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

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Abstract
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 exports financing abroad. Countries exposed to bank failures in London immediately exported significantly less and did not recover to pre-crisis trend levels. Their market shares within each destination were significantly lower for four decades. Decomposing the persistent losses shows that they primarily stem from lack of extensive margin growth relative to unexposed countries, as importers sourced more from new and unexposed trade partners. 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 classification: F14, G01, G21, N20.

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How much and for how long do financial crises impact the patterns of international trade? Trade relationships are highly stable and generally understood to be shaped by slow-moving comparative advantages 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 a given equilibrium and leave it permanently in another 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, studies usually examine short-run outcomes within one country or else combine episodes from a variety of 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 London banking crisis that disrupted trade financing in almost every country in the world. At the time, Britain was the center of the global financial system, and British banks operated in countries that accounted for 98% of world exports while providing over 90% of trade finance globally.² The crisis occurred in London but propagated 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 exports volumes across locations that were more or less exposed to British bank failures, before and after 1866.

The banking crisis in this paper 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 17% of multinational banks headquartered in London failed and ceased both domestic and foreign operations. Second, since these banks dealt almost exclusively in trade finance, their institutional similarities in funding sources, investment model, management structure, and operations mean that their failure affected exports through the same trade finance channel across locations, as opposed to the more omnibus bank shocks of the modern period.³ Third, in contrast to much of the existing literature on long-run outcomes that relies

¹There 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).
²Calculated using the locations and operations of British and non-British banks and values of exports across countries in 1865.
³While many papers with modern data are able to use bank balance sheet data and firm-bank linkages,
primarily on cross-sectional evidence, in this setting I can trace out the full dynamics over the years. This allows me to study the evolution of trade patterns both before the crisis to show the lack of differential pre-trends, and afterwards to examine the speed and dynamics of recovery (or lack thereof). Fourth, locations’ exposure to bank failures is continuously measured and based on observed bank activity, which has the added 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 impact 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 bank characteristics correlated with failure is a public connection to the firm Overend & Gurney. Crucially, Overend & Gurney was not itself involved in trade finance or trade-related activities, so this connection is unlikely to be correlated with exports 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 these banks were operating in, to the extent that those characteristics impacted exports. In a difference-in-differences setting, the identifying assumption is that there are no simultaneous shocks to a location that cause both its exports to decline and the banks operating there to fail (Borusyak, Hull and Jaravel, 2021). I provide a number of covariate balance tests of bank exposure to location characteristics, additional robustness checks, and graphical evidence for the lack of pre-trends to support this identifying assumption.

My analysis necessitated constructing several new datasets 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 it is difficult to directly observe trade finance, so they tend to estimate an overall “bank channel.” As noted in Ahn, Amiti and Weinstein (2011) these measurements are often uncorrelated with trade finance.
records that represent the distribution of pre-crisis 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 a panel of bilateral exports values spanning
the period 1850–1914. I complement these country-level measures with the 1865 pre-crisis
industry composition of exports from contemporary trade statistics reports. Fourth, within
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 cri-
sis in 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 exports volumes.
Countries exposed to a one standard deviation increase in bank failures have 8.2% lower
exports one year later. I find very similar point estimates when using within-country, across-
port variation after including 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, that could potentially drive the results. These results are robust to alternative speci-
fications, 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 afterwards and are less likely to engage
in international trade at all. 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 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 21% less each year from exporters with
a one standard deviation higher exposure to bank failures for four decades. There is almost
no reversion, and the estimated hysteresis is robust to controlling for a wide variety of
contemporary shocks and initial macro-economic conditions. Simulated placebo shocks fail
to recreate these patterns.

I show that these results are virtually unchanged after accounting for changes in mul-
tilateral resistance arising from a fully structural gravity model of trade (see Anderson and
Van Wincoop (2003)). I follow the procedure in Baier and Bergstrand (2009) implemented by Berger, Easterly, Nunn and Satyanath (2013) that uses a first-order log-linear approximation of changes in multilateral resistance. The results are very similar to the baseline approach, indicating that the reduced form controls for trade costs capture most of the meaningful variation in multilateral resistance.

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, i.e. the supply side of trade, but had no role in financing consumption. Therefore, unlike in 2008-9, this global financial crisis did not generate a collapse in global demand but rather was a supply shock.

To rationalize my various results, I provide a conceptual framework featuring sunk costs of establishing relationships and substitutability across exporters that predicts these long-run market share losses for exporters even holding demand constant. The main assumptions of the framework are consistent with the details of the institutional context that features highly substitutable goods (commodities trade), high sunk costs (slow communication), and intense competition (rapidly expanding trade networks in the 1860s and 70s).

Consistent with the framework, 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 pre-existing relationships. This new sourcing on the extensive margin by importers is a cause of the large and persistent divergence in total exports and market shares across countries.

In the final section of the paper, I provide further evidence that trade patterns changed in a manner consistent with the financing shock impacting countries’ costs of exporting. First, exporters that were likely to provide similar goods benefitted 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 are more shielded from the hysteretic effects. Third, relationships that relied less on trade finance, such as those that were physically closer, experience less persistent losses than more distant relationships.

Benchmarking the estimated effects against the importance of other barriers to trade and the impact of crises more generally shows that these results are of similar magnitudes.
Literature review

This paper 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 seminal work by Davis and Weinstein (2002, 2008), Redding, Sturm and Wolf (2011), and Bleakley and Lin (2012) provide early empirical evidence from various settings showing both recovery and persistence in urban agglomeration. With respect to international trade, Baldwin (1988, 1990); Baldwin and Krugman (1989) provide theoretical results of persistence arising from sunk costs. Existing empirical work show that firms’ and countries’ history of exporting predicts contemporary trade patterns (Eichengreen and Irwin, 1998; Bernard and Jensen, 2004) and that sunk costs are large (Roberts and Tybout, 1997), but none of these provides direct evidence from an exogenous shock, nor guidance on how long these effects can last. This paper also highlights a separate mechanism showing how the advantage of uninterrupted financial access while integrating into world markets during a period of globalization impacts cross-country trade patterns and could be viewed as a critical juncture for generating first-mover advantages (Krugman, 1991; Allen and Donaldson, 2018).

It also relates to the macroeconomic literature that has found that financial crises have particularly long-lasting effects on many components of the economy, such as output, consumption, and employment (Kaminsky and Reinhart, 1999; Barro and Ursúa, 2008; Reinhart and Rogoff, 2009; Jordà, Schularick and Taylor, 2013; Romer and Romer, 2018). Cerra and Saxena (2008, 2017) show that GDP dynamics following financial shocks (and recessions 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 rather 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). Both global banks and an international reserve currency can transmit financial conditions in the core to the periphery, thereby amplifying international credit cycles (Goldberg and Tille, 2009; Giannetti and Laeven, 2012; Amiti, McGuire and Weinstein, 2019). The setting of a major shock to the pre-WWI global hegemon in this paper illustrates how conditions in the dominant financial market affect real activity globally, particularly in sectors dependent on external capital flows.

Finally, it speaks to the debate on the role of finance in trade. Many studies use

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5 See Foley and Manova (2015) for a review of the literature pertaining to modern settings.
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., Iacovone and Zavacka, 2009; Manova, 2013; Muûls, 2015). 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, Fontagné, Gaulier, Taglioni and Vicard, 2012; Eaton, Kortum, Neiman and Romalis, 2016). I provide causal evidence using bank-level variation as in Amiti and Weinstein (2011); Paravisini, Rappoport, Schnabl and Wolfenzon (2014) from a context in which banks had a very clear role in financing trade and extend the analysis to every country over much longer periods.

The paper is organized as follows: the next section discusses the historical context, Section 2 discusses the historical data sources, and Section 3 describes the identification strategy. Section 4 summarizes the conceptual framework and reports the main results, and Section 5 provides evidence on the source of persistence and heterogeneous effects. Section 6 concludes.

1 Historical context

1.1 Trade finance & British banking dominance

Contractual and information frictions were a major barrier to establishing international trading relationships in the 19th 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 is staggered, and there is room for default on both sides. Importers are not willing to directly finance exporters through cash-in-advance payment when the exporter is risky and contractual agreements over quantity or quality are difficult to enforce (e.g., Antràs and Foley, 2015; Schmidt-Eisenlohr, 2013). These contractual frictions are particularly high for exporters in countries of low institutional quality or in new, riskier markets. Exporters waiting for payments face higher working capital costs, and contemporary 19th century accounts indicate that uncertainty over payments made it difficult for exporters to operate (Reber, 1979, p.75). Information frictions have also been found to be substantial impediments to trade flows both historically and today, and the historical setting featured significant communication costs (e.g., Steinwender, 2018; Anderson and Van Wincoop, 2004).
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. 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. 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. 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.

The primary instrument used to finance international trade were short-term, often collateralized, loans called “banker’s acceptances” or “bills of exchange.” 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 3–6 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.

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

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6For 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, p. 60-61).

7They 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.”

8The 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.”

9Contemporaries sometimes distinguished the term “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 paper, the two terms will be used interchangeably.

10For 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, p. 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, p. 69).
I interpret their presence as reflecting the supply of trade financing.\textsuperscript{11}

A banker’s acceptance also had a unique legal feature such that it was 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 re-sell ("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.\textsuperscript{12} This unique feature 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.\textsuperscript{13} 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. Since the BoE was still a firm whose banking operations profited its shareholders, the BoE strictly monitored the quality of its assets.\textsuperscript{14} In addition to using a bill’s history to ascertain its quality, the BoE also 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.\textsuperscript{15}

\textsuperscript{11}However, it is impossible to rule out that they were not also providing other sorts of financing that indirectly impacted international trade. Appendix section C.1 provides more qualitative descriptions of these banks’ businesses.

\textsuperscript{12}King, in \textit{History of the London Discount Market}, 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,” (King, 1936, p. xvi).

\textsuperscript{13}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.

\textsuperscript{14}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 19th and 20th centuries.

\textsuperscript{15}Bignon, Flandreau and Ugolini (2012) calculates the “amount at risk” on the Bank of England’s balance sheet over three crises in the 19th century, and they 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 BoE did not suffer losses from its discount window following these crises.
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.\textsuperscript{16} Discounting the bills from abroad allowed the head office to supply the foreign offices with fresh capital in return.\textsuperscript{17} Figure C1 documents the full life cycle of a bill of exchange, and Appendix C.1 provides more detail.

1.2 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 22 out of 128 multinational banks headquartered in London (12\% by assets) 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 also did not finance trade and therefore 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-19th 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, p. 246). However, 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, p. 239). Yet the new capital was not sufficient, and less than one year later on May 10, Overend announced its bankruptcy.\textsuperscript{18}

\textsuperscript{16}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).

\textsuperscript{17}The Eastern Exchange Bank described this cycle of financing between its headquarters in London and its office in Alexandria during its bi-annual 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.”

\textsuperscript{18}The proximate cause for bankruptcy that necessitated the loan was a court decision which 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 for a private loan, but the BoE declined to extend credit, claiming that Overend was insolvent. However, the relationship between the BoE and Overend had been contentious ever since Overend staged a mini run on the BoE in April, 1860, and some scholars believe this was the true reason that their request was rejected (Sowerbutts, Schneebalg and Hubert (2016) contains a discussion of this hypothesis).
After its failure, the shareholders 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 Appendix C.2.

As in the collapse of Lehman Brothers in 2008, Overend’s failure led to widespread panic and a flight to cash. 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.”19 The money market was completely frozen, and the BoE Discount Window was the only source of liquidity.20 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.21 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.22

The crisis in London was also widely reported around the world (see Appendix C.3.2) with the English language newspaper in Buenos Aires writing for example, “an alarming financial crisis has burst in England, threatening widespread misfortune [...] it is certain to affect all parts of the world in commercial relations.” In addition to the dislocation generated by the immediate panic, the BoE kept its discount rate at a punitive 10% for over three months afterwards, which further hindered recovery by making it difficult for banks to re-capitalise and was deeply criticized by contemporaries (Schneider, 2021).23

Ultimately, 22 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, therefore directly affected the supply of trade finance available in each city around the world that depended on these banks’ foreign operations.24

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19 Appendix C.3.1 provides many additional examples of contemporary newspaper reports of the crisis.
20 “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, p. 243).
21 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.
22 The full text of the letters exchanged on May 11 are in Appendix C.2. In them, the 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.”
23 Schneider 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.”
24 For example, the Commercial Bank failed in London May 15. The headquarter’s telegraph to its Bombay
2 Data

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

2.1 Bank characteristics

Lending pre-crisis: The Bank of England kept detailed records of every transaction that occurred at its Discount Window.\textsuperscript{25} I use the ledgers from 1865–1866 to build a dataset of over 11,000 individual loans from the 128 banks that had international operations in the year before the crisis.\textsuperscript{26} 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 of the loan, the bank that brought the bill in to be discounted, and the date it was brought to the Bank of England.\textsuperscript{27} These data allow me to calculate the total amount of financing by each bank in each city before the crisis.

Bank health: Individual bank failures were reported extensively in contemporary newspapers and recorded by the BoE in internal records. For publicly traded banks, balance sheets and narrative evidence of the banks’ risk-taking and financial health are gathered from transcripts of the bi-annual meetings of shareholders at the closest meeting before, during, and after the crisis. Names of the managers, directors, and partners of the banks were listed in

\textsuperscript{25}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 Appendix C.5.

\textsuperscript{26}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 post-crisis reallocation of credit. As 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 Appendix C.5.

\textsuperscript{27}An example of a ledger page is shown in 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.
financial almanacs, advertised in contemporary newspapers, and often mentioned in shareholder meetings.

2.2 Exports

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 measure of within-year, within-country measures of exports for the whole world, which makes port-level analysis possible. Drawbacks are that it does not report values of the goods onboard and is a British publication which 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 Figure B2. I provide additional robustness checks for this concern about mismeasurement in Appendix G.1.5.

There are over 400,000 unique shipping events that I digitized, geocoded, and aggregated to generate measures of exporting activity before and after the crisis. The dataset has two periods that aggregates shipping for the 1 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 post-shock periods. I also build a dataset that includes the ports that were ever active in the two periods, which allows me to measure entry and exit. Figure 1b maps the pre-crisis distribution of exporting activity measured by the log number of ships.

Country-level: I construct the country-level panel of bilateral trade values for 1850–1914 from publicly available datasets of historical trade statistics plus my own contributions. I standardize country definitions across datasets to 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 pre-shock industrial composition.

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28While it is technically possible to digitize the Lloyd’s List for the long-run analysis from 1850–1914, the scale of data collection required is beyond the scope of this paper.

29Origin 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 country-level. An example of this source from September 5, 1866 is shown in Appendix Figure B1b.

30Historical data on bilateral trade flows disaggregated by industry and year is unavailable for most countries and has mostly not been systematically collected.
3 Empirical strategy

The goal of my empirical analysis is to estimate the causal relationship between the supply of trade finance in a location and its exporting activity. I model the underlying relationship between bank finance and economic outcomes by relating the log of exports \( EX_l \) at location \( l \) to the log of the amount of trade financing available in a given period where the variable “\( Finance_l \)” would be measured by loan volumes:

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

Identifying \( \gamma \) from Equation 1 is challenging for two reasons. First, direct measures of trade finance reflect an equilibrium outcome that conflate supply and demand, so places that demand less trade finance are also likely to have less trade. Equation 1 will therefore not satisfy the orthogonality conditions that \( E[Finance_l\varepsilon_l] = 0 \) because \( \varepsilon_l \) includes the unobserved local economic conditions that are positively correlated with trade finance that biases \( \gamma \) upward. Second, there might be reverse causality: firms in locations that are already less productive can weaken their banks’ balance sheets through non-performing loans and cause those banks to contract their lending or even to fail.

I overcome these challenges by combining the unique dataset 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 trade financing.

3.1 Measuring the exposure to bank failures

The crisis in London generates bank-level shocks that affect locations through their dependence on each bank. Bank shocks are captured by the binary variable \( \mathbb{I}(\text{Failure}_b) \) that takes the value of 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 trade financing in location \( l \): \( z_{lb} = \frac{\text{Finance}_{lb}}{\text{Finance}_l} \), measured in the pre-crisis period. These shares sum to 1 in each location. The cross-product of these two terms gives each location’s exposure to bank failure \( \text{Fail}_l \):

\[
\text{Fail}_l = \sum_b z_{lb,\text{pre}} \times \mathbb{I}(\text{Failure}_b) \tag{2}
\]

\( \text{Fail}_l \) takes the form of a Bartik instrument where the pre-crisis importance of each bank to a location \( (z_{lb,\text{pre}}) \) are the “shares,” the bank failure rates are the “shocks,” and trade
finance \((Finance_i)\) is the endogenous variable. I provide the derivation of the instrument in Appendix D and discuss instrument validity in Section 3.2.

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

\[
\ln(EX_{lt}) = \beta (Fail_l \times Post_t) + \Gamma'X_{lt} + \epsilon_{lt}
\]  

(3)

The coefficient \(\beta\) in Equation 3 is the semi-elasticity of the response of trade activity to British bank failures in location \(l\) in the post-crisis \((Post_t)\) period.\(^{31}\)

Figure 1a maps the geographic distribution of exposure to bank failures, \(Fail_l\) at the city level. The size of the points measures the pre-crisis 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. Figure B4 plots the distributions of exposure across ports and countries and shows representation across the entire range. Table 1 reports the descriptive statistics for ports and countries in 1865. The average port had 128 ships leaving in the pre-crisis period and 7 pp exposure to failed banks. The average country exported £12.5 million and was exposed to 11 pp bank failures.

### 3.2 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: \(E[Fail_l \epsilon_l] = E[\sum_b z_{lb} I(Failure_b) \epsilon_l] = 0\). 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 in the locations most exposed to each bank (Borusyak, Hull and Jaravel, 2021). 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.\(^{32}\) One example of problematic sorting would be if banks that failed systematically operated in locations that experienced a boom in the pre-period.

\(^{31}\)Data limitations prevent estimating the full first stage. I estimate a pseudo first stage with the available data in Table A1, which is representative of all banks (Table A2). I provide more details in Appendix A. 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 providers of trade financing, I focus on this interpretation, but other forms of banking activity that impacts exporters would also be affected by bank failures and captured by \(\beta\).

\(^{32}\)Borusyak and Hull (2020) discuss issues arising from non-random exposure to shocks even in the case of linear instruments, but those do not apply to this setting because the shares sum to 1.
and a bust post-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.

### 3.3 Determinants of bank failures

**Quantitative measures & narrative evidence**

Banks are balanced across almost all observable pre-crisis bank characteristics (Table 2). 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’ bi-annual shareholder meetings that cover their operations from 1865 to 1867, which 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 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 (Table A3). Banks in the two groups are similarly geographically diversified, operating in an average of almost 14 cities and 8 countries. This balance helps to address the concern that bank failures and export contractions were simultaneously caused by a shock that was systematically correlated with their geography.33

**Overend & Gurney connection**

The primary explanation for why some banks failed is 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.34 The shareholder list circulated in London at

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33Examples of such shocks include weather patterns that affected agricultural output or regional boom-and-bust patterns.

34Specifically, they had only paid in £15 of the £50 nominal and could be obligated for the remaining £35
2.5 times the publishing price during the crisis, and contemporary evidence indicates that depositors found this a valuable piece of information.\textsuperscript{35}

I digitize the shareholder list from January 1866 (Figure C2a) and compare it to the names of the managers and directors of the London banks (Figure C2b).\textsuperscript{36} 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 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 2 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 (Table C2).\textsuperscript{37}

3.4 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 (2021) and calculate each bank’s exposure to those characteristics and correlate them with the bank failure rates.\textsuperscript{38}

I examine the observable pre-crisis location-level characteristics at both the port-level and the country-level, since 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 the \textit{Lloyd’s List}), importance of the United Kingdom as a destination, geodesic distance to London, latitude, number of destinations, availability of non-British financing, per share at any moment. £35 in 1866 is equivalent to £4,193 in 2020.

\textsuperscript{35}See Appendix C.3 for contemporary documentation of the crisis and the demand for the shareholder list.

\textsuperscript{36}The shareholder list was found at the Royal Bank of Scotland archives in Edinburgh, Scotland. January, 1866 was the last list that as compiled before the firm declared bankruptcy.

\textsuperscript{37}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 being held on the balance sheet—Overend’s failure would not have directly impacted 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.

\textsuperscript{38}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 Adao, 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 over-reject 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 (2021) show that implementing the Adao, Kolesár and Morales (2019) standard error correction is equivalent to translating the location-level characteristics into bank-level exposure rates.
and whether the port is a capital city. At the country-level, observable characteristics include total value of exports, exports growth rates pre-crisis, 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 these pre-crisis characteristic $X_l$ is calculated as $\bar{X}_b = \frac{\sum_l \hat{z}_{lb} X_l}{\sum_l \hat{z}_{lb}}$ 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 $\bar{X}_b$:

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

Table 3 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 UK were more likely to fail, and those operating in ports that were also the capital cities within 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 3 Panel B shows that banks that failed did not systematically operate in countries with lower exports values, with a heavier reliance on commodities, or with exposure to military conflicts. There is also no correlation between exposure to different currency standards (gold, silver, or bimetallic) and bank failures.

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. The full distribution of exports by SITC categories is plotted in Figure B6. Table 3 Panel C shows that banks that failed are not differentially exposed to the top eight industries of raw cotton exports, cotton manufactured goods, bullion, grains, coffee, alcohol, and tobacco. In Appendix Table G18, I provide balance checks for all remaining SITC industries. Finally, I check that these characteristics are not jointly significant (Appendix Figure B3) and find an F-stat of 1.31 with p-value 0.17.

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

40 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 (2021).
4 Results

4.1 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.\textsuperscript{41} 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.\textsuperscript{42} Firm profits are an increasing function of productivity, financing supply in the exporter’s location, and inverse trade costs. 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 (1) export less (because their prices have increased) and (2) be less likely to enter new markets (because they are less profitable). They may not exit a market because re-entry in the future will require paying the sunk costs again, so there is an option value to staying. In a destination, importers will indirectly experience their trade partners’ financing shock through an increase in their price index. Higher price indices raise average profits, and (3) firms will enter a destination and increase the number of relationships importers have. Post-shock, 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, the (4) market share losses can persist.\textsuperscript{43}

\textsuperscript{41}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, 2018), persistence (or multiple equilibria) arises out of agglomeration forces. In all cases, large enough shocks can shift the equilibrium, either due to sunk costs investments or by allowing the agglomeration forces to entrench elsewhere.

\textsuperscript{42}Chaney (2016) and Manova (2013) also both 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 constrains some firms that are productive enough to export from doing so.

\textsuperscript{43}In addition, exporters learn about their trade partners, become more productive, and direct their investments to accommodate demand. There is much empirical evidence that the quality and value of trade relationships improves over time with learning. In the context of the 19th century Juhász and Steinwender (2017) 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 itself improves firm productivity
Following Redding and Weinstein (2017), the underlying firm-level dynamics described in the conceptual framework can be aggregated and then analyzed using these more macro measurements of trade. Appendix E provides more discussion of this point.

4.2 Immediate effects

Intensive margin

I examine the immediate impact of bank failures on the intensive margin of exports by restricting the analysis to locations that are active both 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-difference regression with continuous treatment intensity:

\[
\ln (S_{ot}) = \beta (Fail_o \times Post_t) + \gamma_o + \Gamma' X_o \times Post_t + \varepsilon_{ot}
\] (5)

\(S_{ot}\) is the total number of ships departing a country per period. \(\beta\) is the coefficient of interest, and \(Fail_o\) is an exporting country’s exposure to bank failures calculated according to Equation 2 using country-level shares of pre-crisis bank dependence. It is continuously measured from zero to one. \(Post_t\) is an indicator for the post-crisis period that controls for macroeconomic shocks affecting the export trends over time. \(X_o\) are pre-crisis country characteristics that can be included as additional controls when interacted with \(Post_t\). Country fixed effects \(\gamma_o\) absorb all time-invariant differences in levels of shipping, including those correlated with their exposure to bank failures. Regressions are weighted by the pre-crisis size of country-level shipping activity. Standard errors are clustered by country of origin.

Table 4 column 1 shows the baseline effect without any additional country-level controls. The coefficient of -0.51 implies that countries with an average exposure of 11pp exported 5.6% less than non-exposed countries in the post-crisis year. Appendix Table A4 adds origin-country characteristics as controls to show that the results are not affected by differences in initial macroeconomic conditions, such as the industry composition of exports.

It is possible that unobserved country-level shocks are partly accounting for the results, so next 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 \(t\):

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

through learning-by-doing.
Each port in the panel is matched to the closest city of financing by geodesic distance, and its exposure to bank failures $\text{Fail}_{po}$ is assumed to come from that city.\textsuperscript{44} Ports more than 500 km from the nearest city of financing are part of a control group of completely unexposed ports and given an exposure of zero.\textsuperscript{45} 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 pre-crisis port characteristics that can be included as additional controls. Port fixed effects $\alpha_p$ absorb all time-invariant port-specific differences in levels of shipping, and origin-country-by-post fixed effects $\gamma_{ot}$ flexibly control for all observed and unobserved changes at the country-level that affect shipping, such as GDP and multilateral resistance.\textsuperscript{46} Including these fixed effects means $\beta$ is identified by comparing ports from the same country and year.\textsuperscript{47} Standard errors are clustered by the country of origin to allow for heteroskedasticity and within-country spatial correlations.

Table 4 Columns 2–4 present the baseline results with fixed effects and controls added sequentially. The country-by-post fixed effect does not significantly impact magnitudes, indicating that there were unlikely 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 3 and include the average age of the banks, the Overend & Gurney connection, whether the port is the capital city, and the fraction of ships going to the UK in the pre-crisis year.\textsuperscript{48} The coefficient of -0.64 in Column 4 with all the controls indicates that a one standard deviation of exposure (19 pp) reduces exports by 12% in the post-crisis 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 Appendix Table G1, I add each control variable in turn and show that the coefficients remain stable and statistically significant.\textsuperscript{49} I also implement the recommended bounds in Oster (2019) to show that selection on location-level unobservable characteristics

\textsuperscript{44}For example, the port of Piraeus in Greece is designated as receiving its funding from Athens.

\textsuperscript{45}I include an indicator for these ports interacted with the post time period. The results are not sensitive to the 500 kilometer boundary and the main coefficients are robust for a range of distances and to not including the time-varying intercept for distant ports. See Figure B7 for the coefficient plot for the baseline specification estimated using different distance cutoffs.

\textsuperscript{46}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.

\textsuperscript{47}There are on average 5 ports per country. Countries with only one port are effectively dropped from this estimation. These account for 8 of the 289 ports (2.8 percent), which reduces the effective number of countries in the estimation from 54 to 46.

\textsuperscript{48}Bank-level characteristics are aggregated to the port-level using the pre-crisis shares $z_{lb,pre}$ of the importance of each bank to each location.

\textsuperscript{49}In Appendix Table G3, I create this table without country fixed effects to provide a comparison of the magnitudes with and without them.
is minimal. These calculations show that the degree of unobservables bias would have to be at least 40 times larger than the degree of observables bias. The Fail measure is skewed so in Appendix Table G6, I provide a breakdown of the effect roughly by terciles.

Table 4 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; column 6 accounts for potential mismeasurement in the outcome variable by re-estimating all the results using count data methods; and column 7 uses the time-series granularity of the Lloyd’s List and to allow for communication lags between London and cities around the world. The estimated effects are very similar across these specifications. Appendix G.1 provides extensions of each result (Table G8 for cotton and G9 for news lags). In addition, I show that the baseline effects are robust to controlling for exposure to different regions (Table G2) and are not due to demand shocks both specifically from the UK and more generally (Table G7, G11); I limit the sample to well-traveled routes to diminish the impact of outliers along routes (Figure G2, Tables G10 and G12); I remove ports in countries that are islands and entrepots (Table G13) and I show similarity to annual regressions using country-level values of exports (Table A7).

**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 (estimated without any controls) in Table 4 column 1) into the change in the number of destinations (Table 5 column 1) and the change in average exports per destination (Table 5 column 2). This change in the number of destinations (column 1) is my first measure of extensive margin changes, estimated using the specification in Equation 5. This decomposition shows that while both margins contribute to the drop in total exports, most of the drop is accounted for by the fact that more exposed ports end up with relatively fewer partners.\(^{50}\)

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 post-shock periods. I categorize ports as “Entering” into international trade if there is no exporting activity in the pre-crisis period and positive exports in the post-crisis 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

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\(^{50}\)Note that in a difference-in-differences setting, this result does not necessarily imply a drop in the number of destinations but rather that exposed locations gained relatively fewer in the post-period. Appendix Table A5 reports all margins of the decomposition together.
Entry or Exit and standard errors are clustered by the origin-country:

\[
\text{Pr}(E_{po}) = \alpha + \beta \text{Fail}_{po} + \gamma o + \Gamma'X_{po} + \varepsilon_{po}
\] (7)

Table 5, columns 4 and 6 present the within-country likelihood of Entry\textsubscript{po} and of Exit\textsubscript{po}, respectively. The shock is a statistically significant deterrent to the extensive margin ability to establish new links with foreign markets, but it does not significantly impact exit by those who had already paid the sunk costs.\textsuperscript{51}

Taken together, these results show within and across-country evidence of the immediate impact of the financing shock on exports. The majority of the effect comes from lack of extensive margin growth relative to unexposed exporters.

### 4.3 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.

**Total exports**

There is a permanent divergence in the cumulative growth of total exports across countries. In Figure 2a, 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, and levels for each group are indexed to one in 1866. The blue line 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 pre-trends between the groups, but after 1866 there is an immediate divergence in levels that does not recover. Figure 2b graphs the difference between the two groups and Figure B11 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 1867–1870, after which they level off at approximately -0.5. These coefficients imply that an exporter with 11 pp (average) exposure has 20.6\% persistently lower levels of total exports than in the unexposed counterfactual.

**Bilateral exports & market-share effects**

\textsuperscript{51}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), it is the extensive margin of exports that generates changes in aggregate trade flows in response to valuation shocks.
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 imports growth after the crisis, their exports would be affected, but not through the financing channel. My baseline estimation therefore uses bilateral exports volumes and controls for annual demand shocks from importers:

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

The dependent variable is the log value of exports $EX_{odt}$ (in nominal pounds sterling) 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 over time. As in the port-level specifications, it is continuously measured from zero to one. $X_{ot}$ includes pre-crisis origin-country characteristics that are interacted with year dummies, which control for macroeconomic differences among countries. Origin-country fixed effects $\gamma_o$ 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. Destination-country year fixed effects $\gamma_{dt}$ control for demand shocks by restricting the identifying variation to being across exporters, within-destination-year. I control for the distance between countries $\text{dist}_{od}$ measured either geodisically or as the fastest travel time. 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.

I allow $\beta_t$ to vary annually and at five-year intervals ([1850, 1855], ..., [1911, 1914]). $\beta_t$ 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 off 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. It is expected to be negative if increases in the exposure to bank failures reduces exports.

Figures 3a and 3b plot the estimated $\beta_t$ coefficients annually and at five-year intervals, where $\beta_{1866}$ and $\beta_{1861−5}$ 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.  

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

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

54 Results are robust to different ways of clustering in Table A7.

55 The average level of exposure is 11 pp. In Appendix Figure B8, I also plot the estimated coefficients
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 post-period, $\beta_{\text{post}}$ is -1.10 log points. This magnitude implies that the average importer bought 11\% less from an exporter with one standard deviation exposure (17 pp) relative to an unexposed exporter. Given the average exposure of 11 pp, this estimation implies that the (partial equilibrium) disruption in the shape of global trade was 7\% every year year in the post-crisis period until WWI.

The estimated coefficients support the patterns in the raw data for total exports that exposure to the crisis had no effect on exports pre-crisis but immediately lowered trade flows between countries afterward. The lack of pre-trends is also relevant for addressing the potential identification concern that stronger banks were systematically better at choosing their operating locations and therefore tended to operate in better places. Although the balance on observables discussed in Section 3.4 suggests this is not the case, one might still be concerned that certain locations were on a better exporting trend for unobserved reasons and that non-failed banks were systematically located there. This sort of selection bias would lead to a pre-trend 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.

**Structural gravity**

While the reduced form estimation in Equation 8 controls for the average differences across exporters ($\gamma_\alpha$), it does not fully accommodate the annual changes in multilateral resistance that countries experience in general equilibrium, where a change in the bilateral trade costs between any two countries will impact 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 not directly observable, but using exporter and importer country fixed effects in cross-sectional estimations (Hummels, 2001; Feenstra, 2016) or exporter-year and importer-year fixed effects with panel data (Olivero and Yotov, 2012; Head and Mayer, 2014) delivers consistent estimations of trade cost variables.

Equation 8 differs from the fixed effects implementation of structural gravity because it cannot include origin-country-year fixed effects, as those are collinear with treatment. I at the median, 75th, and 90th percentiles. Appendix Figure B9 plots the unscaled results, and those point estimates are reported in Table G14 (column 2). Appendix Figure B10 decomposes the effect roughly by terciles of exposure.

56The standard error is 0.349, implying a t-stat of 3.14.

57These time-varying country level fixed effects ($\gamma_{dt}$ and $\gamma_{ot}$) also have the advantage of being easily interpretable as absorbing all observable or unobservable country-specific characteristics that shift the overall level of exports and imports of a country.
therefore follow the technique introduced by Baier and Bergstrand (2009) and implemented in Berger, Easterly, Nunn and Satyanath (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 that emerge from structural gravity. The $\beta_t$ coefficient still captures the reduced-form impact of the financing shock on an exporter’s market share after accounting for the component of trade that is explained by the gravity model. 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 is not available for all countries, so the results are estimated on a smaller set of observations. Appendix Table F1 shows the results from adding GDP controls and this structural approach along with the baseline estimated on the same subsample of observations. The estimated effects with these alternative approaches are even larger in magnitude and even more persistent, although not statistically different from the baseline. 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 indicates that unobserved bias arising from not directly controlling for changes in multilateral resistance is small. Appendix Figure F1 provides a visual comparison of the estimates from my baseline and this structural approach. I view the structural approach as a secondary set of results given the smaller sample size and lower quality of GDP data.

No effect on imports
Exposure to bank failures does not impact a country’s imports, consistent with the institutional context that banks were not financing consumption. I estimate the impact 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. As in the baseline, I saturate the estimation with fixed effects to account for exporter supply shocks ($\gamma_{ot}$) and the importer’s overall size ($\gamma_{dt}$). I present these

\begin{align*}
58 \text{Destination country-size is already controlled for with the fixed effects.} \\
59 \text{The 95\% confidence intervals on the estimates after controlling for multilateral resistance overlap with the baseline coefficients, but the increased noise is partly due to the smaller sample size.} \\
60 \text{Given the symmetry in trade flows, one country’s exports is its trade partner’s imports. Therefore estimating the impact 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 exported to country A. The equation of interest, } \ln(IM_{dot}) = \beta_t \text{Fail}_o + \varepsilon_{odt} \text{, is equivalent to } \ln(EX_{odt}) = \beta_t \text{Fail}_d + \varepsilon_{odt}. \text{ Note that in the notation } IM_{dot}, \text{ the goods are traveling from country } o \text{ to country } d \text{ at time } t.
\end{align*}
results in Table A8. Column 2 shows that exposure to bank failures has no impact 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.61

Robustness
I provide several robustness checks for the long-run exports effects in Appendix G.2. First, I control for a wide variety of initial and contemporaneous macro-economic conditions that could potentially explain the effects. In Table G14 columns 3–8, I show robustness to origin-country controls, including the pre-crisis characteristics that are correlated with bank failures. In Table G15, I report the estimates after including standard gravity covariates, such as shared language, shared land border, and being in the same European empire. Additional robustness includes controlling for pre-crisis and contemporary military conflicts and exchange rate regimes pre-crisis (Table G16); industry composition of exports pre-crisis and initial trade intensity with the UK (Tables G17 – G19); excluding the cotton-exporting countries (Table G20); financial crises like sovereign debt, domestic debt, stock market crashes both contemporaneous and in 1865 (Table G21 and G22); the trade cost changes from the Suez Canal opening in 1869 (Table G24);62 and excluding islands and entrepots (Table G25). 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. These controls rule out the possibility that these other events were the actual drivers of the persistent collapse in exports 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 Figure G3, I plot the distribution of the estimated coefficients as well as the distribution of the associated p-values. These show that not only are the magnitudes of the coefficients robust, but also 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 to address the potential concern that the results are driven by a small number of countries that experienced unobserved positive shocks after 1866.63

61Directly 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 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.

62The 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).

63Note that the immediate effects are estimated using within-country variation, which already indicates this is unlikely to be the case.
Finally, I test the robustness of the long-term results by implementing 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 hundreds or thousands of placebos. At longer time horizons, countries’ exports could be affected by 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 banking 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, then estimate the long-term effects in Equation 8 using the simulated data. I plot the distribution of the coefficients for each group of five years in Figure G4. These plots show 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.

5 Changes to the shape of trade

5.1 New sourcing by importers

To better understand the source of the persistent market share losses by exposed countries, I examine how their trade partners—i.e. importers—reacted when their trade partners were shocked. Guided by the conceptual framework, for each importer, I proxy for the financing shock’s impact on the importer’s own price index using its pre-crisis trade partners’ exposure. Destinations reliant on more-exposed exporters are indirectly exposed to their trade partners’ bank failures because the change in financing costs will either be 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 therefore induce new relationships to form.

I first examine the impact of indirect exposure on a country’s aggregate imports.\(^{64}\) I plot these results in Figure 4a. I find that more indirectly exposed importers did not systematically buy less than less indirectly exposed importers. Next, I estimate and decompose

\(^{64}\)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 \(\text{Fail}_{d,\text{pre}}\) by weighting a country \(d\)'s trade partners' exposure \(o\) using the three years of trade flows prior to 1866. The specification for the impact of indirect exposure on total imports is: \(\ln(IM_{dt}) = \beta_t \text{Fail}_{d,\text{pre}} + \gamma_d + \gamma_t + \varepsilon_{dt}\). The results are virtually unchanged by using trade shares calculated with data from 1–5 years pre-crisis.
the effects on aggregate imports into the effect on the number of trade relationships and the average imports per relationship. Total imports were not significantly impacted, but indirect exposure did lead 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 who lost market share. A second decomposition of the aggregate imports into the share of total imports from new trade partners formed after the shock shows that pre-existing trade partners (with high exposure) experience a sharp decline in total market share. I plot these results in Figure 4b.

Comparing substitutable exporters
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 19th century, most countries exported commodities that were often produced in multiple locations, suggesting there was 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. Given the initial sunk costs, 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 pre-crisis, categorized by two-digit SITC codes, to test for importers substituting among similar countries. 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 $\beta_t$ 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 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 Table G14 column 6 and

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65 These specifications follow the same form as the previous one, with the different dependent variables as noted.
66 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 pre-existing exporters relative to before the shock, but just that the denominator of total imports is growing faster than the numerator.
67 As noted in Section 2, 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.
68 The global value of exports by SITC is shown in Figure B6.
plotted in Figure B12a).

Next, 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. This fraction is equivalent to an exports-weighted average of the cross-country export concentration within the top three categories. Figure B13 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 specification in Equation 8. The additional controls restrict the variation such that $\beta_t$ is estimated off comparisons of countries in the same geographic area exporting to the same destination in the same year. Figure 5 (Table G14 column 8) shows that there is no recovery in this setting implying that countries that are more exposed to bank failures export relatively less to the average destination every year until 1914. As robustness, I also re-estimate the baseline with region-year fixed effects using the subsample of countries that have SITC information and verify that the patterns are similar (Table G14 column 7 and plotted in Figure B12b).

Third, I directly test for the competitive effects within region by estimating the effect of other countries’ average exposure on a given country’s exports, controlling for that country’s own exposure. The prediction is that there should be positive spillovers ($\psi_t > 0$) because a trade cost shock to a given country will benefit its competitors with similar exports. Figure B14 shows that negative shocks to a country’s competitors does benefit its own exports into a given market. While the standard errors are large, it is worth noting that the outcome variable is a country’s total exports rather than industry-specific exports, which would generate attenuation bias.

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. As in the

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

70 The specification is: $\ln(\text{EX}_{odt}) = \psi_t\text{Region Fail}_{-o,t} + \beta_t\text{Fail}_o + \gamma_o + \gamma_{dt} + \theta_t\ln(\text{dist}_{od}) + \varepsilon_{odt}$ where Region Fail$_{-o,t}$ is the average exposure of other countries in a region for a given country o.

71 This competitive effect is also documented in Hanlon (2015) which shows that during the US Civil War, other cotton producers benefited and exported more to the major global cotton buyer, the UK.

72 This measure is calculated by removing each city of financing’s contribution from the country-level
country-level analysis, $\psi > 0$ indicates that a port benefits when it is in a country where the rest of the ports are more exposed. Table 6 Columns 1–3 present the results. Columns 1–2 estimates the elasticity for total exports from a port while Column 3 estimates the elasticity using within destination variation. In this specification, a given destination imports more from a port when the other most similar ports are more exposed.

The sustained persistence of the effects within regions are not driven by the smaller sample comparisons. In a robustness check, I conduct a Fisher exact test for the country groups by simulating 1,000 random group assignments and re-estimating the coefficients. I plot the distribution of the five-year coefficients in Figure G5. This figure shows that the true estimates are very similar to the simulated estimates for the years until 1900. At that point, the true coefficients are larger in magnitude than the average simulated coefficient.

5.2 Heterogeneity

Lower financial needs

First, shorter routes are 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.\(^\text{73}\) Figure B15 plots the baseline effect of exposure $\beta_t$ in orange and the additional effect of failure for close relationships $\theta_{t,\text{close}}$ in blue. $\beta_t$ is very similar to the baseline effect in previous estimations. $\theta_{t,\text{close}} > 0$ 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 $\theta_{t,\text{close}} + \beta_t$, 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 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 “Failure” coefficients are plotted in orange and the additional heterogeneity in blue in Figure B16. These results show that exposed countries lost market

exposure measure rather than simply leaving out the port’s exposure in order not to double-count cities that financed more than one port. The specification is: $\ln(S_{pot}) = \beta\text{Fail}_{po} \times \text{Post}_t + \psi\text{Fail}_{other,o} \times \text{Post}_t + \alpha_p + \Gamma\text{X}_{pot} + \varepsilon_{pot}$.

\(^\text{73}\)The specification is: $\ln(\text{EX}_{odt}) = \theta_{t,\text{close}}\text{Fail}_o \times \mathbf{1}(\text{Close}_{od}) + \beta_t\text{Fail}_o \times \mathbf{1}(\text{Close}_{od}) + \psi'\text{X}_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}$.
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).

**Access to alternative sources of financing**

Exporters that were not funded by just 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 pre-crisis. I re-estimate Equation 5 with an interaction term between exposure to failure and the number of non-British banks.\(^{74}\) \(\phi\) is the main coefficient of interest and captures the additional impact of failure on locations with non-British banks. Table 6 (Columns 4 & 5) confirms that having access to more non-British banks pre-crisis mitigated the main losses (\(\phi > 0\)). The magnitude of \(\phi\) is 32% of the baseline effect. The average port had access to 0.6 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 20%.

I estimate the long-term effects of gaining access to alternative banking networks by using the nationalities and identities of the multinational banks within each city in the five year windows from 1850–1914.\(^{75}\) French and German banks are the most important alternatives because they accessed the second and third largest money markets in the world after London. I construct a binary variable called “European bank” (\(I(EB_o)\)) that takes the value of 1 when the exporting country gains access to either a French or German bank, and 0 otherwise. This variable proxies for access to the most likely alternative to the London money market, which is interacted with the origin-country exposure.\(^{76}\) Figure B17 plots \(\beta_t\) in orange and \(\theta_t\) in blue. Interacting \(I(EB_o)\) with the exposure to failure each year estimates the additional effect of access to alternative financing for exposed places. The full effect for exposed places is \(\theta_t + \beta_t\), which is close to 0 for most years, indicating that countries that did not gain access to other financing networks are the ones driving the main losses seen in Figure 3.

**Benchmarking the magnitudes**

This paper shows that the effect on an exposed exporter’s aggregate export levels is perma-

\(^{74}\)The specification is: \(\ln(S_{pot}) = \beta_{\text{Fail}_{po}} \times \text{Post}_t + \phi_{\text{Non-Brit}_{po}} \times \text{Post}_t + \alpha_p + \gamma_{ot} + \Gamma'X_{pot} + \varepsilon_{pot}\)

\(^{75}\)Data are from Kisling, Meissner and Xu (2020).

\(^{76}\)The specification is: \(\ln(EX_{odt}) = \theta_{t}\text{Fail}_o \times I(EB_{od}) + \beta_{t}\text{Fail}_o + \lambda_tI(EB_{od}) + \Psi'X_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}\)
nent 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 ones. I now benchmark the total losses in exports against estimates in the trade and macroeconomic literatures and show they are highly consistent.

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 initial 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 post-crisis growth rates of aggregate exports after two years is 7.9% for unexposed countries relative to countries with average (11 pp) exposure. Using my dataset, I estimate a trade elasticity of -1.1 to geodesic distance. Relative to 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 7.2%. This magnitude is also similar 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 ten years, leaving a permanent effect on levels. These dynamics of lack of recovery in levels are consistent with the empirical evidence that crises permanently lower the levels of output relative to a no-crisis counterfactual (Cerra and Saxena, 2008).

Note that the difference-in-differences partial equilibrium analysis in this paper 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, as is the case with these more macro estimations, simply adding the partial equilibrium losses across countries likely overstates aggregate global impact.

\[77\text{In other words, a 1\% increase in physical distance between two countries reduces the trade flows between them by 1.1\%}. \text{ This elasticity is, coincidentally, exactly the average elasticity found in the literature based on the survey of structural gravity by Head and Mayer (2014). It is slightly larger than the average estimate of -0.93 found in all gravity papers. Table A6 reports the estimates and robustness to controlling for gravity measurements of bilateral resistance.}\]

\[78\text{While there are instances of countries exhibiting higher growth after a shock to recover losses in levels (notably historically in the US), this pattern is the exception rather than the norm. Cerra, Fatás and Saxena (2020) provides an in-depth review and synthesis of this long literature.}\]
6 Conclusion

Despite the prevalence of financial shocks and their known impact on short-run outcomes, there is little causal evidence whether these temporary events can have long-run impacts on the shape of global trade patterns. This paper uses a salient historical setting and novel archival data to provide new 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 exports destinations that persisted for four decades.

These persistent effects can be understood within a framework in which 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 post-crisis recovery among advanced economies in recent decades suggests that the historical record is more relevant than ever. While this paper has provided one set of magnitudes for the impact on trade, gaining a broader understanding of how major shocks impact economies at longer horizons in other contexts would be a fruitful avenue for future research.
References


Allen, Treb, and Dave Donaldson. 2018. “Geography and path dependence.”


### Tables

**Table 1: Summary statistics: ports and countries**

<table>
<thead>
<tr>
<th></th>
<th>mean</th>
<th>Ports</th>
<th>sd</th>
<th>mean</th>
<th>Countries</th>
<th>sd</th>
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<tbody>
<tr>
<td>Exposure to failed British banks</td>
<td>0.07</td>
<td>0.00</td>
<td>(0.19)</td>
<td>0.01</td>
<td>0.03</td>
<td>(0.17)</td>
</tr>
<tr>
<td>Exports</td>
<td>127.99</td>
<td>32.00</td>
<td>(231.05)</td>
<td>12.49</td>
<td>2.15</td>
<td>(32.96)</td>
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<td>Fraction exports to UK</td>
<td>0.39</td>
<td>0.30</td>
<td>(0.34)</td>
<td>0.62</td>
<td>0.69</td>
<td>(0.37)</td>
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<tr>
<td>Destinations (# countries)</td>
<td>7.60</td>
<td>5.00</td>
<td>(7.28)</td>
<td>3.95</td>
<td>2.00</td>
<td>(8.32)</td>
</tr>
<tr>
<td>Distance to destination (km k)</td>
<td>5.31</td>
<td>5.12</td>
<td>(3.48)</td>
<td>6.12</td>
<td>5.26</td>
<td>(3.51)</td>
</tr>
<tr>
<td>Banks</td>
<td>6.03</td>
<td>3.00</td>
<td>(7.54)</td>
<td>5.27</td>
<td>1.00</td>
<td>(9.96)</td>
</tr>
<tr>
<td>Non-British banks</td>
<td>0.60</td>
<td>0.00</td>
<td>(1.06)</td>
<td>2.97</td>
<td>0.00</td>
<td>(8.74)</td>
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<td>Fraction in British Empire</td>
<td>0.34</td>
<td>0.00</td>
<td>(0.47)</td>
<td>0.33</td>
<td>0.00</td>
<td>(0.47)</td>
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**Notes:** Table 1 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. Fraction of exports to the UK is similarly calculated using the number of ships and values of exports.
Table 2: Pre-crisis comparison of bank characteristics

### Panel A: Balance sheet characteristics (joint-stock banks)

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Not Failed</th>
<th>Failed</th>
<th>Diff</th>
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</thead>
<tbody>
<tr>
<td>OG Connection</td>
<td>0.17 (0.38)</td>
<td>0.12 (0.32)</td>
<td>0.39 (0.50)</td>
<td>-0.27 (0.09)*****</td>
</tr>
<tr>
<td>Capital, authorized (£m)</td>
<td>1.43 (1.06)</td>
<td>1.38 (1.05)</td>
<td>1.63 (1.11)</td>
<td>-0.25 (0.28)</td>
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<tr>
<td>Capital, paid up (£m)</td>
<td>0.58 (0.38)</td>
<td>0.60 (0.38)</td>
<td>0.48 (0.40)</td>
<td>0.13 (0.10)</td>
</tr>
<tr>
<td>Deposits (£m)</td>
<td>2.22 (2.73)</td>
<td>2.27 (2.85)</td>
<td>1.99 (2.24)</td>
<td>0.27 (1.08)</td>
</tr>
<tr>
<td>Reserve fund (£m)</td>
<td>0.13 (0.12)</td>
<td>0.13 (0.12)</td>
<td>0.13 (0.15)</td>
<td>-0.00 (0.04)</td>
</tr>
<tr>
<td>Total size (£m)</td>
<td>4.76 (6.08)</td>
<td>4.97 (6.43)</td>
<td>3.89 (4.47)</td>
<td>1.09 (1.82)</td>
</tr>
<tr>
<td>Leverage ratio</td>
<td>0.06 (0.05)</td>
<td>0.07 (0.05)</td>
<td>0.06 (0.05)</td>
<td>0.01 (0.02)</td>
</tr>
<tr>
<td>Reserve ratio</td>
<td>0.03 (0.02)</td>
<td>0.03 (0.02)</td>
<td>0.03 (0.02)</td>
<td>-0.00 (0.01)</td>
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<td>Liquidity ratio</td>
<td>0.14 (0.11)</td>
<td>0.14 (0.10)</td>
<td>0.12 (0.15)</td>
<td>0.02 (0.03)</td>
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### Panel B: Other characteristics (all banks)

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<th></th>
<th>All</th>
<th>Not Failed</th>
<th>Failed</th>
<th>Diff</th>
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<tbody>
<tr>
<td>OG connection</td>
<td>0.18 (0.39)</td>
<td>0.15 (0.36)</td>
<td>0.33 (0.48)</td>
<td>-0.18 (0.1)****</td>
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<tr>
<td>Trade finance (£th)</td>
<td>105.79 (246.77)</td>
<td>111.78 (263.41)</td>
<td>75.30 (133.34)</td>
<td>36.48 (59.0)</td>
</tr>
<tr>
<td>Age (years)</td>
<td>36.24 (53.15)</td>
<td>40.65 (56.38)</td>
<td>13.76 (21.34)</td>
<td>26.89 (12.5)****</td>
</tr>
<tr>
<td>Cities (#)</td>
<td>13.76 (22.88)</td>
<td>14.79 (24.54)</td>
<td>8.48 (9.97)</td>
<td>6.32 (5.5)</td>
</tr>
<tr>
<td>Countries (#)</td>
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<td>7.85 (9.24)</td>
<td>6.52 (6.94)</td>
<td>1.33 (2.1)</td>
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<td>Asia (£th)</td>
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<td>48.93 (184.16)</td>
<td>31.13 (60.75)</td>
<td>17.80 (40.7)</td>
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<td>Africa (£th)</td>
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<td>7.06 (21.86)</td>
<td>13.83 (37.69)</td>
<td>-6.76 (6.0)</td>
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<td>N. America (£th)</td>
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<td>15.25 (47.99)</td>
<td>3.85 (13.37)</td>
<td>11.39 (10.6)</td>
</tr>
<tr>
<td>S. America (£th)</td>
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<tr>
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<td>23.46 (47.13)</td>
<td>29.65 (35.7)</td>
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<tr>
<td>UK (£th)</td>
<td>12.59 (40.28)</td>
<td>14.83 (43.72)</td>
<td>1.19 (2.73)</td>
<td>13.63 (9.6)</td>
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</table>

**Notes:** Table 2 Panels A and B 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 < 0.1, **p < 0.05, ***p < 0.01. Sources: Bank of England Archives C24/1, Banker’s Magazine, The Economist.
Table 3: Correlation between bank failures and pre-crisis location characteristics

\[ I(\text{Failure}_b) = \alpha + \beta \bar{X}_b + \varepsilon_b \]

**Panel A: Port characteristics**

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<td>Steam ships</td>
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<td>[0.0336]</td>
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<tr>
<td>Fraction to UK</td>
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<td>[0.0324]</td>
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Panel B: Country characteristics

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<td>Δ Exports ('60-'65)</td>
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<td>[0.0393]</td>
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N | 122 | 122 | 122 | 122 | 122 | 122 | 122 | 122 |
### Panel C: Country characteristics: exports composition

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<td>Cotton, manufactured</td>
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<td>Grains</td>
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<td>Bullion</td>
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<td>Sugar</td>
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<td>0.0456</td>
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**Notes:** Table 3 reports estimates from the bank-level regression of bank exposure to location characteristics pre-crisis on bank failure rates. The dependent variable is $I(\text{Failure}_b)$, the measure of bank failure. The independent variable of interest $\bar{X}_b$ is the share-weighted exposure of banks to location characteristics, normalized to have zero mean and unit variance. 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 28.48 for ports and 30.43 for countries. Regressions are weighted by the average location’s exposure to bank $b$. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

As discussed in Borusyak, Hull and Jaravel (2021), 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 columns 1–8, there are 122 observations instead of the full 128 because 6 banks operated in cities which were not the closest city for any port, so they do not contribute to the port-level exposure measures.
Table 4: Intensive margin effect of bank failures on shipping

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

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Country (1)</th>
<th>Port (2)</th>
<th>Port (3)</th>
<th>Port (4)</th>
<th>Excl. cotton (5)</th>
<th>Poisson (6)</th>
<th>News dates (7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>\text{Fail}_{po} \times \text{post}</td>
<td>-0.514**</td>
<td>-0.687***</td>
<td>-0.707***</td>
<td>-0.637***</td>
<td>-0.535***</td>
<td>-0.991***</td>
<td>-0.578***</td>
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<tr>
<td>Capital city \times \text{post}</td>
<td>[0.225]</td>
<td>[0.246]</td>
<td>[0.154]</td>
<td>[0.201]</td>
<td>[0.195]</td>
<td>[0.154]</td>
<td>[0.198]</td>
</tr>
<tr>
<td>Age of banks \times \text{post}</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>OG link \times \text{post}</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td>Fraction to UK \times \text{post}</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>\text{Country}_o \times \text{post FE}</td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>\text{Port}_p \times \text{FE}</td>
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<td>\text{Country}_{po} \times \text{post FE}</td>
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<td>54</td>
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Notes: Table 4 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 is the log of the total number of ships departing in each period. \text{Fail}_{po} is the share of the port’s banks that failed during the crisis. The mean of \text{Fail}_{po} is 0.07, and the standard deviation is 0.2. Post is a dummy for the post-crisis 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 within the country, the average age of banks, and the fraction of shipping to the UK. The sample is restricted to ports active in both the pre- and post-period. Standard errors in brackets are clustered by country of origin. *p < 0.1, **p < 0.05, ***p < 0.01
### Table 5: Extensive margin effect of exposure to bank failures

<table>
<thead>
<tr>
<th></th>
<th>Con: dest</th>
<th>Con: avg ships</th>
<th>Ports: dest</th>
<th>I(Port Entry)</th>
<th>I(Port Exit)</th>
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<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
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<tr>
<td>Fail_o × post</td>
<td>-0.484***</td>
<td>-0.111</td>
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<tr>
<td></td>
<td>[0.163]</td>
<td>[0.242]</td>
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<td>Fail_po × post</td>
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<td>-0.426***</td>
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<td></td>
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<td>[0.135]</td>
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<td>Fail_po</td>
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<td>-0.159**</td>
<td>-0.281***</td>
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<td>Port controls Y</td>
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Notes: Table 5 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 2 is the log number of unique destinations accessed by countries and ports, respectively. The sample in columns 1 and 2 is restricted to ports that were active in both the pre-shock and the post-shock periods. The dependent variable in columns 3 and 4, “I(Port Entry)” is a binary variable that takes the value of 1 for a port that was not active in the pre-shock period and became active in the post-shock period, and 0 otherwise. The dependent variable in columns 5 and 6, “I(Port Exit)” is a binary variable for a port that was active in the pre-shock period and became inactive in the post-shock period. The sample size in columns 4–6 reflects the number of ports that were active in the post-period (for Entry) or pre-period (for Exit).

All variables are defined the same way as in Table 4. Standard errors in brackets are clustered by country of origin. *p < 0.1, **p < 0.05, ***p < 0.01
Table 6: Exporter substitution and effect of access to alternative sources of financing

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<td>Fail_po × post</td>
<td>-0.711***</td>
<td>-0.819***</td>
<td>-0.645**</td>
<td>-0.936***</td>
<td>-0.906***</td>
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<td></td>
<td>[0.248]</td>
<td>[0.255]</td>
<td>[0.277]</td>
<td>[0.227]</td>
<td>[0.279]</td>
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<tr>
<td>Fail_other, po × post</td>
<td>0.0912</td>
<td>0.139*</td>
<td>0.270**</td>
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<td></td>
<td>[0.0715]</td>
<td>[0.0745]</td>
<td>[0.110]</td>
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<tr>
<td>non-Brit banks × Fail_po × post</td>
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<td></td>
<td></td>
<td>0.290***</td>
<td>0.287***</td>
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<td>Destination_d × post FE</td>
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<td>51</td>
<td>54</td>
<td>54</td>
</tr>
</tbody>
</table>

Notes: Table 6 reports estimates of the effect of the rest of the country’s exposure to bank failures and access to alternative forms of financing on shipping activity. The dependent variable is the log of the number of ships sailed. Fail\_po is the share of the port’s banks that failed during the crisis, Fail\_o is the share of the country’s banks that failed, and Fail\_other,po is the country-level share of bank failures outside of port p. “non-Brit banks” is the number of non-British banks in the port’s city of financing in the pre-crisis year. All other variables are defined the same way as in Table 4. Standard errors in brackets are clustered by the origin-country. *p < 0.1, **p < 0.05, ***p < 0.01
Figures

Figure 1: Geography of banking and trade

(a) British multinational bank lending and failures

(b) Port-level trade activity

Notes: Figure 1a maps the distribution of the city-level exposure to bank failures $\text{Fail}_l$. 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. Figure 1b maps the distribution of shipping activity at ports in the pre-crisis 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.
Figure 2: Aggregate exports, grouping countries by above and below average exposure to bank failures

(a) Exports by group

![Graph showing exports by group over years](image)

(b) Difference between groups

![Graph showing difference between groups over years](image)

Notes: Figure 2a plots the raw data for the total value of exports by groups of countries from 1850–1914. 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. Exports values are normalized to equal 1 in 1866. Figure 2b plots the difference between the values for the two groups. The vertical line marks 1866. Figure B11 plots the coefficients and standard errors from the equivalent regression.
Figure 3: Persistent effect of financing shock on exporter market share

\[ \ln(\text{EX}_{o dt}) = \beta_t \text{Fail}_o + \Gamma' \text{X}_o + \gamma_o + \gamma_{dt} + \theta_t \ln(\text{dist})_{od} + \varepsilon_{odt} \]

(a) \( \beta_t \) estimated annually

(b) \( \beta_t \) estimated every 5 years

Notes: Figure 3 plots the \( \beta_t \) point estimates and 95 percent confidence intervals for the specification given in equation 8 estimated on the country-level panel of trade. \( \beta_t \) 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 Table G14 column 1 for the point estimates.
Figure 4: Decomposition of effect of indirect exposure on importers

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

\[ \ln(IM_{dt}) = \beta_t \text{Fail}_{d,\text{pre}} + \gamma_d + \gamma_t + \epsilon_{dt} \]

(b) Decomposition of total market share

\[ \frac{IM_{dt,\text{new}}}{IM_{dt}} = \beta_t \text{Fail}_{d,\text{pre}} + \gamma_d + \gamma_t + \epsilon_{dt} \]

Notes: Figure 4a plots the \( \beta_t \) point estimates and 95 percent confidence intervals for three separately estimated regressions of the specification written above and the dependent variable indicated. The specification written for total imports is given above and includes destination (importer) \( d \) and year fixed effects. The dependent variable is the log of total imports (dark blue triangle), the log of number of trade partners (light blue square), and the log of average imports per source (orange circle). \( \text{Fail}_{d,\text{pre}} = \sum_o s_{od} \text{Fail}_o \) 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 pre-crisis were fully exposed to the bank failures. Figure 4b has the share of total imports from pre-crisis trade partners as the dependent variable. Standard errors are clustered by destination country. The observations are weighted by country size proxied by number of trading partners in order to mostly closely mirror the aggregates in Figure 2.
Figure 5: Persistent effects within groups of countries with similar exports

$$\ln(Ex_{odt}) = \beta_t \text{Fail}_o + \Gamma'X_{ot} + \gamma_o + \gamma_{dt} + \psi\text{Region}_{ot} + \theta_t\ln(\text{dist})_{od} + \varepsilon_{odt}$$

Notes: Figure 5 plots the point estimates and 95 percent confidence intervals for the specification given above estimated on the country-level panel of trade. $\beta_t$ 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 Table G14 column 8 for the point estimates.