroad. Countries exposed to bank failures in London immediately exported significantly less and did not recover their lost growth relative to unexposed places. Their market shares within each destination also remained significantly lower for four decades. Decomposing the persistent market-share losses shows that they primarily stem from lack of extensive-margin growth, as the financing shock caused importers to source more from new trade partnerships. Exporters producing more substitutable goods, those with little access to alternative forms of credit, and those trading with more distant partners experienced more persistent losses, consistent with the existence of sunk costs and the importance of finance for intermediating trade. *JEL Codes:* F14, G01, G21, N20.

I. INTRODUCTION

How much and for how long do financial crises affect the patterns of international trade? Trade relationships are highly stable and generally understood to be shaped by slow-moving forces of comparative advantage such as differences in technologies, endowments, and institutions. In most trade frameworks, transient shocks like financial crises can only have temporary effects. Yet models with multiple equilibria stress that large shocks can dislodge the economy from one equilibrium and leave

*I am grateful to the editor Nathan Nunn, five anonymous referees, Adrien Matray, and my committee Claudia Goldin, Sam Hanson, Matteo Maggiori, Kenneth Rogoff, and Jeremy Stein. Thanks to members of the audiences and discussants at numerous conferences and presentations. I am especially indebted to Margherita Orlando and the archives team at the Bank of England for their unflagging assistance, and to Reka Juhasz and Claudia Steinwender for sharing the scans of the *Lloyd's List* newspaper. This research was supported by grants from the Economic History Association, the History Project, the Lab for Economic Applications and Policy at Harvard University, and NSF Grant no. 2117003. All errors are my own.

The Quarterly Journal of Economics (2022), 1–55. https://doi.org/10.1093/qje/qjac016.
Advance Access publication on April 4, 2022.

it permanently in a different one.[1] Whether financial shocks can induce such an equilibrium shift remains unknown.

Establishing the long-run effect of financial shocks on the patterns of global trade is difficult for multiple reasons. First, economic fundamentals simultaneously impact exports and banking sector health. Second, even when it is possible to isolate an exogenous shock to the financial sector, data are usually limited to short-run outcomes in one country or require combining episodes from institutionally dissimilar countries and time periods. Ideally, one would be able to trace the long arm of history over uninterrupted decades in a setting where all countries are exposed to the same shock to their financial institutions.

I address these challenges by studying the 1866 banking crisis that originated in London but which disrupted short-term bank-intermediated financing in almost every country in the world. At the time, Britain was the center of the global financial system: British banks were the dominant providers of short-term credit and operated in countries that accounted for 98% of world exports.[2] The crisis propagated from London around the world in varying degrees based on the network of British banks. This variation in the intensity of the shock allows me to implement an event study difference-in-differences estimation that compares export volumes across locations that were more or less exposed to British bank failures, before and after 1866.

The banking crisis in this article has many attractive features for estimating the immediate to long-run effects of a financing shock on the patterns of trade around the world. First, it was caused by the unexpected failure of a fraudulent financial intermediary, the firm Overend & Gurney. This event triggered severe bank runs on London's deposit-issuing banks, and ultimately 16% of multinational banks headquartered in London failed and ceased domestic and foreign operations. Second, in contrast to much of the existing literature on long-run outcomes that relies primarily on cross-sectional evidence, this setting allows me to trace out the full dynamics over time. This allows me to study the evolution of trade patterns both before the crisis to show the lack of differential pretrends and afterward to examine the speed and

1. There is a literature on historical persistence in which multiple equilibria, institutional and cultural characteristics, and agglomeration forces can entrench the effects of a one-time shock in the economy (see Nunn 2014 for an overview).

2. Calculated using the locations and operations of British and non-British banks and values of exports across countries in 1865.

dynamics of recovery (or lack thereof). Third, locations' exposure to bank failures is continuously measured and based on observed bank activity, which has the advantage of being more precise than indicator variables of financial crises in most macro-oriented studies. Finally, the dominance of British banks makes it possible to compare the effect around the world using additional cross-country heterogeneity to better understand the mechanisms.

Pure randomness in bank failures delivers the exogenous variation sufficient for identification, and I provide narrative and quantitative evidence that the crisis followed a panic scenario where bank failures are mostly unrelated to observable measures of bank solvency. I show that the main observable characteristic correlated with a bank's failure is a public connection to the firm Overend & Gurney. Crucially, Overend & Gurney was not itself involved in trade-related activities, so this connection is unlikely to be correlated with export fundamentals in the banks' operating regions. Moreover, Overend & Gurney's failure was due to fraudulent mismanagement that was so well concealed that the firm had successfully "IPO'd" just nine months prior. Consistent with the environment of limited knowledge during the panic, there is also no relationship between the Overend connection and quantitative measures of bank health, liquidity, and risk taking, or with narrative accounts of the banks' investment opportunities and growth in their operating regions. This panic scenario distinguishes the setting here from other historical banking crises that were triggered by negative real economic shocks.

More formally, identification does not require pure bank failure randomness, nor does it require that the geographic distribution of bank subsidiary operations is random. It only requires randomness with respect to the characteristics of the locations where these banks were operating, to the extent that those characteristics affected exports. In a difference-in-differences setting, the identifiying assumption is that there are no simultaneous shocks to a location that cause its exports to decline and the banks operating there to fail (Borusyak, Hull, and Jaravel 2022). I provide a number of covariate balance tests of bank exposure to location characteristics, control for a large number of observables, and show graphical evidence for the lack of pretrends to support this identifying assumption.

My analysis necessitated constructing several new data sets of historical trade and financing activity around the world, both within and across countries. First, I measure cities' and

countries' exposures to British bank failures from over 11,000 handwritten archival loan records that represent the distribution of precrisis British bank lending relationships around the world. To my knowledge, these are the only data with global coverage of the dominant financial center's banking relationships in any time period. Second, for each bank, I collect balance sheets, shareholder meeting transcripts, and other narrative sources before and after the crisis. Third, for each country, I assemble and standardize a panel of bilateral export values spanning the period 1850–1914. I complement these country-level measures with the 1865 precrisis industry composition of exports from contemporary trade statistics reports. Fourth, in countries, I measure exporting activity with daily port-level ship movements from the *Lloyd's List* newspaper for the two-year window around the crisis.

I compare the exports of more versus less exposed locations before and after the crisis at the immediate and long-run horizons and at various levels of geographic aggregation. My first set of results shows that the financing shock immediately lowers export volumes. Countries exposed to a one standard deviation increase in bank failures have 8.5% lower exports one year later. I find very similar point estimates when using within-country, across-port variation, which allows me to include country-by-period fixed effects. Doing so nets out time-varying unobserved heterogeneity at the country level, including other changes in a country's trade costs, which could potentially drive the results. These results are robust to alternative specifications, count regression methods, and subsample restrictions.

In my second set of short-run results, I find that on the extensive margin, exporters more exposed to the shock have fewer trade partners afterward and are less likely to engage in international trade. These differences are due to a lower propensity to start trading as opposed to an increase in the likelihood of stopping.

My third set of results shows that the export losses are highly persistent in the long run, both in the aggregate of total exports and in terms of exporter market shares within destinations. Using the full panel of bilateral trade data allows me to control for demand shocks with importer-by-time fixed effects and include the host of measures of resistance to trade between countries that are standard in empirical gravity frameworks. This specification has a straightforward reduced-form interpretation as the relative market share difference between more versus less exposed exporters within the average destination each year. In the baseline

estimation, I find that importers bought on average 18% less each year from exporters with a one standard deviation higher exposure to bank failures after the crisis, with effects significantly different from zero for four decades. There is little recovery, and the estimated hysteresis is robust to controlling for a wide variety of contemporary shocks and initial macroeconomic conditions. Simulated placebo shocks also fail to re-create these patterns.

The reduced-form estimation in the baseline provides an elasticity that captures the overall effect of the financing shock on trade flows after controlling for other observable factors that are known to affect trade. Although the institutional context indicates that trade financing is likely an important channel for these effects, and in a series of exercises I directly control for other potential channels, it is impossible to fully rule out other mechanisms.

By making the stronger assumption that the financing shock acts as a pure change to trade costs, it is possible to estimate a trade cost elasticity based on structural gravity. This approach arises from many classes of microfounded trade models and shows that any changes in direct trade costs between two countries also affect prices in general equilibrium, which in turn indirectly affect trade flows for all countries through multilateral resistance terms (e.g., Anderson and Van Wincoop 2003; Head and Mayer 2014). The estimated coefficients have the advantage of being theoretically grounded, directly comparable to other trade cost shocks in the literature, and a necessary input for calculating welfare effects.

I follow the procedure in Baier and Bergstrand (2009) implemented by Berger et al. (2013) that uses a first-order log-linear approximation of changes in multilateral resistance to estimate the market share effects under this assumption. I find an average postcrisis elasticity of −1.88 (standard error 0.84) that is 37% larger in magnitude than the reduced form (although the 95% confidence intervals include the reduced-form estimates). Taken at face value, the larger magnitude from this approach relative to the reduced-form elasticity indicates that the general-equilibrium effects produced by the trade cost shock amplify the direct effect of financial shocks on trade. However, given that the structurally motivated elasticity requires the strong interpretation that bank failures only affected trade costs, I present the reduced-form results as the baseline.

The empirical finding of persistence motivates a conceptual framework featuring sunk costs of establishing relationships and

substitutability across exporters. The main assumptions of the framework follow from the details of the institutional context, which features highly substitutable goods (commodities trade), high sunk costs (slow communication), and intense competition (rapidly expanding trade networks in the 1860s and 1870s). The framework predicts long-run market share losses for exporters even holding demand constant. These effects come from importers expanding their trade relationships on the extensive margin once the financing shock raises their existing exporters' prices. Empirically, I show that importers whose trade partners were more exposed to the shock formed more new relationships, resulting in a lower share of their total imports coming from preexisting relationships. This extensive-margin growth by importers map onto the market share losses by exposed exporters in the baseline specification.

When looking at whether exposure to bank failures affected countries' imports, I find no effect. This corroborates the evidence that these international banks were primarily financing exporting activity, that is, the supply side of trade, but had no role in financing consumption. Therefore, unlike in 2008–9, the relative reduction in trade did not appear to be demand-driven but was a supply shock.

In the final section of the article, I provide evidence that trade patterns changed in a manner consistent with the financing shock affecting countries' costs of exporting, likely through a trade-financing channel. First, exporters that were likely to provide similar goods benefited when their competitors were exposed to the shock. This effect is true both across countries (where countries in the same geographical region produce and export similar bundles of goods) and within countries (where ports in the same country ship similar bundles of goods). In both cases, after controlling for a country or port's own exposure to bank failures, the average exposure of their neighboring competitors positively benefit their own exports.

Second, exporters whose exposure to bank failures are likely to be dampened by access to alternative sources of financing during the shock had lower losses. Third, relationships that likely relied less on trade finance, such as those that were physically closer, experience less persistent losses than more distant relationships.

I conclude by benchmarking the estimated effects against those available in the literature, which primarily focus on

short-term effects, and I show that these losses at short horizons are very similar to those due to other barriers to trade and for other types of crises.

This article contributes to the literature providing empirical evidence of the hysteretic effects of a temporary shock. Multiple equilibria are theoretically possible in many contexts, and the work by Davis and Weinstein (2002, 2008), Redding, Sturm, and Wolf (2011), and Bleakley and Lin (2012) provides early empirical evidence from various settings showing both recovery and persistence in urban agglomeration.[3] With respect to international trade, Baldwin (1988, 1990) and Baldwin and Krugman (1989) provide theoretical results of persistence arising from sunk costs. Empirically, firms' and countries' history of exporting predicts contemporary trade patterns (Eichengreen and Irwin 1998; Bernard and Jensen 2004) and sunk costs appear to be large (Roberts and Tybout 1997), but none of these provide direct evidence from an exogenous shock or guidance on how long these effects can last. This article also highlights a separate mechanism showing how the advantage of uninterrupted financial access while integrating into world markets during a period of globalization affects cross-country trade patterns and could be viewed as a critical juncture for generating first-mover advantages (Krugman 1991; Allen and Donaldson 2020).

It also relates to the macroeconomic literature that has found that financial crises have particularly large effects on many components of the economy, such as output, consumption, and employment (Barro and Ursúa 2008; Reinhart and Rogoff 2009; Chodorow-Reich 2014). Cerra and Saxena (2008, 2017) show that GDP dynamics following financial shocks (and crises more generally) do not recover at all, which relates to a classic empirical business cycle literature that recessions are not simply temporary cyclical events but have a highly persistent component (Blanchard and Summers 1986; Campbell and Mankiw 1987).

In the modern economy, credit conditions in peripheral countries have been found to be disproportionately associated with capital flows from the current global financial center (Eichengreen and Rose 2004; Maggiori 2017). Global banks and an

3. Related papers showing persistent effects of temporary historical events include Nunn (2008) on the African slave trade, Dell (2010) on forced-labor institutions in Peru, Sequeira, Nunn, and Qian (2020) on the age of mass migration in the United States for development, Juhász (2018) on the Napoleonic blockade for industrial growth, and Hanlon (2015) on the U.S. Civil War for innovation.

international reserve currency can transmit financial conditions in the core to the periphery, thereby amplifying international credit cycles (Goldberg and Tille 2009; Amiti, McGuire, and Weinstein 2019). The setting of a major shock to the pre-World War I global hegemon in this article illustrates how conditions in the dominant financial market affect real activity globally, particularly in sectors dependent on external capital flows.

Finally, this article speaks to the debate on the role of finance in trade. Many studies use cross-industry variation in external finance dependence and measure a country or firm's access to finance as a static source of comparative advantage, finding that financial constraints differentially affect exports for countries or firms specializing in financially dependent sectors (e.g., Manova 2013; Muûls 2015; Iacovone et al. 2019; Beaumont and Lenoir 2019). The trade response to financial shocks also gained prominence following the Great Trade Collapse of 2008, and while some studies find financial conditions to be a first-order determinant of trade flows (Chor and Manova 2012), others attribute the majority of the decline to demand and inventory (Alessandria, Kaboski, and Midrigan 2010; Bricongne et al. 2012; Eaton et al. 2016). I provide causal evidence using bank-level variation as in Amiti and Weinstein (2011) and Paravisini et al. (2014) and extend the analysis to every country over much longer periods.

The article is organized as follows: the next section discusses the historical context, Section III discusses the historical data sources, and Section IV describes the identification strategy. Section V summarizes the conceptual framework and reports the main results, and Section VI provides evidence on the source of persistence and heterogeneous effects. Section VII concludes.

II. Historical Context

II.A. Trade Finance and British Banking Dominance

Contractual and information frictions were a major barrier to establishing international trading relationships in the nineteenth century (Reber 1979), just as they are today. The long lag between the initial shipment by exporters, the receipt of goods by importers, and their final sale to consumers means that purchase and payment are staggered, and there is room for default on both sides. Contractual terms over quantities or quality may be difficult to enforce when the exporter is

risky, which is particularly likely for exporters in countries of low institutional quality or in new, riskier markets (e.g., Schmidt-Eisenlohr 2013; Antràs and Foley 2015). In that case, importers are unwilling to directly finance exporters through cash-in-advance payments, raising exporters' working capital costs.[4] Information frictions were also high historically, which further impeded trade (Steinwender 2018).

Banks were well positioned to overcome these frictions because they operated locally, which gave them superior knowledge of an exporting firm's risk and allowed them to take collateral, often in the form of goods shipped.[5] Their role in learning about exporters while providing them with short-term financing means that they stimulated international trade both by easing contractual frictions and by facilitating costly information flows.[6] Their business model also benefited from a form of exorbitant privilege due to the pound sterling's centrality: banks paid low rates on domestic liabilities (deposits) in the largest capital pool in the world and received high rates on their foreign assets (trade finance) abroad.[7] These structural advantages stemming from the London connection contributed to British banking dominance and global reach such that by 1865 these banks operated in almost every country and well beyond the British empire.

4. Contemporary nineteenth-century accounts indicate that uncertainty over payments made it difficult for exporters to operate (Reber 1979, 75).

5. For example, the Bank of London and the River Plate "attempted to assess the credit standing of its customers, although a good deal of business was carried on through personal contacts and oral agreements. The board of directors of the bank sought to establish credit guidelines. It stipulated that no credit exceeding £20,000 should be given to any single person or firm. The bank evaluated the respectability and soundness of mercantile houses and curtailed credit when necessary. . . . Each credit case was worked out individually with the house, and the amount of credit extended depended on the bank's knowledge of the customer's reliability" (Reber 1979, 60–61).

6. They were not permitted to act as general commercial banks and invest in long-term, illiquid assets in their local markets abroad (Chapman 1984). For example, it was in the Chartered Bank of India, Australia and China's prospectus that it would be "[prohibited from] the making of advances on landed or other immovable Securities, or on growing crops."

7. The English and Swedish Bank described this business model in the following way in their shareholder meeting on January 15, 1867: "When the bank was formed it was intended to receive money in England on deposit at the ordinary rate, and lend it out in Sweden at the high rate which was paid there upon such transactions. . . . Money was cheap in England, but a very high rate could be obtained for it in Sweden."

The primary instrument for financing international trade were short-term, often collateralized loans called "banker's acceptances" or "bills of exchange."[8] Acceptances were IOUs written between a borrower (an exporting firm) and a creditor (in this case the British bank) in which the creditor "accepted" that the borrower would repay him in the future (usually after three to six months). This source of financing provided exporters with working capital costs during the duration of shipment. Contemporaries emphasized that British banks were not limited to funding trade with Britain and that in fact the "bill on London" was predominant even for trade that had no British counterparties.[9]

These features of the institutional context where banks primarily held short-term bills as assets and were usually explicitly prevented from making illiquid investments is the reason I interpret their presence as reflecting the supply of trade financing.[10]

A banker's acceptance also had the unique legal feature of being guaranteed by the acceptor, meaning that in the case of default by the original borrower, the bank was responsible for the debt. This bank guarantee transformed these instruments from bearing the idiosyncratic risk of the individual exporter into bearing the better known bank's credit risk instead. The bank absorbed the exporting firms' credit risk at the rate it deemed appropriate in its foreign offices. This risk transformation then allowed the bank to resell (discount) the debt to others. When bills were discounted in the London money market, each subsequent holder (endorser) also guaranteed the ultimate debt in turn.[11] This unique feature

8. Contemporaries sometimes distinguished the phrase "banker's acceptance" from the more general "bills of exchange" to emphasize that the former instrument was backed by a trustworthy financial institution. In this article, the terms are used interchangeably.

9. For example, "the bill on London enabled the banks. . . . to finance a large share of international trade regardless of whether that trade touched Britain's shores" (Orbell 2017, 8), and "wines from France, coffee from Brazil, sugar from the West Indies, and silk from Hong Kong were paid alike with bills on London" (Jenks 1927, 69).

10. However, it is impossible to rule out that they were not also providing other sorts of financing that indirectly affected international trade. Online Appendix C.1 provides more qualitative descriptions of these banks' businesses.

11. King (1936), describes it as: "a bill of exchange is therefore something more than an acknowledgement of a debt: it is a legally binding undertaking to pay the debt, which is guaranteed by all 'parties' to the bill–the acceptor, drawer, and endorser(s). It is, moreover, indisputable evidence that the debt exists, and is therefore an instrument upon which a holder can base a legal action, even against parties with whom he has no direct contractual relations" (xvi).

of joint liability protected the London money market from asymmetric information with bad bills knowingly traded and passed along.

The multinational banks in this study also had accounts at the Bank of England (BoE), and the BoE only discounted the bills of its own customers.[12] The BoE always held bills to maturity, so it would ultimately absorb the losses if the original borrower (the exporting firm), the original lender (the British bank), and the previous endorsers of the bill all defaulted. Because the BoE was still a firm whose banking operations profited its shareholders, it strictly monitored the quality of its assets.[13] In addition to using a bill's history to ascertain its quality, the BoE monitored its customers' ability to meet their acceptance liabilities. These many layers of precautions in conjunction with joint liability meant that the bills discountable at the BoE were the safest short-term assets in financial markets with banks unable to strategically offload bad bills at the BoE.[14]

Bills of exchange originating abroad to finance international trade were remitted to London, where the demand for short-term, safe, liquid assets fueled the second half of a bill's life cycle in the world's largest money market.[15] Discounting the bills from abroad allowed the head office to supply the foreign offices with

12. The BoE used a double-entry accounting system, and all bills that went to the BoE's discount window (whether they were successfully discounted or not) had their attributes recorded in multiple ledgers, including ledgers tracking the obligations of the acceptors and the liquidity needs of the discounters. This system makes it straightforward to verify that only customers with accounts were discounters or acceptors.

13. The gold standard was maintained by a completely separate set of operations, so bad debts in the Banking Department could not simply be inflated away by printing money in the Issue Department. There is a large literature on the history of the BoE's transition from a purely private entity to a modern central bank (e.g., Clapham 1945; Capie 2004; Kynaston 2020). However, the principle that Bagehot (1873) outlined that the BoE (and central banks more generally) would combat moral hazard by only discounting the highest quality collateral remained in place throughout the nineteenth and twentieth centuries.

14. Bignon, Flandreau, and Ugolini (2012) calculate the "amount at risk" on the BoE's balance sheet over three crises in the nineteenth century and show that the BoE was careful to limit this amount from any given lender. I also use the BoE's profit/loss statements to show that the bank did not suffer losses from its discount window following these crises.

15. The safety and liquidity features of these commercial bills made them the safe assets of this era, much as Treasury bills are today (Xu 2019).

fresh capital in return.[16] Online Appendix Figure C1 documents the full life cycle of a bill of exchange, and Online Appendix C.1 provides more detail.

II.B. London Banking Crisis of 1866

The 1866 crisis was the first modern global banking crisis and one of the most severe to ever affect the London money market, during which 21 out of 128 multinational banks headquartered in London stopped operations.

The crisis was caused by the unanticipated bankruptcy of the firm Overend & Gurney, the largest and most prestigious interbank lender in the city of London. Its business as an intermediary, strictly speaking, was restricted to the safe business of buying and selling liquid, short-term bills of exchange from and to London banks. It did not lend long-term on illiquid assets, and it had no overseas operations. It did not finance trade and had no exposure to overseas exports markets.

Overend's business had been built over decades by earlier generations of partners such that by the mid-nineteenth century, it was one of the most reputable firms in London. In the early 1860s, a younger generation of partners took over the firm and delegated the business to "wily sycophants," who mismanaged the firm's assets with speculative and illiquid investments that quickly began to fail (King 1936, 246). The true state of affairs was not known to the public, and in July 1865 the firm successfully raised equity and converted to a publicly listed joint-stock firm in a gamble for resurrection. *Banker's Magazine*, a leading financial market publication, fully endorsed the firm and its equity issuance. Soon after, Overend's shares were trading at almost a 100% premium (King 1936, 239). Yet the new capital was not sufficient, and less than one year later on May 10, Overend announced its bankruptcy.[17] After its failure, the shareholders

16. The Eastern Exchange Bank described this cycle of financing between its headquarters in London and its office in Alexandria during its biannual meeting on March 1, 1865, the following way: "The bills sent home from Alexandria for correction had to be re-discounted in the Liverpool and London market at the current rates, so as to turn them into gold and send them out to Alexandria to be employed in fresh operations."

17. The proximate cause for bankruptcy that necessitated the loan was a court decision that ruled that Overend & Gurney could not collect from a debtor (Sowerbutts, Schneebalg, and Hubert 2016). Overend's directors had approached the BoE for a private loan, but the BoE declined to extend credit, claiming that

sued the directors for fraudulently misleading them about the true state of affairs in the prospectus. I provide details on the company's history, evidence on shareholders' ignorance of the true state of affairs, arguments presented in the court case, and previous scholarship on Overend in Online Appendix C.2.

As in the collapse of Lehman Brothers in 2008, Overend's failure led to widespread panic and a flight to safety. *Banker's Magazine* wrote, "It is impossible to describe the terror and anxiety which took possession of men's minds... a run immediately commenced upon all the banks, the magnitude of which... can hardly be conceived."[18] The money market was completely frozen, and the BoE Discount Window was the only source of liquidity.[19] The BoE was constrained by the gold standard from freely printing notes, so it could only meet the liquidity demands with its own reserves, and the panic was fueled by concerns that the reserves would be drained.[20] Eventually the BoE obtained permission from the Exchequer to suspend the gold standard and to meet liquidity demands with unbacked notes if necessary, which ended the bank runs.[21]

The crisis in London was also widely reported around the world (see Online Appendix C.3.2) with the English-language

Overend was insolvent. However, the relationship between the BoE and Overend had been contentious since Overend staged a mini-run on the BoE in April 1860, and some scholars believe that this was the true reason their request was rejected (Sowerbutts, Schneebalg, and Hubert 2016).

18. Online Appendix C.3.1 provides many additional examples of contemporary newspaper reports of the crisis.

19. "It was impossible to sell either Consols or Exchequer bills, while jobbers in most other securities refused to deal throughout the day.... Open market discounts were unobtainable" (King 1936, 243).

20. The Bank Act of 1844 began the process of consolidating the money supply in the BoE and allowed it a limited fiduciary issue, after which all notes were backed 1:1 in gold. During panics, demand for liquidity quickly drew down the BoE's reserves, after which the gold constraint would bind.

21. The full text of the letters exchanged on May 11 are in Online Appendix C.2. In them, Chancellor of the Exchequer Gladstone emphasizes that his reason for allowing the suspension is because this crisis was purely financial and not a commercial crisis. Walter Bagehot, the editor of *The Economist*, blamed the severity of the crisis on the BoE's lack of communication about its true willingness to act as a lender of last resort. He wrote, "either shut the [BoE] at once... or lend freely, boldly, and so that the public will feel you mean to go on lending. To lend a great deal, and not give the public confidence that you will lend sufficiently and effectually, is the worst of all policies, but it is the policy now pursued" (Bagehot 1873, 31).

newspaper in Buenos Aires writing, "an alarming financial crisis has burst in England, threatening widespread misfortune... it is certain to affect all parts of the world in commercial relations" (The Standard 1866). In addition to the dislocation generated by the immediate panic, the BoE kept its discount rate at a punitive 10% for over three months afterward, which further hindered recovery by making it difficult for banks to recapitalize and was deeply criticized by contemporaries (Schneider 2021).[22]

Ultimately, 21 banks headquartered in London were forced to close or indefinitely suspend operations. Headquarter closures caused branches abroad to close immediately since they relied on the capital from London.[23]

III. Data

This article combines several newly collected and digitized historical data sets, and this section gives an overview of the most important data sets and variables constructed. I provide further detail in Online Appendix H.

III.A. Bank Characteristics

1. *Lending Precrisis.* The Bank of England kept detailed records of every transaction that occurred at its Discount Window.[24] I use the ledgers from 1865 to 1866 to build a data set of over 11,000 individual loans from the 128 banks that had

22. Schneider (2021, 14) writes, "The Bank's discount policy... in the general atmosphere of panic and suspicion, had the consequence of forcing even solvent houses to their knees."

23. For example, the Commercial Bank failed in London on May 15. The headquarters's telegraph to its Bombay offices arrived on May 24 and read, "This bank suspended. Cease all operations. Make no payments. Allow no transfers or sales." While these branches were directed by the local branch manager, who had wide latitude in daily decisions due to the communication lags with London, they relied on regular fresh injections of capital from London to operate. Unlike the modern context, where branches and subsidiaries have very different structures with implications for risk-sharing (Fillat, Garetto, and Smith 2018), the historical operations were a mix between the two: capital was shared, as in a branch system, but decisions were local, as in a subsidiary system.

24. I consider the bills discounted at the BoE during the crisis to be a representative sample of the universe of loans extended by British banks in locations around the world just before the crisis. I provide further explanation and evidence in Online Appendix C.5.

international operations in the year before the crisis.[25] All banks are headquartered in London and most had both domestic and international operations.

For each handwritten loan record, I document the bank that originated and guaranteed the loan, the city the loan was extended in, the amount, the bank that brought the bill in to be discounted, and the date it was brought to the BoE.[26] These data allow me to calculate the share of financing by each bank in each city before the crisis.

2. *Bank Health.* Individual bank failures were reported extensively in contemporary newspapers and recorded by the BoE in internal records. I gathered balance sheets and narrative evidence of the banks' risk-taking and financial health from transcripts of the biannual meetings of shareholders at the closest meeting before, during, and after the crisis for publicly traded banks. Names of the managers, directors, and partners of the banks were listed in financial almanacs, advertised in contemporary newspapers, and often mentioned in shareholder meetings.

III.B. Exports

1. *Port Level.* I measure shipping activity for ports outside the United Kingdom using the daily publications of the *Lloyd's List* newspaper for a two-year window around the crisis. *Lloyd's List* is unique for providing a within-year, within-country measure of exports for the whole world, which makes port-level analysis possible.[27] Drawbacks are that it does not report values

25. For the entries from 1866, I only include bills that originated before the crisis so that the shares are not calculated using any loans that may reflect a postcrisis reallocation of credit. For robustness, I can further restrict the banks' portfolios to only bills discounted in May 1866 (but originated before then) during the height of the panic when all banks were using the Discount Window. The results are very similar, and the full discussion is in Online Appendix C.5.

26. An example of a ledger page is shown in Online Appendix Figure B1a. Deciphering the handwriting was not trivial. When there was uncertainty about the city of origination, I looked for other loans extended to the same borrower to compare entries. I was able to identify the location and geocode 99.7% of the value of loans.

27. Although it is technically possible to digitize the *Lloyd's List* for the long-run analysis from 1850 to 1914, the scale of data collection required is beyond the scope of this article. An example of this source from September 5, 1866, is shown in Online Appendix Figure B1b.

of the goods onboard and is a British publication that may have a British bias, but there is a strong positive correlation between the number of ships leaving a country in a year and the total value of the country's exports, shown in Online Appendix Figure B2. I provide additional robustness checks for this concern about mismeasurement in Online Appendix G.1.5.

There are more than 400,000 unique shipping events that I digitized, geocoded, and aggregated to generate measures of exporting activity before and after the crisis.[28] The data set has two periods that aggregates shipping for the one year before and after May 1866.

I build several panels, each with two periods: the first aggregates total exports and number of destinations from each country; the second aggregates total exports and number of destinations from each port; and the third panel captures bilateral trade between origin ports and destination countries. In all of these, I restrict the set of ports to those active in both the pre- and postcrisis periods. I also build a data set that includes the ports that were ever active in the two periods, which allows me to measure entry and exit. Figure I, Panel B maps the precrisis distribution of exporting activity measured by the number of ships.

2. *Country Level.* I assemble and standardize the country-level panel of bilateral trade values for 1850–1914 from publicly available data sets of historical trade statistics plus my own contributions. I define countries across datasets as the smallest landmass unit that is consistently reported over all the years.

I collect data on the product-level composition of total exports by country for 1865 and assign them to two-digit SITC codes to capture the precrisis industrial composition.[29]

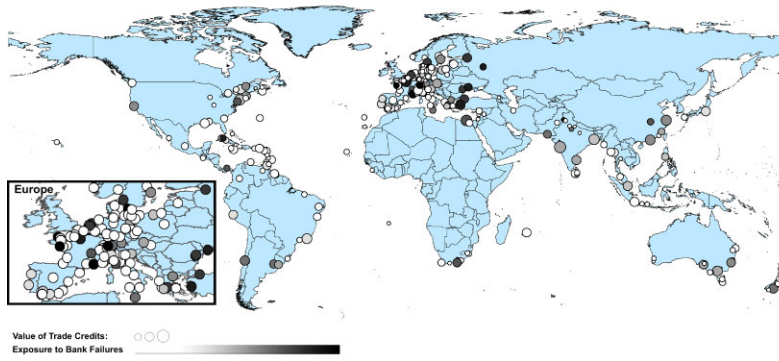
IV. Empirical Strategy

The goal of my empirical analysis is to estimate the causal relationship between the supply of financing in a location and

28. Origin ports and destinations are listed very precisely, so they are aggregated into larger units. Different parts of the same port were often named separately, and destinations are inconsistently listed as countries or cities. Ports within 10 kilometers of each other are aggregated into one unit and destinations are countries.

29. Historical data on bilateral trade flows disaggregated by industry and year is unavailable for most countries and has mostly not been systematically collected.

(A) British multinational bank lending and failures



(B) Port-level trade activity

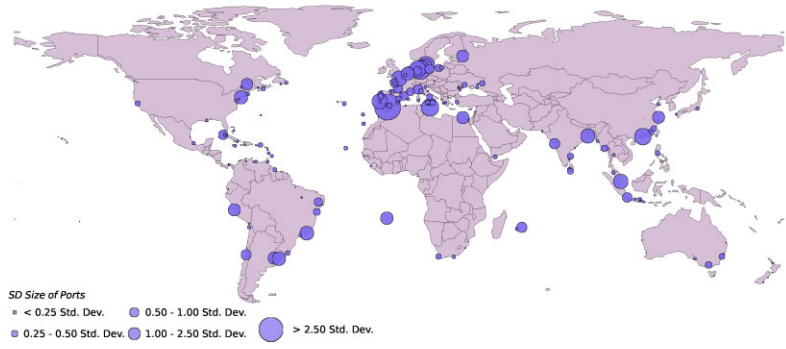


FIGURE I

Geography of Banking and Trade

Panel A maps the distribution of the city-level exposure to bank failures Fail_l. The size of the points denote the log value of total credit at each city and the color gradient denotes the exposure to failure, ranging from 0 to 1. Panel B maps the distribution of shipping activity at ports in the precrisis year. The size of the points denote the log number of ships leaving. Ports in the United Kingdom are not included. *Source: Lloyd's List*.

its exporting activity. I relate the log of exports EX_l at location l to the log of the amount of financing in a given period, $Finance_l$, measured by loan volumes:

$$\ln(EX_l) = \alpha + \gamma \ln(Finance_l) + \Gamma' X_l + \varepsilon_l. \tag{1}$$

Identifying γ from equation (1) is challenging for two reasons. First, observed financing is an equilibrium outcome that conflates

supply and demand, so places that demand less financing are also likely to have less trade. Equation (1) will therefore not satisfy the orthogonality conditions that $\mathrm{E}[Finance_l \varepsilon_l] = 0$ since ε_l includes the unobserved local economic conditions that are positively correlated with finance that biases γ upward. Second, there might be reverse causality: firms in locations that are already less productive can weaken their banks' balance sheets with nonperforming loans and cause those banks to contract their lending or even to fail.

I overcome these challenges by combining the unique data set of the cross section of multinational British bank lending with the institutional structure where branch offices depended directly on their headquarter's ability to provide capital. Banks whose headquarters in London failed due to the panic generate plausibly exogenous variation for their branch locations' exposure to bank failures and therefore to the supply of bank financing.

IV.A. Measuring the Exposure to Bank Failures

Bank shocks are captured by the binary variable $\mathbb{I}(Failure_b)$ that takes the value 1 if the bank failed in 1866 and 0 otherwise. A location l's dependence on each bank b is measured as bank b's share of financing in location l: $z_{lb} = \frac{Finance_{lb}}{Finance_l}$, measured in the precrisis period. These shares sum to 1 in each location. The dot product of these two terms gives each location's exposure to bank failure $Fail_l$:

$$Fail_l = \sum_b z_{lb,\,pre} \times \mathbb{I}(Failure_b). \tag{2}$$

$Fail_l$ takes the form of a Bartik instrument where the precrisis importance of each bank to a location ($z_{lb,\,pre}$) are the "shares," the bank failure rates are the "shocks," and financing ($Finance_l$) is the endogenous variable. $Fail_l$ is continuously measured from 0 to 1. I provide the derivation of the instrument in Online Appendix D and discuss instrument validity in Section IV.B.

I estimate the following reduced-form relationship between exposure to bank failures and log exports:

$$\ln(EX_{lt}) = \beta(Fail_l \times Post_t) + \Gamma' X_{lt} + \varepsilon_{lt}. \tag{3}$$

The coefficient β in equation (3) is the reduced-form semi-elasticity of the response of trade activity to British bank

TABLE I
SUMMARY STATISTICS: PORTS AND COUNTRIES

	Ports			Countries		
	Mean	Median	Std. dev.	Mean	Median	Std. dev.
Exposure to failed British banks	0.07	0.00	(0.19)	0.09	0.03	(0.14)
Exports	118.76	27.00	(215.53)	12.20	2.14	(31.89)
Share exports to United Kingdom	0.39	0.30	(0.34)	0.60	0.69	(0.38)
Destinations (no. countries)	7.63	5.00	(7.21)	3.85	2.00	(8.06)
Distance to destination (km k)	5.06	4.80	(3.31)	6.52	6.31	(3.51)
British banks	6.03	3.00	(7.54)	13.45	9.00	(13.82)
Non-British banks	0.45	0.00	(0.82)	1.26	0.00	(3.25)
Fraction in British Empire	0.34	0.00	(0.47)	0.33	0.00	(0.47)
N	289			55		

Notes: The table shows summary statistics from the port-level panel of shipping activity and the country-level panel of values of exports. All variables are measured at the end of 1865, before the crisis. "Exports" is measured by the number of ships departing for ports, and by the value of exports in millions of pounds sterling for countries. Share of exports to the United Kingdom is similarly calculated using the number of ships and values of exports.

failures in location l in the postcrisis ($Post_t$) period.[30] Note that estimating the reduced-form relationship means it is not possible to distinguish between the many different roles of banking activity, such as credit provision or risk assessment. Given the historical evidence of these banks' role as mostly providers of trade financing, this trade finance channel is likely important, but other forms of banking activity that affect exporters would also be affected by bank failures and captured by β.

Figure I, Panel A maps the geographic distribution of exposure to bank failures, $Fail_l$ at the city level. The size of the points measures the precrisis amount of British lending in the city, and the color denotes the bank failure share. This map shows within- and across-country variation in failure rates. Online Appendix Figure B4 plots the distributions of exposure across ports and countries and shows representation across the entire range. Table I reports the descriptive statistics for ports and

30. Data limitations prevent estimating the full first stage. I estimate a pseudo first stage with the available data in Online Appendix Table A1, which is representative of all banks (Online Appendix Table A2). I provide more details in Online Appendix A.

countries in 1865. The average port had 119 ships leaving in the precrisis period and 7 percentage points exposure to failed banks. The average country exported £12.2 million and was exposed to 9 percentage points of bank failures.

IV.B. Validity of the Reduced-Form Estimation

The reduced-form relationship in equation (3) will identify the effect of contractions in bank finance on exports if $Fail_l$ satisfies the standard exclusion restriction: $\mathrm{E}[Fail_l \varepsilon_l] = \mathrm{E}[\sum_b z_{lb} \mathbb{I}(Failure_b) \varepsilon_l] = 0$. The equation is immediately satisfied if bank failures are randomly assigned, but it does not require it.

The less restrictive requirement is that the instrument will be valid if the bank-level shocks are uncorrelated with the average location-level characteristics that determine exporting activity (Borusyak, Hull, and Jaravel 2022). The identifying assumption is that banks did not sort to locations such that location characteristics were correlated with both failures of the British multinational banks operating there and declines in exports in 1866.[31] One example of problematic sorting would be if banks that failed systematically operated in locations that experienced a boom precrisis and a bust after 1866. Declines in exports and failures of the banks operating in those locations would coincide and be falsely attributed to the London crisis. However, to the extent that indicators of a boom and bust cycle are observable, it is possible to test for systematic sorting to address this concern.

IV.C. Determinants of Bank Failures

1. *Quantitative Measures and Narrative Evidence.* Banks are balanced across most observable precrisis bank characteristics (Table II). Panel A lists publicly held banks (joint-stock banks) that published balance sheets. The characteristics of the banks that failed are not statistically or economically different from those of banks that did not fail along all dimensions: equity capital, equity already paid in by shareholders, reserve funds, deposit liabilities, total size, leverage ratio, liquidity ratio, and reserve ratio.

I also analyze transcripts of the banks' biannual shareholder meetings that cover their operations from 1865 to 1867, which

31. Borusyak and Hull (2020) discuss issues arising from nonrandom exposure to shocks even in the case of linear instruments, but those concerns do not apply to this setting because the shares sum to one.

TABLE II

PRECRISIS COMPARISON OF BANK CHARACTERISTICS

	All		Not failed		Failed		Diff	
Panel A: Balance sheet characteristics (joint-stock banks)								
OG connection	0.16	(0.37)	0.11	(0.32)	0.37	(0.50)	−0.26	(0.09)***
Capital, authorized (£m)	1.42	(1.06)	1.39	(1.05)	1.56	(1.13)	−0.16	(0.28)
Capital, paid up (£m)	0.58	(0.38)	0.60	(0.37)	0.47	(0.41)	0.13	(0.10)
Deposits (£m)	2.22	(2.73)	2.29	(2.82)	1.85	(2.37)	0.44	(1.14)
Reserve fund (£m)	0.13	(0.12)	0.13	(0.12)	0.13	(0.16)	−0.01	(0.04)
Total size (£m)	4.76	(6.08)	4.96	(6.38)	3.83	(4.65)	1.13	(1.88)
Leverage ratio	0.06	(0.05)	0.06	(0.05)	0.06	(0.06)	0.00	(0.02)
Reserve ratio	0.03	(0.02)	0.03	(0.02)	0.03	(0.02)	−0.00	(0.01)
Liquidity ratio	0.14	(0.11)	0.14	(0.09)	0.12	(0.15)	0.02	(0.03)
N	100		81		19		100	
Panel B: Other characteristics (all banks)								
OG connection	0.16	(0.37)	0.13	(0.34)	0.33	(0.48)	−0.20	(0.09)**
Trade finance (£k)	105.65	(246.58)	112.21	(263.75)	72.23	(126.50)	39.98	(58.98)
Age (years)	36.95	(53.22)	41.31	(56.48)	14.76	(21.34)	26.55	(12.53)**
Cities (#)	13.91	(23.11)	15.01	(24.74)	8.29	(10.34)	6.72	(5.51)
Countries (#)	6.98	(7.78)	7.16	(8.00)	6.05	(6.60)	1.11	(1.86)

TABLE II
CONTINUED

	All		Not failed		Failed		Diff	
Regions (#)	2.58	(1.58)	2.62	(1.61)	2.38	(1.43)	0.24	(0.38)
Asia (£k)	44.31	(169.41)	47.56	(183.47)	27.79	(59.26)	19.76	(40.56)
Africa (£k)	7.83	(23.60)	7.14	(21.85)	11.39	(31.47)	−4.26	(5.64)
N. America (£k)	10.89	(37.47)	12.30	(40.47)	3.73	(13.05)	8.56	(8.95)
S. America (£k)	7.44	(34.26)	8.38	(37.22)	2.65	(9.15)	5.73	(8.19)
Oceania (£k)	6.39	(17.24)	7.06	(18.51)	2.98	(7.54)	4.07	(4.12)
Europe (£k)	25.98	(61.57)	26.48	(64.95)	23.40	(41.28)	3.08	(14.75)
British Emp (£k)	60.39	(159.09)	65.47	(171.52)	34.50	(62.90)	30.96	(38.02)
UK (£k)	12.11	(39.69)	14.26	(43.10)	1.18	(2.73)	13.07	(9.44)
N	128		107		21		128	

Notes: The table shows bank-level balance across characteristics for banks that failed and did not fail. All variables are measured at the end of 1865 before the crisis. Balance sheet variables were only published for publicly traded banks; these are reported separately in Panel A. "Not failed" and "Failed" refers to whether a bank suspended or closed during the crisis. Means are reported first, and standard deviations are given in parentheses. "Diff" refers to the difference in means between groups. Standard errors are reported in parentheses for the "Diff" column. £k denotes units of thousands of pounds sterling. £m denotes units of millions of pounds sterling. Leverage ratio is defined as capital (paid and reserves) divided by total assets. Reserve ratio is defined as reserve assets divided by deposit liabilities. Liquidity ratio is defined as cash, gold, and short-term bills divided by total assets. "Cities," "countries," and "regions" count the number of unique geographic locations banks operate in. Significance is marked by * $p < .1$, ** $p < .05$, *** $p < .01$.
Sources: Bank of England Archives C24/1, *Banker's Magazine*, *The Economist*.

provide qualitative evidence on the nature of each bank's business. There is no evidence that differences in local economic conditions or bank risk-taking behavior affected their failure rates. The full results, discussion of the sources, and examples of the language are in Online Appendix C.6.

Panel B includes all banks and their other observable characteristics. Banks that survived were older by 27 years (half a standard deviation), but age would only be a confounder if older banks systematically operated in locations that are both exposed to bank failures and less likely to experience declines in exports. Geographical region of specialization is also not systematically different and does not predict bank failure, measured either in nominal values (Panel B) or as a share of the bank's assets (Online Appendix Table A3). Banks in the two groups are similarly geographically diversified, operating in an average of almost 14 cities and 7 countries. This balance helps address the concern that bank failures and export contractions were simultaneously caused by a shock that was systematically correlated with their geography, such as adverse weather.

2. *Overend & Gurney Connection.* The primary explanation for why some banks failed appears to be a public connection to Overend & Gurney. As was standard at the time, investors had only "paid in" a fraction of their equity and could be "called" for the remainder.[32] The shareholder list circulated in London at 2.5 times the publishing price during the crisis, and contemporary evidence indicates that depositors found this a valuable piece of information.[33]

I digitize the shareholder list from January 1866 (Online Appendix Figure C2a) and compare it with the names of the managers and directors of the London banks (Online Appendix Figure C2b).[34] A bank is characterized as having a known connection to Overend & Gurney if one of its managers or directors is listed as a shareholder. It is reasonable that

32. Specifically, they had only paid in £15 of the £50 nominal and could be obligated for the remaining £35 per share at any moment; £35 in 1866 is equivalent to £4,193 in 2020.

33. See Online Appendix C.3 for contemporary documentation of the crisis and the demand for the shareholder list.

34. The shareholder list was found at the Royal Bank of Scotland archives in Edinburgh, Scotland. January 1866 was the last list that was compiled before the firm declared bankruptcy.

upon observing this public connection to a failed, fraudulent firm, depositors lowered their assessments of their bankers' investment decisions, which worsened the runs on those institutions. Table II, Panel B, row 1 shows that this public Overend connection significantly predicts bank failure (Panel A, row 1 shows this relationship is even stronger for joint-stock banks, which more heavily advertised the identities of their managers and directors). Moreover, there is little correlation between the Overend connection and the observable characteristics of either bank age or bank lending patterns (Online Appendix Table C2).[35]

IV.D. Correlation between Location Characteristics and Bank Failures

To test the exogeneity of bank-level failure rates to location-level characteristics, I follow Borusyak, Hull, and Jaravel (2022) and calculate each bank's exposure to those characteristics and correlate them with the bank failure rates.[36]

I examine the observable precrisis location-level characteristics at the port level and the country level, because those are the two units of observation in the analysis. At the port level, the observable characteristics include the volume of exports (proxied by the number of ships from *Lloyd's List*), importance of the United Kingdom as a destination, geodesic distance to London, latitude, number of destinations, availability of non-British financing, and

35. Overend's ledgers do not appear to have survived, so it is not possible to calculate each bank's operational exposure to the firm. However, since the primary business banks had with Overend was buying and selling bills—a transaction that was cleared immediately without any liabilities held on the balance sheet—Overend's failure would not have directly affected their books. In addition, shareholders eventually covered all of Overend's debts in full, and the extent of operational relationships was not known to the public.

36. The advantage of testing the bank-level relationship rather than the location-level relationship, the latter of which is also used in the literature, is that it performs the Adão, Kolesár, and Morales (2019) standard error correction. They show that when the source of identification from a Bartik instrument are the shocks, the standard errors of regressions of the instrument on location characteristics tend to overreject the null hypothesis. Intuitively, the location-level tests target randomness in the shares, but when the location shares themselves are not suitable instruments, the covariance between the shocks and the shares may be relevant. Borusyak, Hull, and Jaravel (2022) show that implementing the Adão, Kolesár, and Morales (2019) standard error correction is equivalent to translating the location-level characteristics into bank-level exposure rates.

whether the port is a capital city.[37] At the country level, observable characteristics include total value of exports, export growth rates precrisis, value of exports by industry, share of commodities in the composition of exports, monetary system, and whether the country was engaged in conflict. Each bank's share-weighted average exposure $\bar{X}_b$ to a precrisis characteristic X_l is calculated as $\bar{X}_b = \frac{\sum_l z_{lb} \times X_l}{\sum_l z_{lb}}$ where larger weights are given to locations more dependent on bank b. The normalized individual bank failure rates are regressed on the transformed location-level characteristics $\bar{X}_b$:[38]

$$\mathbb{I}(Failure_b) = \alpha + \beta \bar{X}_b + \varepsilon_b. \tag{4}$$

Table III reports the results and shows that there is balance on almost all characteristics. In terms of port-level characteristics, Panel A shows that two factors are unbalanced: banks operating in ports with a higher fraction of exports going to the United Kingdom were more likely to fail, and those operating in ports that were also the capital cities in countries were less likely to fail. These characteristics are included as controls in the baseline specifications to residualize any direct effect that they have on exports.

Table III, Panel B shows that banks that failed did not systematically operate in countries with lower export values, higher precrisis export growth rates, a greater share of trade to the United Kingdom, or exposure to military conflicts and different monetary standards.

In order to address the possibility of commodity booms and busts, I categorize each country's 1865 exports by two-digit SITC categories and test balance across all industries and to the overall share of commodities exported. Table III, Panel C shows that banks that failed are not differentially exposed to the top seven industries of raw cotton exports, cotton manufactured goods, all cotton, bullion, grains, coffee, alcohol, and the overall share of commodities exported. In Online Appendix Table G16, I provide balance checks for the remaining SITC industries. Finally, I check

37. Results are similar using sailing distance instead of geodesic distance to London. Online Appendix Figure B5 shows the strong positive correlation between the two types of distances.

38. The regressions are weighted by $\hat{z}_b$, which is the average location exposure to bank b: $\hat{z}_b = \frac{1}{L}\sum_{l=1}^{L} z_{lb}$. The weighting is necessary to translate location-level relationships to bank-level relationships. The full derivation for the equivalence is given in Borusyak, Hull, and Jaravel (2022).

TABLE III

CORRELATION BETWEEN BANK FAILURES AND PRECRISIS LOCATION CHARACTERISTICS

$$\mathbb{I}(\text{Failure}_b) = \alpha + \beta \tilde{X}_b + \varepsilon_b$$

	Ships (1)	Share to UK (2)	Distance to London (3)	Brit empire (4)	Foreign banks (5)	Destinations (6)	Capital city (7)	
Panel A: Port characteristics								
	0.0273	0.171***	−0.0641	−0.0723	−0.0341	−0.0612	−0.131***	
	[0.0358]	[0.0335]	[0.0394]	[0.0411]	[0.0262]	[0.0337]	[0.0384]	
N	122	122	122	122	122	122	122	
	Exports val. (1)	Δ Exports (2)	Share to UK (3)	Gold (4)	Silver (5)	Bimetallic (6)	Conflict: any (7)	Conflict: interstate (8)
Panel B: Country characteristics								
	0.129	−0.0269	−0.0163	−0.0387	−0.00457	0.0434	0.00720	−0.00946
	[0.0677]	[0.0246]	[0.0346]	[0.0353]	[0.0449]	[0.0432]	[0.0456]	[0.0408]
N	128	128	128	128	128	128	128	128

TABLE III
CONTINUED

	Cotton, raw (1)	Cotton, manu. (2)	Cotton (3)	Grains (4)	Bullion (5)	Coffee (6)	Alcohol (7)	Commodities share (8)
Panel C: Country characteristics: exports composition								
	0.0428	− 0.0563	− 0.0438	0.0571	− 0.0624	− 0.0166	− 0.0446	− 0.0253
	[0.0299]	[0.0357]	[0.0339]	[0.0301]	[0.0344]	[0.0252]	[0.0360]	[0.0302]
N	128	128	128	128	128	128	128	128

Notes: The table reports estimates from the bank-level regression of bank exposure to location characteristics precrisis on bank failure rates. The dependent variable is $\mathbb{I}(\text{Failure}_b)$, the measure of bank failure. The independent variable of interest $\tilde{X}_b$ is the share-weighted exposure of banks to location characteristics, normalized to have zero mean and unit variance. Each column is labeled with the independent variable. The coefficients are interpreted as the increase in the probability that a bank fails given a standard deviation increase in the average bank exposure to a particular characteristic. Panel A includes location characteristics from the port panel. Panels B and C includes country-level characteristics like the monetary standard and presence of conflict in the exporting country in 1865/1866, and the industry composition of exports in 1865. The effective sample size is the inverse of the HHI of shock-level average exposure: $\frac{1}{\sum_b z_b^2}$ and is equal to 27.55 for ports and 28.05 for countries. Regressions are weighted by the average location's exposure to bank b. * $p < .1$, ** $p < .05$, *** $p < .01$. As discussed in Borusyak, Hull, and Jaravel (2022), another advantage of transforming the balance tests into shock-level (bank-level) regressions is that it makes it clear which shocks (banks) are the most relevant for the results. In Panel A, there are 122 observations instead of the full 128 because six banks operated in cities that were not the closest city for any port, so they do not contribute to the port-level exposure measures.

that these characteristics are not jointly significant (Online Appendix Figure B3) and find an F-stat of 1.14 with a p-value of .32.

V. Results

V.A. *Conceptual Framework*

The conceptual framework tying financial constraints to persistent changes in export dynamics relies on two main assumptions: a sunk cost of entry and substitutability among producers.[39] The sunk cost of establishing a trade relationship with a given destination (which cannot be recovered if the exporter exits) covers expenses such as learning about the local market and setting up communication and distribution networks.

Given the institutional framework of bank-intermediated trade financing where exporters need to cover working capital costs during the period of a shipment, I add external financing as a marginal cost to a Melitz (2003) framework.[40] Firm profits are an increasing function of productivity, financing supply in the exporter's location, and inverse trade costs. Online Appendix E contains the details of the model, discussion, and extensions.

In this framework, a financing shock lowers the supply of financing, which raises exporters' prices, lowers their profits, and raises the overall price index in their destinations. Exposed exporters will (i) export less (because their prices have increased) and (ii) be less likely to enter new markets (because they are less profitable). They may not exit a market because reentry in the future will require paying the sunk costs again, so there is

39. In the classic Baldwin (1988) framework, firms produce a homogeneous good and are completely substitutable. In Redding, Sturm, and Wolf (2011), the possibility of multiple equilibria is expressed as a function of how similar (and profitable) each potential location is for an industry. In spatial geography models (Krugman 1991; Davis and Weinstein 2008; Allen and Donaldson 2020), persistence (or multiple equilibria) arises out of agglomeration forces. In all cases, large enough shocks can shift the equilibrium, either due to sunk costs of investments or by allowing the agglomeration forces to entrench elsewhere.

40. Chaney (2016) and Manova (2013) also adapt Melitz (2003) to incorporate financing shocks, where the former assumes that the liquidity necessary to export is randomly drawn and uncorrelated with firm productivity, while the latter ties firm borrowing constraints to firm productivity through default risks. In both cases, financing is only necessary to pay for entry costs, and imperfect financial markets constrain some firms that are productive enough to export from doing so.

an option value to continuing to export even with negative flow profits.[41]

In a destination, importers will indirectly experience their trade partners' financing shock through an increase in their own price index. Higher price indices raise average profits, and (iii) new firms will enter a destination and increase the number of relationships importers have. Postshock, the competitive entry plus lack of exit means that the mass of firms operating is higher, making future entry more difficult. In an environment with high substitutability and switching costs, (iv) the market share losses can persist.[42]

Following Redding and Weinstein (2017), the underlying firm-level dynamics described in the conceptual framework can be aggregated and then analyzed using more macro-measurements of trade. Online Appendix E provides more discussion of this point.

V.B. Immediate Effects

1. *Intensive Margin.* I examine the immediate effect of bank failures on the intensive margin of exports with the *Lloyd's List* panel and restrict the analysis to locations that are active before and after the shock in the two-year window around the crisis.

First, using the country-level panel, I estimate the following difference-in-differences regression with continuous treatment intensity:

$$(5)\quad \ln(S_{ot}) = \alpha Post_t + \beta(Fail_o \times Post_t) + \gamma_o + \Gamma' X_o \times Post_t + \varepsilon_{ot}.$$

S_{ot} is the total number of ships departing a country per period. β is the coefficient of interest, and $Fail_o$ is an exporting country's exposure to bank failures calculated according to equation (2)

41. Note that if exporters are only exposed to a recurring fixed cost, the financing shock will also increase the likelihood of exit. The presence of sunk costs is also the friction that generates hysteresis in exporting status in Roberts and Tybout (1997) and in the geographic distribution of industrial activity in Redding, Sturm, and Wolf (2011).

42. In addition, exporters learn about their trade partners, become more productive, and direct their investments to accommodate demand so early movers accumulate advantages. There is much empirical evidence that the quality and value of trade relationships improves over time with learning. In the context of the nineteenth century Juhász and Steinwender (2018) document that agreeing on the specific goods traded is a costly process while in a modern context, Atkin, Khandelwal, and Osman (2017) show that the process of exporting improves firm productivity through learning-by-doing.

using country-level shares of precrisis bank dependence. $Post_t$ is an indicator for the postcrisis period that controls for macroeconomic shocks affecting the export trends over time. X_o are precrisis country characteristics that can be included as additional controls when interacted with $Post_t$. Country fixed effects γ_o absorb all time-invariant differences in levels of shipping, including those correlated with their exposure to bank failures. Regressions are weighted by the precrisis size of country-level shipping activity. Standard errors are clustered by country of origin.

Table IV, column (1) shows the baseline effect without any additional country-level controls. The coefficient of −0.61 implies that countries with an average exposure of 9 percentage points exported 5.4% less than nonexposed countries in the postcrisis year. Online Appendix Table A4 adds origin-country characteristics as controls to show that the results are not affected by differences in macroeconomic characteristics, such as the industry composition of exports.

It is possible that unobserved country-level shocks are partly accounting for the results, so I identify the effects using within-country variation from port-level shipping activity that allows me to control for unobserved time-varying shocks to the origin-country. To do so, I estimate the port-level analogue to equation (5) where S is now the number of ships leaving from port p in origin country o in period t:

$$(6)\quad \ln(S_{pot}) = \beta(Fail_{po} \times Post_t) + \Gamma' X_{po} \times Post_t + \alpha_p + \gamma_{ot} + \varepsilon_{pot}.$$

Each port in the panel is matched to the closest city of financing by geodesic distance, and its exposure to bank failures Fail_{po} is assumed to come from that city.[43] Ports more than 500 km from the nearest city of financing are part of a control group of completely unexposed ports and are given an exposure of zero.[44] This control group allows for ports that are still connected to London but experienced no bank failures to react differently from ports that were not connected to London at all. X_{po} are precrisis port characteristics that can be included as additional controls. Port

43. For example, the port of Piraeus in Greece is designated as receiving its funding from Athens.

44. I include an indicator for these ports interacted with the post time period. The results are not sensitive to the 500 km boundary and the main coefficients are robust for a range of distances and to not including the time-varying intercept for distant ports.

TABLE IV

INTENSIVE-MARGIN EFFECT OF BANK FAILURES ON SHIPPING

Col 1: $\ln(S_{ot}) = \alpha Post_t + \beta \text{Fail}_o \times \text{Post}_t + \Gamma' X_o \times \text{Post}_t + \gamma_o + \varepsilon_{ot}$
Col 2-7: $\ln(S_{pot}) = \beta \text{Fail}_{po} \times \text{Post}_t + \Gamma' X_{po} \times \text{Post}_t + \alpha_p + \gamma_{ot} + \varepsilon_{pot}$

	Country	Port			Excl. cotton	Poisson	News dates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Fail}_o \times$ post	−0.606***						
	[0.214]						
$\text{Fail}_{po} \times$ post		−0.727***	−0.724***	−0.656***	−0.554***	−0.966***	−0.592***
		[0.251]	[0.159]	[0.210]	[0.204]	[0.169]	[0.208]
Capital city × post				Y	Y	Y	Y
Age of banks × post				Y	Y	Y	Y
OG link × post				Y	Y	Y	Y
Share to UK × post				Y	Y	Y	Y
Country_o FE	Y						
Port_p FE		Y	Y	Y	Y	Y	Y
$\text{Country}_o \times$ post FE			Y	Y	Y	Y	Y
N	108	578	578	578	494	578	570
Ports		289	289	289	247	289	289
Clusters	54	54	54	54	50	54	54

Notes: The table reports estimates from the difference-in-difference regressions from the two-period panel of port-level shipping activity in the year before and after the crisis. The dependent variable in columns (1)–(5) and (7) is the log of the total number of ships departing in each period. In column (6), a Poisson estimator is used so the dependent variable is the count of ships. Fail_{po} is the share of the port's banks that failed during the crisis. The mean of Fail_{po} is 0.07, and the standard deviation is 0.2. Post is a dummy for the postcrisis year that takes the value of 1 after May 1866 and 0 otherwise. The time-invariant control variables are measured in 1865 and interacted with the post dummy. They include an indicator for the port being a capital city in the country, the average age of banks, the fraction of banks with an Overend connection, and the share of shipping to the United Kingdom in the preshock period. The sample is restricted to ports active in both the pre- and postperiod. In column (5) the sample excludes major cotton exporting countries (defined as Brazil, Egypt, India, and the United States). In column (7), the "post" crisis period varies at the port level based on the news arrival dates of the crisis from London. Standard errors in brackets are clustered by country of origin. * $p < .1$, ** $p < .05$, *** $p < .01$.

fixed effects α_p absorb all time-invariant port-specific differences in levels of shipping, and origin-country-by-post fixed effects γ_{ot} flexibly control for all observed and unobserved changes at the country level that affect shipping, such as GDP and multilateral resistance.[45] Including these fixed effects means β is identified by comparing ports from the same country and year.[46] Standard errors are clustered by the country of origin to allow for heteroskedasticity and within-country spatial correlations.

Table IV, columns (2)–(4) presents the baseline results with fixed effects and controls added sequentially. The country-by-post fixed effect does not significantly affect magnitudes, indicating that there were unlikely to have been short-term country-level shocks correlated with both exposure and exports. The additional controls are based on the port-level characteristics that are not balanced between banks that failed and did not fail in Table III and include the average age of the banks, the Overend & Gurney connection, whether the port is the capital city, and the share of ships going to the United Kingdom in the precrisis year.[47] The coefficient of −0.66 in column (4) with all the controls indicates that ports with an average exposure of 7 percentage points have 4.6% lower exports in the postcrisis year. These magnitudes are very similar to the country-level estimates (column (1)) which provides further evidence that banking failures are orthogonal to country-level characteristics that would have changed the path of exports.

In Online Appendix Table G1, I add each control variable in turn and show that the coefficients remain stable and statistically significant. I also implement the recommended bounds in Oster (2019) to show that selection on location-level unobservable characteristics is minimal. These calculations show that the degree of unobservables bias would have to be approximately 40 times larger than the degree of observables bias. The $Fail_{po}$ measure is skewed, so in Online Appendix Table G4, I provide a breakdown of the effect by terciles.

45. Assuming that all other relevant trade costs that countries face vary at the country level, these fixed effects will absorb changes in the multilateral resistance that countries experience in general equilibrium.

46. There are on average five ports per country. Countries with only one port are effectively dropped from this estimation. These account for 8 of the 289 ports (2.8%), which reduces the effective number of countries in the estimation from 54 to 46.

47. Bank-level characteristics are aggregated to the port level using the precrisis shares $z_{lb,\,pre}$ of the importance of each bank to each location.

Table IV, columns (5)–(7) summarize several robustness checks. Column (5) excludes the cotton-exporting countries that may have experienced a correlated shock due to the end of the American Civil War;[48] column (6) accounts for potential mismeasurement in the outcome variable by reestimating all the results using count data methods; and column (7) uses the time-series granularity of *Lloyd's List* and allows for communication lags between London and cities around the world. The estimated effects are very similar across these specifications. Online Appendix G.1 provides extensions of each result (Online Appendix Table G6 for cotton, G7 for news lags, and G8 for the Poisson estimator). In addition, I show that the baseline effects are robust to controlling for exposure to different regions (Online Appendix Table G2) and empires (Online Appendix Table G3) and are not attributable to demand shocks specifically from the United Kingdom or more generally (Online Appendix Tables G5 and G9); I limit the sample to well-traveled routes to diminish the effect of outliers along routes (Online Appendix Figure G2, Tables G8 and G10); I remove ports in countries that are islands and entrepots (Online Appendix Table G11); and I show similarity to annual regressions using country-level values of exports (Online Appendix Table A7).

2. *Extensive Margin.* To study the extensive margin, I categorize exporting activity in two ways. The first is the number of unique destinations that a port or country trades with conditional on exporting, and the second is the likelihood that a port engages in any international trade.

I can decompose the change in total exports to countries (estimated without any controls) in Table IV, column (1) into the change in the number of destinations (Table V, column (1)) and the change in average exports per destination (Table V, column (2)). The change in the number of destinations (column (1)) is my first measure of extensive-margin changes, estimated using the specification in equation (5). The decomposition shows that although both margins contribute to the drop in total exports, most of the drop is accounted for by the fact that more exposed places end up with relatively fewer trade partners.[49] Table V, column (3) shows a similar drop in the number of destinations for ports.

48. These countries are Brazil, Egypt, India, and the United States.

49. Note that in a difference-in-differences setting, this result does not necessarily imply a drop in the number of destinations but that exposed locations gained relatively fewer in the postperiod.

TABLE V
EXTENSIVE-MARGIN EFFECT OF EXPOSURE TO BANK FAILURES

	Country: dest (1)	Country: avg ships (2)	Ports: dest (3)	I(Port entry) (4)	I(Port exit) (5)
$\text{Fail}_o \times$ post	−0.592***	−0.0138	−0.409***		
	[0.196]	[0.299]	[0.149]		
$\text{Fail}_{po} \times$ post					
Fail_{po}				−0.303***	−0.00227
				[0.0846]	[0.169]
Port controls × post			Y		
Port controls				Y	Y
Port_p FE			Y		
$\text{Country}_o \times$ post FE			Y		
Country_o FE	Y	Y		Y	Y
N	108	108	574	331	318
Ports			289	331	318
Clusters	54	54	54	55	54

Notes: The table reports estimates of the effect of the exposure to bank failures on the extensive margin of shipping activity. The dependent variable in columns (1) and (3) is the log number of unique destinations accessed by countries and ports, respectively. The dependent variable in column (2) is the log of the average number of ships per destination from a country. The sample in columns (1)–(3) is restricted to ports that were active in both the preshock and the postshock periods. The dependent variable in column (4), I(Port entry), is a binary variable that takes the value of 1 for a port that was not active in the preshock period and became active in the postshock period, and 0 otherwise. The dependent variable in column (5), I(Port exit), is a binary variable for a port that was active in the preshock period and became inactive in the postshock period. The sample sizes in columns (4) and (5) reflect the number of ports that were active in the postperiod (for *Entry*) or preperiod (for *Exit*). All variables are defined the same way as in Table IV. Standard errors in brackets are clustered by country of origin. * $p < .1$, ** $p < .05$, *** $p < .01$.

Next, I examine the extensive margin of exporting at all. I expand the sample to include all ports that are present in either the pre- or postshock periods. I categorize ports as *Entering* into international trade if there is no exporting activity in the precrisis period and positive exports in the postcrisis period, and *Exiting* if the reverse is true. I estimate a linear probability model on a one-period cross section where E_{po} is an indicator for either *Entry* or *Exit* and standard errors are clustered by the origin-country:

$$\Pr(\mathrm{E}_{po}) = \alpha + \beta Fail_{po} + \gamma_o + \Gamma' X_{po} + \varepsilon_{po}. \tag{7}$$

Table V, columns (4) and (5) present the likelihood of $Entry_{po}$ and of $Exit_{po}$, respectively, including all port-level controls. The shock is a statistically significant deterrent to the extensive-margin ability to establish links with foreign markets, but it does not significantly affect exit. The differential effects for entry versus exit provide further evidence that sunk costs of establishing

relationships were a relevant friction.[50] Taken together, these results show within- and across-country evidence of the financing shock immediately lowering exports, primarily by preventing the extensive-margin growth experienced by unexposed countries.

It is worth noting that the extensive margin of trade is sensitive to mismeasurement where very small flows may not be reported. These concerns are bigger with more disaggregated data, and in the context of port-level flows, *Lloyd's List* may be less accurate for smaller ports. If so, changes on the extensive margin, especially for entry, may not be captured. However, as long as the mismeasurement is not systematically correlated with exposure, this would not be a source of omitted variable bias.

V.C. Long-Run Effects

I turn to the country-level bilateral trade data from 1850–1914 to examine the changes to global trade patterns in the long run.

1. *Total Exports.* There is a permanent divergence in the cumulative growth of total exports across countries. In Figure II, I plot the annual aggregate values of exports for countries binned into above- and below-average exposure to bank failure, where the average exposure is defined in the cross section of countries (9 percentage points), and levels for each group are indexed to equal 1 in 1866. The blue dot-dashed line (color version available online) shows the total value of world exports. The overall pattern is of tremendous growth: total global trade increased five fold over this period. Before 1866, exports are expanding at the same rate between the two groups of countries, so there are no differential pretrends between the groups, but after 1866 there is an immediate divergence in levels that does not recover. Online Appendix Figure B9a graphs the difference between the two groups, and Online Appendix Figure B9b plots the coefficients and standard deviations from the equivalent regression (including country and year fixed effects). The coefficients are significantly different from zero and increase in magnitude from 1867 to 1870, after which they level off at approximately −0.5. These coefficients imply

50. Similarly, Berman and Héricourt (2010) empirically find that access to finance influences the firm entry decision, but that it has no effect on the exit decision. In Chaney (2016), the extensive margin of exports generates changes in aggregate trade flows in response to valuation shocks.

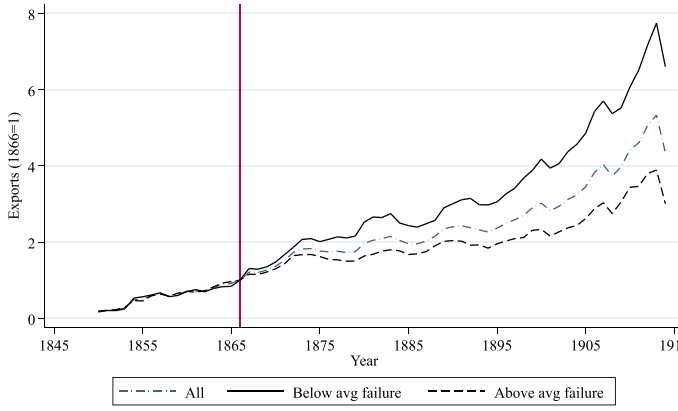


FIGURE II

Aggregate Exports, Grouping Countries by Above- and Below-Average Exposure to Bank Failures

Figure plots the raw data for the total value of exports by groups of countries from 1850 to 1914. Countries are binned into two categories: "Below avg failure" refers to countries that experienced below-average exposure to bank failures in London, where the average rate was calculated in the cross section of exporting countries in 1866. "Above avg failure" refers to countries that experienced above-average exposure to bank failures. Export values are normalized to equal 1 in 1866. The vertical line marks 1866.

that an exporter with average exposure has 20.6% persistently lower levels of total exports on average than with the unexposed counterfactual.

2. *Bilateral Exports and Market Share Effects.* The divergence in total values of exports between more and less exposed countries could be driven in part by the importing country's demand. In particular, if more exposed countries happen to have stronger relationships with countries that experienced slower import demand growth after the crisis, their exports would be affected. My baseline estimation therefore uses bilateral export volumes and controls for annual demand shocks from importers:

$$\ln(EX_{odt}) = \beta_t Fail_o + \Gamma' X_{ot} + \gamma_o + \gamma_{dt} + \theta_t \ln(\text{dist})_{od} + \varepsilon_{odt}. \quad (8)$$

The dependent variable is the log value of exports EX_{odt} from origin country o to destination country d in year t. $Fail_o$ is the origin-country exposure to bank failure, and it is interacted with leads and lags that estimates the effect pre- and postshock.

X_{ot} includes precrisis origin-country characteristics that are interacted with year dummies, which control for macroeconomic differences among countries. Origin-country fixed effects γ_o control for time-invariant country characteristics and restrict the source of variation to the change in exports within each country between periods. As in the port-level estimation, I control for the effect of the origin country not having any British banks at all in 1866, which separates the effect of any exposure from the degree of exposure to failed banks.[51] Destination-country year fixed effects γ_{dt} control for demand shocks by restricting the identifying variation to being across exporters, within destination-year. I control for the distance between countries dist_{od} measured either geodesically or as the fastest travel time.[52] I omit the covariate in the baseline year and normalize it to zero. Standard errors are clustered at the unit of treatment, the exporter country.[53]

I allow β_t to vary annually and at five-year intervals ([1850, 1855],..., [1911, 1914]). β_t is interpreted as the semi-elasticity of the relative volumes that the average importer buys from more versus less exposed exporters each period. It is identified using cross-sectional variation in every period, but it uses the full panel of data to control for determinants of trade flows like average country size.

Figure III, Panels A and B plot the estimated β_t coefficients annually and at five-year intervals, where β_{1866} and β_{1861-5} are the omitted years in each specification respectively. I scale the point estimates and standard errors to reflect the effect for the average level of exposure (9 percentage points). Online Appendix Figure B7 plots the estimated coefficients at the 25th, 50th, and 75th percentiles of exposure (3 percentage points, 8 percentage points, and 18 percentage points, respectively) conditional on nonzero exposure. Online Appendix Table A8 reports the distribution at various percentiles conditioning and not conditioning on nonzero exposure. Online Appendix Figure B8 plots the unscaled results, and those point estimates are reported in Online Appendix Table G12 (column (2)).

51. These countries accounted for 2% of the value of exports in 1866 since British banks operated in countries accounting for 98% of world exports.

52. I use Pascali (2017) for distance by either sail or steam.

53. Results are robust to different ways of clustering as reported in Online Appendix Table A7.

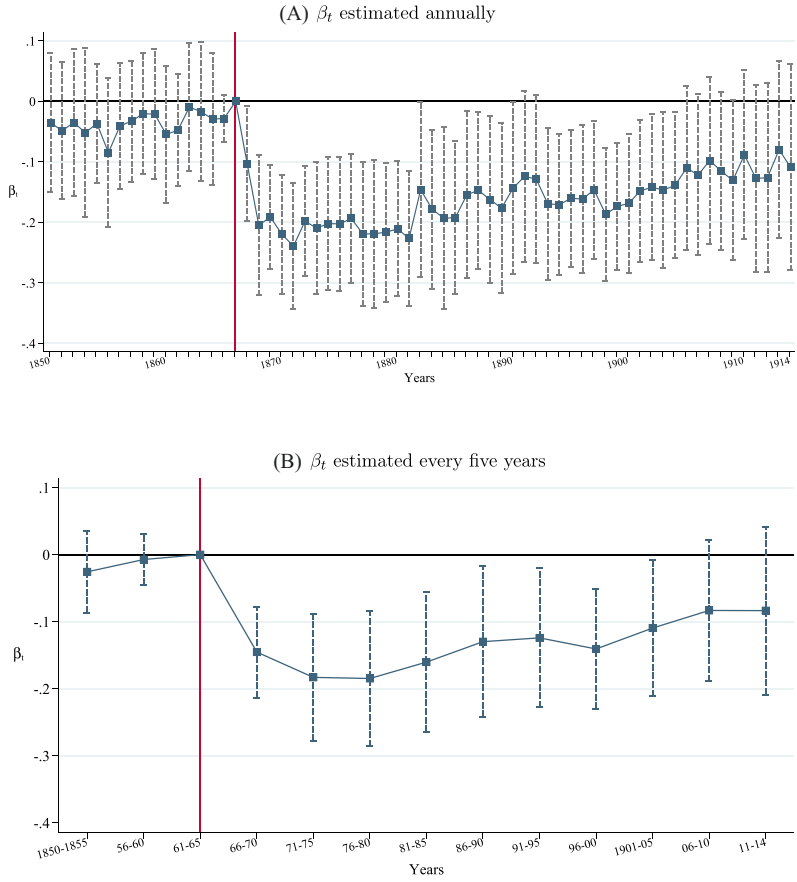


FIGURE III

Persistent Effect of Financing Shock on Exporter Market Share
$\ln(\text{EX}_{odt}) = \beta_t \text{Fail}_o + \Gamma' X_{ot} + \gamma_o + \gamma_{dt} + \theta_t \ln(\text{dist})_{od} + \varepsilon_{odt}$

Figure plots the β_t point estimates and 95% confidence intervals for the specification given in equation (8) estimated on the country-level panel of trade. β_t is the treatment coefficient on the effect of exposure to failed banks on exports in each group of years. Point estimates and standard errors are scaled by the mean of treatment, so the magnitudes should be interpreted as the effect for the average exporter. The dependent variable is the log value of exports. The specification includes origin country o FE, destination country-year dt FE, and time-varying controls for the bilateral distance between countries. Standard errors are clustered by the origin country. See Online Appendix Table G12, column (1) for the point estimates.

The persistence is striking: destination countries imported significantly less from exporters that had been exposed to bank failures for almost 40 years. $\beta_{1901-05}$ is the first period when the effect is only borderline statistically different from zero. In a two-period estimation with a single postperiod, β_{post} is -1.32 log points (standard error 0.42). Given the average exposure of 9 percentage points, this magnitude implies that the (partial-equilibrium) disruption in the shape of global trade was 12% every year in the postcrisis period until World War I.

The estimated coefficients support the patterns in the raw data for total exports that exposure to the crisis had no effect on exports precrisis but immediately lowered trade flows between countries afterward. The lack of pretrends and the balance on observables discussed in Section IV.D helps address the potential identification concern that stronger banks were systematically better at choosing their operating locations and therefore tended to operate in better places. This sort of selection bias would lead to a pretrend in the event study difference-in-differences because good places with low exposure would already be growing before 1866, but this is not the case.

3. *Structural Gravity.* The reduced-form specification in equation (8) estimates the overall effect of the financing shock on exports, which may be operating through a trade-financing channel that only affects exporting or a more general bank-lending channel that also affects production for instance. However, assuming that the financing shock acts as a pure trade cost shock makes it possible to estimate a theoretically grounded elasticity arising from structural gravity models that can be compared with other trade costs in the literature. Structural gravity models show that a change in the bilateral trade costs between any two countries will affect all other countries even if everything else remains the same. These multilateral-resistance terms are nonlinear functions of the full set of bilateral trade costs and are not directly observable, but using exporter and importer country fixed effects in cross-sectional estimations (e.g., Feenstra 2016) or exporter-year and importer-year fixed effects with panel data (Olivero and Yotov 2012; Head and Mayer 2014) delivers consistent estimations.

Equation (8) differs from the fixed-effects implementation of structural gravity because I cannot include origin-country-year fixed effects, as those are collinear with the treatment. I therefore follow the technique introduced by Baier and Bergstrand (2009)

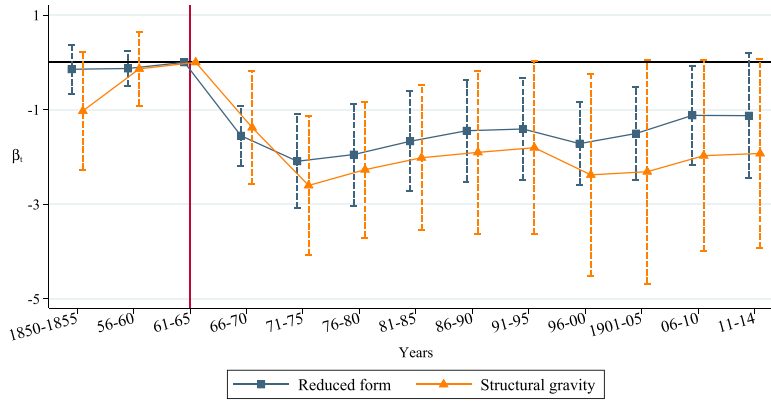


FIGURE IV

Persistent Effects: Comparing Baseline with Structural Version

Figure plots the point estimates and 95% confidence intervals for two separately estimated regressions. The one labeled "Reduced form" follows the specification in equation (8) that estimates the reduced-form β without controlling for multilateral resistance terms. The one labeled "Structural gravity" corresponds to the coefficients in Online Appendix Table F1, column (4) that estimates both the ϕ and ψ in the estimating equation (F7) in the Online Appendix. Standard errors are clustered by the origin country.

and implemented in Berger et al. (2013), which approximates the nonlinear multilateral resistance terms with a first-order log-linear Taylor series expansion. This technique imposes additional restrictions on the estimated coefficients for trade costs. Online Appendix F provides the full derivation of the estimating equation.

The structural estimation requires a measure for origin-country size, which is typically proxied with GDP. Historical GDP data are not available for all countries, so the results are estimated on a smaller set of observations. In addition, because these data are less reliable than trade data, I do not impose the additional requirement that trade and GDP have unit elasticities throughout the sample. I plot the results from this estimation in Figure IV along with the reduced form, where I again scale the coefficients by the average exposure. The two-period unscaled postcrisis β_t estimated on this subsample is -1.50 (std. err. 0.41) for the reduced form and -1.88 (std. err. 0.84) for the structural approach. While the estimated effect from this approach is

25% larger in magnitude and even more persistent, it is not statistically different from the baseline.

Online Appendix Table F1 reports the coefficients from adding GDP controls, the baseline estimated on the same sample of observations, and this structural approach. Restricting the baseline estimation to the subset of countries with GDP data partly accounts for the difference in magnitudes (column (1) versus (2)), as does directly controlling for GDP (column (2) versus (3)). Adding the Baier and Bergstrand (2009) multilateral resistance terms (column (4)) leads to estimates that are completely persistent. Overall, the qualitative similarity in these results relative to the reduced-form baseline indicates that unobserved bias arising from not directly controlling for changes in multilateral resistance is small. Given the stronger assumptions that the financing shock only affected trade costs, and the empirical limitations of the smaller sample size and lower quality of GDP data, I present the structural approach as a secondary set of results and refer to the reduced-form approach as the baseline.

4. *No Effect on Imports.* Exposure to bank failures does not affect a country's imports, consistent with the institutional context that banks were not financing consumption. I estimate the effect of the crisis on a country's imports using the baseline specification in equation (8), replacing the key regressor of the exporting country's exposure with the exposure to bank failure in the importing country.[54] As in the baseline, I saturate the estimation with fixed effects to account for exporter supply shocks (γ_{ot}) and the importer's overall size (γ_d). I present these results in Online Appendix Table A9. Column (2) shows that exposure to bank failures has no effect on a country's imports with coefficients close to zero and not statistically significant. In column (3), the import effects are robust to controlling for the shock to exporters as well.[55]

54. Given the symmetry in trade flows, one country's exports is its trade partner's imports. Therefore estimating the effect of country A's exposure to bank failures on the amount it imports from country B is equivalent to estimating the effect of country B's exposure to bank failures on the amount that it exported to country A. The equation of interest, $\ln(IM_{dot}) = \beta_t \text{Fail}_o + \varepsilon_{odt}$, is equivalent to $\ln(EX_{odt}) = \beta_t \text{Fail}_d + \varepsilon_{odt}$. Note that in the notation IM_{dot}, the goods are traveling from country o to country d at time t.

55. Directly controlling for the importer's exposure to the crisis makes it impossible to include the full set of controls, such as the destination-year fixed effects

5. *Robustness.* I provide several robustness checks for the long-run exports effects in Online Appendix G.2. First, I control for a wide variety of initial and contemporaneous macroeconomic conditions that could be sources of omitted variable bias. In Online Appendix Table G12, columns (3)–(8), I show robustness to origin-country controls, including the precrisis characteristics that are correlated with bank failures. In Online Appendix Table G13, I report the estimates after including additional gravity covariates, such as shared language, shared land border, and being in the same European empire. I control for precrisis and contemporary military conflicts and exchange rate regimes (Online Appendix Table G14); industry composition of exports precrisis and initial trade intensity with the UK (Online Appendix Tables G15–G17); exclude the cotton-exporting countries (Online Appendix Table G18); control for financial crises like sovereign debt, domestic debt, stock market crashes both contemporaneous and in 1865 (Online Appendix Tables G19 and G20); account for the trade cost changes from the Suez Canal opening in 1869 (Online Appendix Table G21);[56] and exclude islands and entrepots (Online Appendix Table G22). The static and the time-varying versions of all of these controls do not affect the statistical significance or the qualitative patterns of the results, and they make it unlikely that these other events were the real drivers of the persistent collapse in export market share.

Second, I generalize the concern that any individual country is affecting the results by estimating the baseline specification while dropping each exporting country in turn. In Online Appendix Figure G3, I plot the distribution of the estimated coefficients and the distribution of the associated p-values. These show that not only are the magnitudes of the coefficients robust, but the patterns of statistical significance are as well. The coefficients before the shock are close to zero and not significant, and as in the baseline results, they become large in magnitude and economically significant after 1866 before exhibiting recovery in 1900. This robustness check also helps address the potential

that are included in the baseline estimations. Not being able to fully and flexibly control for demand shocks from importers attenuates the estimated effect of the crisis on exporters.

56. The Suez Canal primarily reduced trade costs between Asia/Oceania and Europe/East Coast of the Americas. I allow for this differential change in bilateral trade costs using data collected by Pascali (2017).

concern that the results are driven by a small number of countries that experienced unobserved positive shocks after 1866.

Third, I implement the Fisher exact test for randomization inference. This test is conducted by reassigning treatment randomly and without replacement to countries to compare the estimated treatment effect against thousands of placebos. At longer time horizons, countries' exports could be affected for a number of reasons, and assigning the treatment randomly will show whether the long-term negative effects could arise naturally from the data for reasons unrelated to the financing shock. If that is the case, the distribution of estimated coefficients will become more negative with each subsequent group of years. In this test, I redistribute the shocks randomly and simulate the data 1,000 times, and then I estimate the long-term effects in equation (8) using the simulated data. I plot the distribution of the estimated coefficients for each β_t coefficient in Online Appendix Figure G4, which shows that the coefficients are centered around zero in all periods. The lack of drift indicates that the long-term effects are statistically very unlikely to have been generated by unobserved processes of divergence.

The large number of fixed effects and controls for observable characteristics, the balance on the correlation between the shock and precrisis characteristics, and the lack of differential pretrends help address many concerns about omitted variable bias. However, given the long time horizon of the estimated effects, it is impossible to rule out that other unobserved economic shocks or secular trends and developments may have contributed to the persistent effects that I find.

VI. Changes To Trade Patterns

VI.A. New Sourcing by Importers

To better understand the cause of exporters' persistent market share losses, I examine how their trade partners (the importers) reacted to exporters being exposed to the shock. Guided by the conceptual framework, for each importer, I proxy for the financing shock's effect on the importer's own price index with its precrisis trade partners' exposure. Destinations reliant on more exposed exporters were indirectly exposed to bank failures because the change in financing costs was either passed on in the form of higher prices or fewer varieties. The third prediction in the framework is that the higher price index from

indirect exposure to bank failures will then make a destination more profitable and induce new exporters to enter.

I examine the effect of indirect exposure on a country's aggregate imports. I retain the notation from the baseline specifications with importers labeled as country d and exporters labeled as country o. I calculate indirect exposure denoted as $\overline{Fail_{d,pre}}$ by weighting a country d's trade partners' exposure o using the three years of trade flows before 1866.[57] The specification for the effect of indirect exposure on total imports is:

$$\ln(IM_{dt}) = \beta_t \overline{Fail_{d,pre}} + \gamma_d + \gamma_t + \varepsilon_{dt}. \tag{9}$$

I plot these results in Figure V, Panel A. First, I find that more indirectly exposed importers did not systematically buy less than other importers. Next, I estimate and decompose the effects on aggregate imports into the effect on the number of trade relationships and the average imports per relationship. These specifications follow the same form as equation (9), with the different dependent variables as noted. Total imports were not significantly affected, but indirect exposure led to more trade linkages after the shock with a concomitant decline in average imports per source.

While on average importers sourced less from each country, there is significant heterogeneity in the exporters that lost market share. A second decomposition of the aggregate imports into the share of total imports between new trade partners formed after the shock versus preexisting trade partners shows that the preexisting relationships with high exposure experience a sharp decline in total market share.[58] I plot these results in Figure V, Panel B. This market share decomposition has the same overall pattern as the market share losses in the baseline estimation.

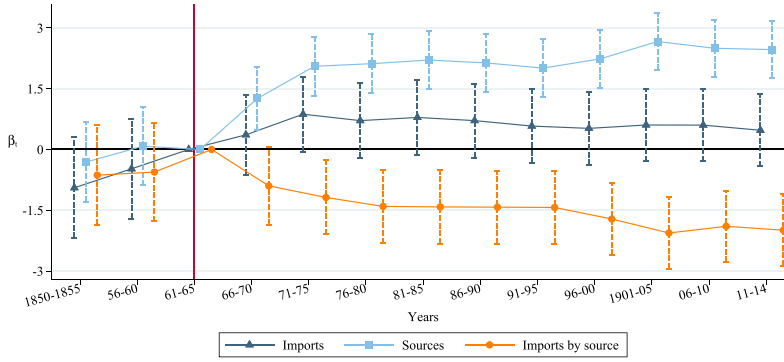
1. *Comparing Substitutable Exporters.* So far I have shown that a trade cost shock between parties leads importers to increase the share they buy from unexposed countries and to source from new relationships. In the nineteenth century, most countries exported commodities that were often produced in multiple

57. The results are virtually unchanged by using trade shares calculated with data from one to five years precrisis.

58. Note that in a world of growing levels of aggregate trade, the reduction in market share from this decomposition does not require that importers are buying less in absolute levels from preexisting exporters relative to before the shock, just that the denominator of total imports is growing faster than the numerator.

(A) Decomposition of total imports, import sources, and average imports per source

$$\ln(IM_{dt}) = \beta_t \overline{Fail_{d,pre}} + \gamma_d + \gamma_t + \varepsilon_{dt}$$



(B) Decomposition of total market share

$$\frac{IM_{dt,pre}}{IM_{dt}} = \beta_t \overline{Fail_{d,pre}} + \gamma_d + \gamma_t + \varepsilon_{dt}$$

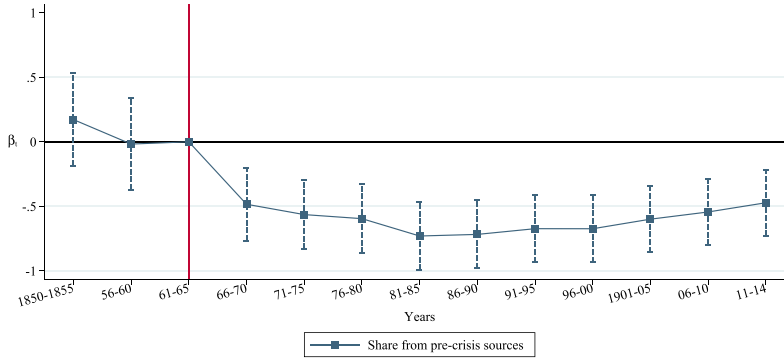


FIGURE V

Decomposition of Effect of Indirect Exposure on Importers

Panel A plots the β_t point estimates and 95% confidence intervals for three separately estimated regressions of the specification written above and the dependent variable indicated. The specification includes destination (importer) d and year fixed effects. The dependent variable is the log of total imports (dark blue triangle), the log of the number of trade partners (light blue square), and the log of average imports per source (orange circle). $\overline{Fail_{d,pre}} = \sum_o s_{od} Fail_o$, where $s_{od} = \frac{IM_{do}}{IM_d}$. The estimated effects are not scaled by average indirect exposure, so they should be interpreted as the effect if all of an importer's trade partners precrisis were fully exposed to the bank failures. Panel B has the share of total imports from precrisis trade partners as the dependent variable. Standard errors are clustered by destination country. The observations are weighted by country size proxied by the number of trading partners to most closely mirror the aggregates in Figure II.

locations, likely leading to a high degree of substitutability across countries. For example, a country importing sugar could choose among many producers in the Caribbean and South America. A large shock to the cost of exporting from one country can give its competitors a relative advantage in each destination where those competitors can enter and capture market share. Once importers establish a relationship, it may be difficult for exporters who had experienced a shock to regain their lost market share, even after the shock passes.

First, I use the industry composition of a country's total exports precrisis, categorized by two-digit SITC codes, to test for importers substituting among similar countries.[59] I estimate the baseline specification in equation (8) with time-varying industry controls where each country is assigned the SITC industry of its biggest exports in 1865. The SITC industry controls mean that β_t should be interpreted as the loss of market share into a given destination in a given year by an exporting country relative to other countries whose exports were also concentrated in the same industry. This estimation is restricted to the 44 countries that reported the composition of their exports in 1865, and they show that the direct comparison implies larger and more persistent losses (coefficients reported in Online Appendix Table G12, column (6) and plotted in Online Appendix Figure B10a).

Second, I broaden the measure of a country's export composition by using its geographic region as a proxy. I validate that geographic region is a reasonable proxy for the goods exported for the subset of 44 countries with observable industry composition in 1865. For each region, I identify the top three export categories by SITC codes and calculate the fraction of the total value of exports from the region that fall into those categories.[60] This fraction is equivalent to an exports-weighted average of the cross-country export concentration within the top three categories. Online Appendix Figure B11 shows that this fraction is above 0.5 for all regions and averages 0.73 across regions, indicating that the industry composition of exports is very similar within region.

I compare the countries within regions to each other by including origin-country region-year fixed effects in the baseline

59. As noted in Section III, the industrial composition of exports at the bilateral level is not systematically available, and hence it is not possible to use industry-origin-destination variation.

60. Each region has at least two countries, and the primary exports for all countries outside of northwest Europe are raw commodity goods.

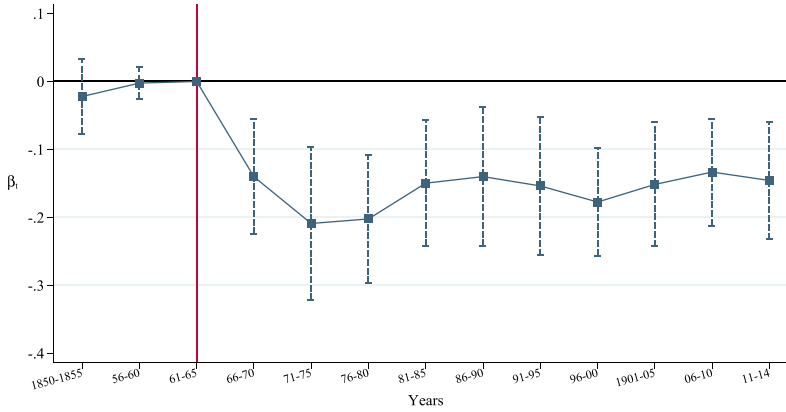


FIGURE VI

Persistent Effects within Groups of Countries with Similar Exports
$\ln(\text{EX}_{odt}) = \beta_t \text{Fail}_o + \Gamma' X_{ot} + \gamma_o + \gamma_{dt} + \psi \text{Region}_{ot} + \theta_t \ln(\text{dist})_{od} + \varepsilon_{odt}$

Figure plots the point estimates and 95% confidence intervals for the specification given above estimated on the country-level panel of trade. β_t is the treatment coefficient on the effect of exposure to failed banks on exports in each group of years. Point estimates and standard errors are scaled by the mean of treatment, so the magnitudes should be interpreted as the effect for the average exporter. The dependent variable is the log value of exports. The specification includes origin-country region-year FE, origin country o FE, destination country-year dt FE, and time-varying controls for the bilateral distance between countries. Standard errors are clustered by the origin country. See Online Appendix Table G12, column (8) for the point estimates.

specification in equation (8). The additional controls restrict the variation such that β_t is estimated off comparisons of countries in the same geographic area exporting to the same destination in the same year. Figure VI (Online Appendix Table G12, column (8)) shows that there is no recovery in this setting. As robustness, I also reestimate the baseline with region-year fixed effects using the subsample of countries that have SITC information and verify that the patterns are similar (Online Appendix Table G12, column (7) and plotted in Online Appendix Figure B10b).

I also find evidence of this mechanism in the analogous estimation using the short-run port-level panel. I ask whether the average exposure of other ports within a country benefits a given port's trade, both overall and to a specific destination. I calculate the average exposure to bank failures, leaving out the port's own city of financing.[61] As in the country-level analysis,

61. This measure is calculated by removing each city of financing's contribution from the country-level exposure measure rather than simply leaving out

$\psi > 0$ indicates that a port benefits when it is in a country where the rest of the ports are more exposed. Online Appendix Table A5, columns (1) and (2) present the result for total exports from a port.

The sustained persistence of the effects within regions are not driven by the smaller sample comparisons. In a robustness check, I conduct an additional Fisher exact test for the regional assignments of countries by simulating 1,000 random group assignments and reestimating the coefficients. I plot the distribution of the five-year coefficients in Online Appendix Figure G5. This figure shows that after 1900, the true coefficients are larger in magnitude than the average simulated coefficient, which implies that the true effects within regions are more persistent than random groupings would generate.

VI.B. Heterogeneity

1. *Lower Financial Needs.* First, shorter routes are likely less expensive to finance because goods spend less time in transit, implying that trade between more distant partners will decline relatively more.

I test this prediction using the panel of country-level values of trade by allowing for the exposure to the financial shock to differentially affect trading partners that are physically closer. I construct a binary variable *Close* to indicate country pairs that are less than the average distance between countries trading in 1865 and interact it with the origin-country exposure to the financial shock.[62] Online Appendix Figure B12 plots the baseline effect of exposure β_t in orange and the additional effect of failure for close relationships $\theta_{t,close}$ in blue. β_t is very similar to the baseline effect in previous estimations. $\theta_{t,close} > 0$ indicates that conditional on exposure to bank failures, exports to closer destinations are positively affected. The main effect for exports to close destinations is given by $\theta_{t,\,close} + \beta_t$, which is close to zero. The qualitative interpretation is that a country's export losses are borne by more distant trading partners.

Second, shorter institutional distance, as proxied by trade between two countries in the same colony, may also be less

the port's exposure so as not to double-count cities that financed more than one port. The specification is: $\ln(S_{pot}) = \beta \text{Fail}_{po} \times \text{Post}_t + \psi \overline{\text{Fail}}_{other,o} \times \text{Post}_t + \alpha_p + \Gamma' X_{pot} + \varepsilon_{pot}$.

62. The specification is: $\ln(\text{EX}_{odt}) = \theta_{t,close} \text{Fail}_o \times \mathbb{1}(\text{Close}_{od}) + \beta_t \text{Fail}_o + \lambda_t \mathbb{1}(\text{Close}_{od}) + \Psi' X_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}$.

expensive to finance. I test this prediction in a similar way by constructing an indicator variable for country pairs that are trading with their colony or colonizer. The baseline coefficients for *Failure* are plotted in orange and the additional heterogeneity in blue in Online Appendix Figure B13. These results show that exposed countries lost market share on average (the baseline negative effects in orange) but were able to compensate for some of those losses with colonial trade (the blue positive effects).

2. *Access to Alternative Sources of Financing.* Exporters that were not funded just by British banks would have been more likely to be able to draw on these lines of credit during the crisis, thereby shielding themselves from the higher marginal costs from British bank failures.

I use the port-level panel to test this hypothesis in the short term using within-country variation. I do not observe non-British financing relationships directly, so I proxy for them using the number of non-British banks precrisis. I reestimate equation (5) with an interaction term between exposure to failure and the number of non-British banks.[63] ϕ is the main coefficient of interest and captures the additional effect of failure on locations with non-British banks. Online Appendix Table A5 (columns (3) and (4)) shows that having access to more non-British banks precrisis mitigated the main losses ($\phi > 0$). The magnitude of ϕ is 53% of the baseline effect. The average port had access to 0.5 non-British banks, so assuming that non-British banks were as effective as British banks in providing trade financing, this access to other bank-intermediated finance mitigated the main effect by 26%.[64]

3. *Benchmarking the Magnitudes.* This article shows that the effect of the financing shock on exposed exporters' aggregate export levels is permanent and that the market share losses last for several decades. These persistent effects reflect changes in the shape of trade where exposed countries lost market share to unexposed countries. In a final exercise, I benchmark the total losses in exports against existing estimates at shorter horizons in the trade and macroeconomic literatures. Although there is

63. The specification is: $\ln(S_{pot}) = \beta \text{Fail}_{po} \times \text{Post}_t + \phi \text{Fail}_{po} \times \text{non-Brit}_{po} \times \text{Post}_t + \alpha_p + \gamma_{ot} + \Gamma' X_{pot} + \varepsilon_{pot}$.

64. Data on non-British banks are from Xu, Meissner, and Kisling (2020).

no comparable long-run study, the immediate and medium-run dynamics yield similar magnitudes.

The long-term effects of the shock on a country's total exports stems from two components: a large immediate difference in growth rates (consistent with the short-run intensive- and extensive-margin effects) and a lack of additional positive growth to compensate for the initial divergence in levels.

First, I benchmark the immediate growth rate difference against estimates of the elasticity of trade with respect to geographic distance and information frictions (see Anderson and Van Wincoop 2004 for a review of the importance of these and other frictions). The difference in postcrisis growth rates of aggregate exports after two years is 7.9% for unexposed countries relative to countries with average (9 percentage points) exposure. Using my data set, I estimate a trade cost elasticity of −1.2 to geodesic distance.[65] Given this elasticity, the effect on total exports from the average level of exposure is equivalent to increasing a country's geographic distance to its trading partners by 6.6%. Separately, the aggregate loss of 7.9% is similar in magnitude to Steinwender (2018)'s finding that connecting the transatlantic telegraph resulted in an immediate efficiency gain equivalent to 8% of export values.

Second, there is no compensating growth even though annual growth rates reconverge almost completely within 10 years, leaving a permanent effect on levels. These dynamics of lack of recovery in levels are consistent with the medium-term empirical evidence that crises lower the levels of output relative to a no-crisis counterfactual (Cerra and Saxena 2008).[66]

Note that the difference-in-differences partial-equilibrium analysis in this article cannot speak to the general-equilibrium effects for aggregate global trade, and in particular does not provide a counterfactual for the total amount of world trade lost due to the crisis. The persistent relative market share losses also do not imply negative growth rates. Therefore, simply adding the

65. This elasticity is, coincidentally, almost exactly the average elasticity found in the literature based on the survey of structural gravity by Head and Mayer (2014). Online Appendix Table A6 reports the estimates and robustness to controlling for gravity measurements of bilateral resistance.

66. While there are instances of countries exhibiting higher growth after a shock to recover losses in levels (notably historically in the United States), this pattern is the exception rather than the norm. Cerra, Fatás, and Saxena (forthcoming) provides an in-depth review and synthesis of this long literature.

partial-equilibrium losses across countries (as is often done with more macro estimations), likely overstates the aggregate global effect.

VII. Conclusion

Despite the prevalence of financial shocks and the general consensus that they affect short-run outcomes, there is little causal evidence that these temporary events can have long-run effect on the patterns of economic activity. This article uses a salient historical setting and novel archival data to provide such causal evidence that trade patterns can be disrupted for decades. The first modern global banking crisis serves as a laboratory where London's role as the global financial center means that bank failures in London were transmitted to cities and countries around the world. Exposure to bank failures caused large immediate declines in exporting activity on both the intensive and extensive margins within and across countries. Ultimately, countries exposed to larger degrees of bank failures experienced permanently lower aggregate exports and market share losses in their export destinations that persisted for four decades.

These persistent effects can be understood in a framework where establishing trade relationships entails significant sunk costs. Exporters exposed to the financial shock during the cusp of a major expansion in globalization were disadvantaged relative to their competitors. The patterns of substitution across trade partners provides further evidence for the importance of being competitive in world markets during this critical juncture in history.

The slow postcrisis recovery among advanced economies in recent decades suggests that the historical record continues to be relevant for understanding long-run slowdowns. Although this article has provided one set of magnitudes in the context of international trade, gaining a broader understanding of how major shocks affect economies at longer horizons in other contexts would be a fruitful avenue for future research.

Stanford University

Supplementary Material

Supplementary material is available at the *Quarterly Journal of Economics* online.

Data Availability

Data and code replicating the tables and figures in this article can be found in Xu (2022) in the Harvard Dataverse, https://doi.org/10.7910/DVN/EIEQIS.

References

Adão, Rodrigo, Michal Kolesár, and Eduardo Morales, "Shift-Share Designs: Theory and Inference," *Quarterly Journal of Economics*, 134 (2019), 1949–2010.

Alessandria, George, Joseph P. Kaboski, and Virgiliu Midrigan, "The Great Trade Collapse of 2008–09: An Inventory Adjustment?," *IMF Economic Review*, 58 (2010), 254–294.

Allen, Treb, and Dave Donaldson, "Persistence and Path Dependence in the Spatial Economy," NBER Working Paper no. 28059, 2020.

Amiti, Mary, and David E. Weinstein, "Exports and Financial Shocks," *Quarterly Journal of Economics*, 126 (2011), 1841–1877.

Amiti, Mary, Patrick McGuire, and David E. Weinstein, "International Bank Flows and the Global Financial Cycle," *IMF Economic Review*, 67 (2019), 61–108.

Anderson, James E., and Eric Van Wincoop, "Gravity with Gravitas: A Solution to the Border Puzzle," *American Economic Review*, 93 (2003), 170–192.

———, "Trade Costs," *Journal of Economic Literature*, 42 (2004), 691–751.

Antràs, Pol, and C. Fritz Foley, "Poultry in Motion: A Study of International Trade Finance Practices," *Journal of Political Economy*, 123 (2015), 853–901.

Atkin, David, Amit K. Khandelwal, and Adam Osman, "Exporting and Firm Performance: Evidence from a Randomized Experiment," *Quarterly Journal of Economics*, 132 (2017), 551–615.

Bagehot, Walter, *Lombard Street: A Description of the Money Market* (London: Scribner, Armstrong & Company, 1873).

Baier, Scott L., and Jeffrey H. Bergstrand, "Bonus Vetus OLS: A Simple Method for Approximating International Trade-Cost Effects Using the Gravity Equation," *Journal of International Economics*, 77 (2009), 77–85.

Baldwin, Richard, "Hysteresis in Import Prices: The Beachhead Effect," *American Economic Review*, 78 (1988), 773–783.

———, "Hysteresis in Trade," in *Hysteresis Effects in Economic Models* (Berlin: Springer, 1990), 19–34.

Baldwin, Richard, and Paul Krugman, "Persistent Trade Effects of Large Exchange Rate Shocks," *Quarterly Journal of Economics*, 104 (1989), 635–654.

Barro, Robert J., and José F. Ursúa, "Macroeconomic Crises since 1870," *Brookings Papers on Economic Activity*, 2008 (2008), 255–350.

Beaumont, Paul, and Clémence Lenoir, "Building a Customer Base under Liquidity Constraints," McGill University Working Paper, 2019.

Berger, Daniel, William Easterly, Nathan Nunn, and Shanker Satyanath, "Commercial Imperialism? Political Influence and Trade during the Cold War," *American Economic Review*, 103 (2013), 863–896.

Berman, Nicolas, and Jérôme Héricourt, "Financial Factors and the Margins of Trade: Evidence from Cross-Country Firm-Level Data," *Journal of Development Economics*, 93 (2010), 206–217.

Bernard, Andrew B., and J. Bradford Jensen, "Why Some Firms Export," *Review of Economics and Statistics*, 86 (2004), 561–569.

Bignon, Vincent, Marc Flandreau, and Stefano Ugolini, "Bagehot for Beginners: The Making of Lender-of-Last-Resort Operations in the Mid-Nineteenth Century 1," *Economic History Review*, 65 (2012), 580–608.

Blanchard, Olivier J., and Lawrence H. Summers, "Hysteresis and the European Unemployment Problem," *NBER Macroeconomics Annual*, 1 (1986), 15–78.

Bleakley, Hoyt, and Jeffrey Lin, "Portage and Path Dependence," *Quarterly Journal of Economics*, 127 (2012), 587–644.

Borusyak, Kirill, and Peter Hull, "Non-Random Exposure to Exogenous Shocks: Theory and Applications," NBER Working Paper no. 27845, 2020.
Borusyak, Kirill, Peter Hull, and Xavier Jaravel, "Quasi-Experimental Shift-Share Research Designs," *Review of Economic Studies*, 89 (2022), 181–213.
Bricongne, Jean-Charles, Lionel Fontagné, Guillaume Gaulier, Daria Taglioni, and Vincent Vicard, "Firms and the Global Crisis: French Exports in the Turmoil," *Journal of International Economics*, 87 (2012), 134–146.
Campbell, John Y., and N. Gregory Mankiw, "Are Output Fluctuations Transitory?," *Quarterly Journal of Economics*, 102 (1987), 857–880.
Capie, Forrest H., "The Emergence of the Bank of England as a Mature Central Bank," in *The Lender of Last Resort*, Forest H. Capie and Geofffrey E. Wood, eds. (London: Routledge, 2004), 297–316.
Cerra, Valerie, Antonio Fatás, and Sweta Saxena, "Hysteresis and Business Cycles," *Journal of Economic Literature*, forthcoming.
Cerra, Valerie, and Sweta Chaman Saxena, "Growth Dynamics: The Myth of Economic Recovery," *American Economic Review*, 98 (2008), 439–457.
———, *Booms, Crises, and Recoveries: A New Paradigm of the Business Cycle and Its Policy Implications* (Washington, DC: International Monetary Fund, 2017).
Chaney, Thomas, "Liquidity Constrained Exporters," *Journal of Economic Dynamics and Control*, 72 (2016), 141–154.
Chapman, Stanley D., *The Rise of Merchant Banking* (New York: Taylor & Francis, 1984).
Chodorow-Reich, Gabriel, "The Employment Effects of Credit Market Disruptions: Firm-Level Evidence from the 2008–9 Financial Crisis," *Quarterly Journal of Economics*, 129 (2014), 1–59.
Chor, Davin, and Kalina Manova, "Off the Cliff and Back? Credit Conditions and International Trade during the Global Financial Crisis," *Journal of International Economics*, 87 (2012), 117–133.
Clapham, John Harold, *The Bank of England: A History*, vol. 1 (Cambridge: Cambridge University Press, 1945).
Davis, Donald R., and David E. Weinstein, "Bones, Bombs, and Break Points: The Geography of Economic Activity," *American Economic Review*, 92 (2002), 1269–1289.
———, "A Search for Multiple Equilibria in Urban Industrial Structure," *Journal of Regional Science*, 48 (2008), 29–65.
Dell, Melissa, "The Persistent Effects of Peru's Mining Mita," *Econometrica*, 78 (2010), 1863–1903.
Eaton, Jonathan, Samuel Kortum, Brent Neiman, and John Romalis, "Trade and the Global Recession," *American Economic Review*, 106 (2016), 3401–3438.
Eichengreen, Barry, and Douglas A. Irwin, "The Role of History in Bilateral Trade Flows," In *The Regionalization of the World Economy*, J. Frankel, ed. (Chicago: University of Chicago Press, 1998), 33–62.
Eichengreen, Barry, and Andrew K. Rose, "Staying Afloat When the Wind Shifts: External Factors and Emerging-Market Banking Crises," in *Money, Capital Mobility, and Trade: Essays in Honor of Robert A. Mundell*, Guillermo Calvo, Maurice Obstfeld, and Rudiger Dornbusch, eds. (Cambridge, MA: MIT Press, 2004), 171.
Feenstra, Robert C., "Gains from Trade under Monopolistic Competition," *Pacific Economic Review*, 21 (2016), 35–44.
Fillat, José L., Stefania Garetto, and Arthur V. Smith, "What Are the Consequences of Global Banking for the International Transmission of Shocks? A Quantitative Analysis," NBER Working Paper no. 25203, 2018.
Goldberg, Linda, and Cedric Tille, "Macroeconomic Interdependence and the International Role of the Dollar," *Journal of Monetary Economics*, 56 (2009), 990–1003.
Hanlon, W. Walker, "Necessity Is the Mother of Invention: Input Supplies and Directed Technical Change," *Econometrica*, 83 (2015), 67–100.
Head, Keith, and Thierry Mayer, "Gravity Equations: Workhorse, Toolkit, and Cookbook," in *Handbook of International Economics,* vol. 4, Gita Gopinath,

Elhanan Helman, and Kenneth Rogoff, eds. (Amsterdam: Elsevier, 2014), 131–195.
Iacovone, Leonardo, Esteban Ferro, Mariana Pereira-López, and Veronika Zavacka, "Banking Crises and Exports: Lessons from the Past," *Journal of Development Economics*, 138 (2019), 192–204.
Jenks, Leland Hamilton, *The Migration of British Capital to 1875* (New York: Knopf, 1927).
Juhász, Réka, "Temporary Protection and Technology Adoption: Evidence from the Napoleonic Blockade," *American Economic Review*, 108 (2018), 3339–3376.
Juhász, Réka, and Claudia Steinwender, "Spinning the Web: Codifiability, Information Frictions, and Trade," NBER Working Paper No. 24590, 2018.
King, Wilfred T. C., *History of the London Discount Market* (London: Routledge, 1936).
Krugman, Paul, "History versus Expectations," *Quarterly Journal of Economics*, 106 (1991), 651–667.
Kynaston, David, *Till Time's Last Sand: A History of the Bank of England 1694–2013* (London: Bloomsbury Publishing, 2020).
Maggiori, Matteo, "Financial Intermediation, International Risk Sharing, and Reserve Currencies," *American Economic Review*, 107 (2017), 3038–3071.
Manova, Kalina, "Credit Constraints, Heterogeneous Firms, and International Trade," *Review of Economic Studies*, 80 (2013), 711–744.
Melitz, Marc J., "The Impact of Trade on Intra-Industry Reallocations and Aggregate Industry Productivity," *Econometrica*, 71 (2003), 1695–1725.
Muûls, Mirabelle, "Exporters, Importers and Credit Constraints," *Journal of International Economics*, 95 (2015), 333–343.
Nunn, Nathan, "The Long-Term Effects of Africa's Slave Trades," *Quarterly Journal of Economics*, 123 (2008), 139–176.
———, "Historical Development," in *Handbook of Economic Growth*, vol. 2, P. Aghion and S. Durlauf, eds. (Amsterdam: North-Holland, 2014), 347–402.
Olivero, Maria Pia, and Yoto V. Yotov, "Dynamic Gravity: Endogenous Country Size and Asset Accumulation," *Canadian Journal of Economics*, 45 (2012), 64–92.
Orbell, John, *British Banking: A Guide to Historical Records* (London: Taylor & Francis, 2017).
Oster, Emily, "Unobservable Selection and Coefficient Stability: Theory and Evidence," *Journal of Business & Economic Statistics*, 37 (2019), 187–204.
Paravisini, Daniel, Veronica Rappoport, Philipp Schnabl, and Daniel Wolfenzon, "Dissecting the Effect of Credit Supply on Trade: Evidence from Matched Credit-Export Data," *Review of Economic Studies*, 82 (2014), 333–359.
Pascali, Luigi, "The Wind of Change: Maritime Technology, Trade and Economic Development," *American Economic Review*, 107 (2017), 2821–2854.
Reber, Vera Blinn, *British Mercantile Houses in Buenos Aires, 1810–1880* (Cambridge, MA: Harvard University Press, 1979).
Redding, Stephen J., Daniel M. Sturm, and Nikolaus Wolf, "History and Industry Location: Evidence from German Airports," *Review of Economics and Statistics*, 93 (2011), 814–831.
Redding, Stephen J., and David E. Weinstein, "Aggregating from Micro to Macro Patterns of Trade," NBER Working Paper no. 24051, 2017.
Reinhart, Carmen M., and Kenneth S. Rogoff, "The Aftermath of Financial Crises," *American Economic Review*, 99 (2009), 466–472.
Roberts, Mark J., and James R. Tybout, "The Decision to Export in Colombia: An Empirical Model of Entry with Sunk Costs," *American Economic Review*, 87 (1997), 545–564.
Schmidt-Eisenlohr, Tim, "Towards a Theory of Trade Finance," *Journal of International Economics*, 91 (2013), 96–112.
Schneider, Sabine, "The Politics of Last Resort Lending and the Overend & Gurney Crisis of 1866," *Economic History Review* (2021), https://doi.org/10.1111/ehr.13113.

Sequeira, Sandra, Nathan Nunn, and Nancy Qian, "Immigrants and the Making of America," *Review of Economic Studies*, 87 (2020), 382–419.
Sowerbutts, Rhiannon, Marco Schneebalg, and Florence Hubert, "The Demise of Overend Gurney," *Bank of England Quarterly Bulletin* (2016), Q2.
Steinwender, Claudia, "Real Effects of Information Frictions: When the States and the Kingdom Became United," *American Economic Review*, 108 (2018), 657–696.
The Standard (Buenos Aires), June 13 1866.
Xu, Chenzi, "Safety in the Gold Standard Era," Stanford University Working Paper, 2019.
———, "Replication Data for: 'Reshaping Global Trade: The Immediate and Long-Run Effects of Bank Failures'," (2022), Harvard Dataverse, https://doi.org/10.7910/DVN/EIEQIS.
Xu, Chenzi, Chris Meissner, and Wilfried Kisling, "International Banks: Re-Agents of Globalization?," Stanford University Working Paper, 2020.