

Reshaping Global Trade: The Immediate and Long-Run Effects of Bank Failures

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Abstract

I provide causal evidence that financial sector disruptions can reshape international trade patterns for decades. I use the laboratory of the first modern global banking crisis centered in London in 1866 and collect archival loan records that link multinational banks headquartered in London to their exports financing abroad to estimate that countries exposed to bank failures exported significantly less to each destination until 1905. Aggregate global exports did not fall, indicating that the persistent market share losses borne by exposed exporters provided unexposed exporters with a competitive advantage. 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.

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How much and for how long do financial crises impact the patterns of international trade? International trade relationships are highly stable and generally understood to be shaped by slow-moving forces of comparative advantage such as differences in technologies, endowments, and institutions. In most frameworks, transient shocks like financial crises can only have temporary effects, but in others, 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 is unclear. Given the severity of financial sector disruptions in both developing and advanced economies, documenting how the patterns of trade respond to crises is crucial both for crafting policy responses and for understanding the relationship between temporary shocks and immediate to long-run outcomes.

Establishing the effect of financial shocks on trade is difficult for multiple reasons. Economic fundamentals simultaneously impact exports and banking sector health. 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, it would be possible 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 problems by studying the 1866 London banking crisis which 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-difference 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

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.

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, the crisis occurred at the beginning of the First Age of Globalization, the longest globally peaceful post-crisis period in modern history, making it possible to trace out the long-run effects. Fourth, in contrast to much of the existing literature on long-run outcomes that relies primarily on cross-sectional evidence, in this setting we can trace out the full dynamics each year, both before the crisis to show the lack of pre-trends, and afterwards to examine the speed and dynamics of recovery. 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 the bank failures are mostly unrelated to observable measures of bank solvency. I show that one of the only observable bank characteristics correlated with failure is a public connection to Overend & Gurney. Crucially, Overend 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 further distinguishes the setting here from other historical banking crises that have arisen from negative real economic shocks.

More formally, identification does not require pure bank failure randomness but only 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-difference setting, the identifying assumption is that there is not a simultaneous shock to a location that causes both its exports to decline and the banks operating there to fail (Borusyak, Hull and Jaravel, 2021). It does not require that the geographic distribution of bank subsidiary operations is random. 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.

³While many papers with modern data are able to exploit bank balance sheet data and firm-bank linkages, it is difficult to directly observe trade finance, so they tend to estimate an overall "bank channel." As noted in Ahn, Amity and Weinstein (2011) these measurements are often uncorrelated with trade finance.

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 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 original trade statistics publications. 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.

The conceptual framework for how exposure to a temporary financial shock could have long-run effects on the patterns of international trade is based on the hysteresis literature.⁴ One way that hysteresis can arise is with substitutable exporters facing a sunk cost of entering a new market, as in Baldwin (1988) and Baldwin and Krugman (1989).⁵ I model trade by adding financing costs to Melitz (2003) which features monopolistic competition and firm heterogeneity.⁶ Consistent with the institutional framework of bank-intermediated trade financing where exporters need to cover working capital costs during the period of a shipment, I model the external financing cost as a marginal cost that scales with the value of trade.⁷ Firm profits are an increasing function of productivity, financing supply in the exporter’s location, and inverse trade costs. Pre-shock, only firms productive enough to pay the entry costs will be exporting to a given destination, and their prices and varieties in equilibrium determine the price index of goods in the destination.

A financing shock lowers the supply of financing in a location, which raises exporters’

⁴Hysteresis is defined as effects that persist even after the initial stimulus has been removed. Completely hysteretic effects can also point to the existence of multiple equilibria in the economy.

⁵In international trade, sunk costs are generally used to explain hysteretic patterns and can be micro-founded from the costs of learning about a new market or in acquiring a customer base. They are estimated to be high even today when information is much cheaper to obtain (Roberts and Tybout, 1997; Bernard and Jensen, 2004; Das, Roberts and Tybout, 2007). Redding, Sturm and Wolf (2011) also incorporates sunk costs into a model depicting the potential for multiple equilibria in the geographic distribution of industrial activity, with an empirical focus on airport hubs.

⁶The demand side is modeled with CES preferences over varieties produced by individual firms (Krugman, 1980). Deviations from monopolistic competition can be captured through the preference parameter for the elasticity of substitution.

⁷Chaney (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.

prices, lowers their profits, and raises the overall price index in their destinations. Exposed firms will export less on the intensive margin (because their prices have increased) and be less likely to enter new markets on the extensive margin (because they are less profitable). However, they may not necessarily exit a market because re-entry in the future will require paying the sunk costs again, so there is an option value to staying. Since destination market price indices have increased, average profits for entering the destination are higher and new firms enter. Post-shock, the mass of firms operating is higher, which makes a market more competitive and future entry more difficult. The initial market share gains can lead to entrenched advantages as bigger exporters learn about their trade partners, become more productive, and direct their investments to accommodate demand.⁸

I empirically test for these dynamics by comparing the exports behavior of more versus less-exposed exporters, before and after the shock in the immediate, medium, 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 on the intensive margin. Countries exposed to a one standard deviation (16 pp) increase in bank failures have 8.2% lower exports. I find very similar point estimates when using within-country, across-port variation. Including country-by-period fixed effects nets out time-varying unobserved heterogeneity at the country level that could potentially drive the results, which provides further evidence that the bank failures are orthogonal to other unobserved country-level shocks that could affect exports. These results are robust to alternative specifications, count regression methods, subsample restrictions, and other robustness checks.

In my second set of results, I find that on the extensive margin, exporters more exposed to the shock end up with fewer trade partners and are less likely to export at all. In the year after the crisis, a one standard deviation increase in exposure causes ports to have 8% fewer trade partners and to be 5% less likely to engage in international trade. I also find that exposed incumbent exporters are not more likely to exit, consistent with the option value of continuing to export given sunk costs.

My third set of results shows that the exports losses are highly persistent. For aggregate exports, countries with above-average exposure have a large initial difference in annual growth rates which puts them on a permanently lower path after 1866. Annual growth rates re-converge after five years, but there is no compensating positive growth that returns these countries to their pre-shock trend levels of exports, leading to a cumulative difference of 77%

⁸There 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 through learning-by-doing. Redding (2002) provides a model of path dependence from technological leadership.

in aggregate exports by 1914 which is an average difference of 1.6% per year.⁹

Effects on aggregate exports may in part be driven by differential demand patterns among countries' trade partners. I address this possibility by using the full panel of bilateral trade data that allows me to control for demand shocks from the importer by including importer-by-time fixed effects, thereby comparing two countries exporting to the same destination in the same year. I also augment the specification with a host of standard measures of resistance to trade between countries from structural gravity, and I find highly persistent market share effects. In the baseline estimation, importers bought on average 21% less each year from exporters with a one standard deviation higher exposure to bank failures for four decades. After 1905 the effects are borderline significant and magnitudes remain similar. This hysteresis is robust to controlling for a wide variety of contemporary shocks and initial macro-economic conditions and to simulated placebo shocks.

The difference-in-difference partial equilibrium analysis 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. By studying the other side of a trade transaction and looking at countries' imports, I find no impact of the financing shock, suggesting that aggregate global trade did not suffer persistently. These results can be reconciled with the main findings on the market share losses by exporters. If importers are able to substitute across exporters and compensate for losses from exposed countries with gains from unexposed countries, exposed exporters will lose market share but aggregate trade will not be strongly affected. This pattern of cross-country substitution by importers also corroborates the conceptual framework that unshocked exporters were able to differentially benefit and gain market share at the expense of their competitors.

In the final section of the paper, I provide evidence that trade patterns changed in a manner consistent with the financing shock impacting exporters' competitiveness. First, exporters that were likely to provide similar goods benefit 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 hysteresis ef-

⁹The differences in GDP growth average to be 0.6% per year. This result that the overall levels of economic activity do not recover their pre-shock trend and are fully history-dependent has been found in many studies and is more common empirically than post-shock recovery booms that do return countries to trend (Cerra, Fatás and Saxena, 2020; Blanchard, Cerutti and Summers, 2015; Ball, 2014).

fects. Third, relationships that relied less on trade finance, such as those that were physically closer, experience less persistent losses than more distant relationships.

This paper provides the first estimates from a quasi-experimental setting of hysteretic effects of a temporary financial shock on trade patterns. The magnitudes of the long-run market share losses for exporters are large, which can be rationalized by an institutional context featuring highly substitutable goods (commodities trade), high sunk costs (slow communication), and intense competition (rapidly expanding trade networks in the 1860s and 70s). The trend toward eventual recovery in market shares could be explained by changes in economic fundamentals such as technological progress and the introduction of new varieties of goods over decades, which are slow-moving phenomena.

This paper contributes to several literatures. First it provides 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) provided early empirical evidence from various settings of both recovery and persistence in urban agglomeration.¹⁰ With respect to international trade, Baldwin (1988, 1990); Baldwin and Krugman (1989) provide many theoretical results of persistence while empirical work such as Roberts and Tybout (1997), Eichengreen and Irwin (1998), and Bernard and Jensen (2004) show that firms' and countries' history of exporting predict contemporary trade patterns. However, 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 the First Age of Globalization impacts cross-country trade patterns and could be viewed as a critical juncture for generating significant first-mover advantages (Krugman, 1991; Allen and Donaldson, 2018).

It also relates to the macroeconomic literature that has found that financial crises have 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).¹¹ 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 com-

¹⁰Related papers showing persistent effects of temporary historical events include Nunn (2008) on the African slave trade, Dell (2010) on forced labor institutions in Peru, and Sequeira, Nunn and Qian (2020) on the age of mass migration in the US for development, Juhász (2018) on the Napoleonic blockade for industrial growth, and Hanlon (2015) on the US Civil War for innovation.

¹¹The long run risk and rare disasters literature has also shown how these events can help to explain the equity premium puzzle in financial markets (Rietz, 1988; Barro, 2006, 2009; Gabaix, 2012).

ponent (Blanchard and Summers, 1986; Campbell and Mankiw, 1987). Similarly, Nakamura, Steinsson, Barro and Ursúa (2013) finds that consumption can be significantly (and sometimes permanently lower) for decades after disaster events. Abiad, Mishra and Topalova (2014) examines trade outcomes but also use the macroeconomic approach of correlating domestic crises with domestic outcomes over many events. While this literature is primarily concerned with documenting how economies evolve after shocks without focusing on endogeneity or reverse causality, I circumvent these concerns by following the technique of using crises originating abroad (Peek and Rosengren, 1997, 2000; Cetorelli and Goldberg, 2011; Schnabl, 2012). The recent settings of those studies necessarily prevent them from speaking to long-run effects while by contrast, my historical setting makes it natural to examine decades of outcomes.

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; Gourinchas, Rey and Truempler, 2012; 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; Bräuning and Ivashina, 2020). The setting of a major shock to the pre-WWI global hegemon in this paper also 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.¹² Many studies use cross-industry variation in external finance dependence and measure a country or firm's access to finance as a static source of comparative advantage, finding that financial constraints differentially affect exports for countries or firms specializing in financially-dependent sectors (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; Eaton, Kortum, Neiman and Romalis, 2016). My analysis also focuses on post-crisis outcomes, but I present reduced-form evidence using bank-level variation as in Amiti and Weinstein (2011); Paravisini, Rappoport, Schnabl and Wolfenzon (2014); Paravisini, Rappoport and Schnabl (2015) while extending the analysis to every country over much longer periods.

The paper is organized as follows: the next section discusses the historical context,

¹²See Foley and Manova (2015) for a comprehensive review and assessment of the literature pertaining to modern settings.

Section 2 discusses the historical data sources, Section 3 describes the identification strategy, and Section 4 presents a conceptual framework. Section 5 reports the main results, and Section 6 provides evidence on the changes in the patterns of trade. Section 7 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.¹³ 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.¹⁴ Their business model also benefited from a form of exorbitant privilege due to the pound sterling’s centrality: banks paid low rates on

¹³For 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).

¹⁴They 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.”

domestic liabilities (deposits) in the largest capital pool in the world and received high rates on their foreign assets (trade finance) abroad.¹⁵ 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.”¹⁶ 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.¹⁷

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 in turn guaranteed the ultimate debt.¹⁸ 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.¹⁹ The BoE always

¹⁵The 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.”

¹⁶Contemporaries 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.

¹⁷For 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).

¹⁸King, in *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).

¹⁹The BoE used a double-entry accounting system, and all bills that went to the BoE’s discount window

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.²⁰ 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.²¹

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 money market. Discounting the bills from abroad allowed the head office to supply the foreign offices with fresh capital in return.²² 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.

(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.

²⁰The 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.

²¹Bignon, 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.

²²The 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."

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 as a gamble for resurrection. *Banker's Magazine*, a leading financial market publication, fully endorsed the firm as one of the best in the City of London when Overend & Gurney announced 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 9, Overend's directors privately approached the BoE for a private loan.²³ The BoE declined to extend credit, and Overend announced its bankruptcy the following afternoon.²⁴ 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."²⁵ The money market was completely frozen, and the BoE Discount Window was the only source of liquidity.²⁶ 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.²⁷ 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.²⁸

²³The proximate cause for bankruptcy that necessitated the loan was a court decision which ruled that they could not collect from a debtor (Sowerbutts, Schneebalg and Hubert, 2016).

²⁴The Bank of England justified its actions by 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).

²⁵Appendix C.3.1 provides many additional examples of contemporary newspaper reports of the crisis.

²⁶"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).

²⁷The 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.

²⁸The full text of the letters exchanged on May 11 are in Appendix C.2. In them, the Chancellor of the

The crisis in London was also widely reported around the world (see Appendix C.3.2) with the English language newspaper in Buenos Aires reporting 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.” Ultimately, 22 banks headquartered in London were forced to close or indefinitely suspend operations. Headquarter closures caused branches abroad to close immediately, therefore directly affected the supply of trade finance available in each city around the world that depended on these banks’ foreign operations.²⁹

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 the sources and full definitions, details, and documentation for each variable that enters as a regressor in the empirical analysis in Appendix G.

2.1 Bank characteristics

Lending pre-crisis: The Bank of England kept detailed records of every transaction that occurred at its Discount Window. 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.³⁰ An example of a ledger page is shown in Figure B1a. 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

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.”

²⁹For example, the Commercial Bank failed in London May 15. The headquarter’s telegraph to its Bombay offices arrived on May 24 and read, “This bank suspended. Cease all operations. Make no payments. Allow no transfers or sales.” While these branches were directed by the local branch manager, who had wide latitude in daily decisions due to the communication lags with London, they relied on regular fresh injections of capital from London to operate. Unlike in the modern context where branches and subsidiaries have very different structures that have implications for risk-sharing (Fillat, Garetto and Smith, 2018), the historical operations were a mix between the two: capital was shared, as in a branch system but decisions were local, as in a subsidiary system.

³⁰For the entries from 1866, I only include bills that originated before the crisis. For robustness, I can further restrict the banks’ portfolios to only bills discounted in May 1866 during the panic. Since the discounting activity at the BoE primarily occurred during the panic itself (Figure C3), the distribution is very similar. The full discussion is in Appendix C.5.

be discounted, and the date it was brought to the Bank of England.³¹ 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 before, during, and after the crisis. Names of the managers, directors, and partners of the banks were listed in financial almanacs, advertised in contemporary newspapers, and often mentioned in shareholder meetings.

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 the years 1865–1867, a two-year window around the crisis. An example of this source from September 5, 1866 is shown in Figure B1b. There are over 400,000 unique shipping events which I digitized, geocoded, and aggregated to generate measures of exporting activity before and after the crisis.³² Over 8,000 unique destinations were geo-coded and assigned to 60 countries.³³ The dataset has two periods that aggregates shipping for the 1 year before and after May 1866.³⁴

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 which ports began trading and which ones exited.

Figure 1b maps the distribution of pre-crisis exporting activity for the ports using the log number of ships. *Lloyd's List* is the only source of within-year, within-country measures of exports for the whole world historically.³⁵

³¹Deciphering 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.

³²While it is technically possible to digitize the *Lloyd's List* for the long-run analysis from 1850–1914, the scale of digitization required is beyond the scope of this paper.

³³Destinations are inconsistently listed as countries or cities, so they are aggregated to a larger unit of observation to minimize sparsity in the dataset.

³⁴In Appendix F.1 I provide an alternative specification with port-level variation in the crisis start date based on when it would have received news of the crisis from London.

³⁵One drawback of the *Lloyd's List* data is that it does not report values of the goods onboard. However, 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

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 exports by country for 1865 and assign them to two-digit SITC codes to capture the pre-shock industrial composition.³⁶

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:

$$\ln(EX_l) = \alpha + \gamma \ln(Finance_l) + \Gamma' X_l + \varepsilon_l \quad (1)$$

Identifying γ 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 ε_l includes the unobserved local economic conditions that are positively correlated with trade finance, which biases γ 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.

in Appendix F.1.5.

³⁶Historical data on bilateral trade flows disaggregated by industry and year is unavailable for most countries and has mostly not been systematically collected.

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. I proxy the bank shocks with bank failure in 1866 and define the binary variable $\mathbb{I}(\text{Failure}_b)$ that takes the value of 1 if the bank failed 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 Fail_l :

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

Fail_l takes the form of a Bartik instrument where the pre-crisis importance of each bank to a location ($z_{lb,pre}$) are the ‘‘shares,’’ the bank failure rates are the ‘‘shocks,’’ and trade finance (Finance_l) is the endogenous variable. I provide the derivation of the instrument in Appendix D and discuss instrument validity in Section 3.2.

The Discount Window data captures the distribution of trade financing during the crisis well because these loans were the main form of short-term liquidity that could be taken to the BoE for cash and all available collateral in the market made its way to the BoE. In normal times however, many fewer bills are discounted and the volumes are lower. This data limitation means that the full panel of trade financing by each bank in all locations around the world does not exist so that it is not possible to estimate the first stage relationship. However, there is a strong pseudo first-stage relationship between exposure to bank failures and financing contractions at the bank-level, measured by lending on the balance sheet, shown in Table A1.³⁷

The empirical results will be in terms of the reduced form relationship between exposure to bank failures and log exports:

$$\ln(EX_{lt}) = \beta(\text{Fail}_l \times \text{Post}_t) + \Gamma'X_{lt} + \epsilon_{lt} \quad (3)$$

The reduced form coefficient β in Equation 3 is the semi-elasticity of the response of trade activity to British bank failures in location l in the post-crisis period. This β captures the direct effect of British bank contractions, but there may be additional indirect effects stemming from a change in the supply of trade finance by non-British bank in reaction to the exogenous change in British banking. In particular, if the other banks actually expanded their

³⁷Of the 95 joint-stock banks with balance sheet data, only 31 are disaggregated enough to show the total lending in the form of trade finance annually. The pseudo-first stage is calculated using this subset of banks. Table A2 shows that this subsample of banks is representative of the complete sample of all banks on all other observable dimensions.

business by taking advantage of the British bank failures, the estimated β underestimates the total effect of the crisis. Appendix D has the full derivation for the total effect from changes in all trade finance which bounds the estimated β , and Appendix C.4 shows that in fact the remaining banks did grow post-crisis.

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 B3 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.

3.1.1 Measurement error

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 for several reasons. First, banks did not discount their own bills, so there is no mechanical relationship between a bank’s own need for liquidity and the distribution of lending represented by the bills it brought to the Discount Window.³⁸ Second, the legal doctrine governing bills of exchange and their feature of joint liability means that banks would not benefit from discounting lower-quality collateral since they would ultimately be liable for the BoE’s losses on those bills. Third, only three bills were rejected by the BoE during the crisis and all evidence indicates that the BoE did not relax its standards and discount lower quality collateral during the crisis.³⁹ Therefore, any selection bias needs to have originated from the banks themselves and needs to have been towards only discounting *higher*-quality bills, despite the banking sector’s pressing liquidity needs. I provide further explanation and evidence in Appendix C.5.

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: $E[Fail_l \varepsilon_l] =$

³⁸This is verifiable in the data because the BoE recorded both the bank that accepted the bill as well as the discounter who brought in the bill, and in no case are these two the same. Therefore while it is possible that weaker banks may have had more severe liquidity needs and therefore discounted more at the BoE, the collateral they brought in would not mechanically reflect the assets of weaker banks.

³⁹In particular, the BoE continued to monitor its customers during the crisis, and it ultimately did not experience abnormal losses from its Discount Window, despite many banks failing.

$E[\sum_b z_{lb} \mathbb{I}(\text{Failure}_b) \varepsilon_l] = 0$. It is apparent that 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. One example of problematic sorting would be if banks that failed systematically operated in locations that experienced a boom in the pre-period 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.

In the following subsections, I first show that bank failure rates were not correlated with observable quantitative or qualitative characteristics of bank activity using balance sheets and narrative evidence from shareholder meetings. This evidence indicates that the bank failures themselves are mostly unpredictable. I then provide novel evidence that the determining factor for the bank failures is a publicly observable connection to the failed firm Overend & Gurney. Finally, I test for violations of the identifying assumption directly and show that bank failure rates are also mostly uncorrelated with observable characteristics of the locations in which they are operating. To the extent that certain characteristics are correlated with bank failures, they are included as controls in all the specifications to residualize their direct effect on exports activity. I use the Oster (2019) bounds to show that it is unlikely that further unobserved characteristics could be driving the results.

3.3 Determinants of bank failures

3.3.1 Quantitative measures

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, and Panel B has all banks including privately owned banks that did not publish them.

The balance sheet characteristics include proxies for bank health and risk-taking, and characteristics of the banks that failed are not statistically or economically different from those of the banks that did not fail (Panel A). Banks had on average £1.48 million equity capital, of which almost half was already paid by investors, and their reserve funds, deposit liabilities, total size of the balance sheet, leverage ratio, liquidity ratio, and reserve ratio are

also similar. The similarities in the leverage, liquidity, and reserve ratios suggest that banks that failed did not appear to systematically take on more risk than non-failed banks.

In Panel B, I include all other observable characteristics that are available for all the banks. Banks that survived were slightly older, but age would only be a potential 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). 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.⁴⁰ In addition, banks in the two groups are similarly geographically diversified, operating in an average of almost 14 cities and 8 countries.

3.3.2 Narrative evidence

In addition to the quantitative evidence, I analyze transcripts of the banks' bi-annual shareholder meetings that cover their operations from 1865 to 1867. These transcripts provide qualitative evidence on the nature of each bank's business before, during, and after the crisis. Overall, there is no evidence that differences in local economic conditions or bank risk-taking behavior affected their failure rates.

Ex-ante, banks that failed do not appear to be more risk-taking, as measured by the amount of funds they added to their reserve, and their own assessment of the riskiness of their investments. The characterization of economic conditions and opportunities in the markets they served also do not vary systematically across banks that failed and those that did not. During the crisis, those that failed emphasize idiosyncratic circumstances such as instances of fraud or the panic itself. Ex-post, banks that failed cite the panic conditions as the primary reason for closure. These qualitative characteristics of the banks are also not systematically correlated with the public Overend connection. The full results, discussion of the sources, and examples of the language are in Appendix C.6.

3.3.3 Overend & Gurney connection

The primary explanation for why some banks failed is a public connection to Overend & Gurney. Although the firm's shareholders were protected by limited liability, the nominal value of the shares were £50, of which the investors had only paid in £15 and were at risk

⁴⁰Examples of such shocks include weather patterns that affected agricultural output or regional boom-and-bust patterns.

for a call of an additional £35 per share.⁴¹ The shareholder list circulated in London at 2.5 times the publishing price during the crisis, and contemporary evidence indicates that depositors found this a valuable piece of information.⁴²

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).⁴³ A bank is characterized as having a known connection to Overend & Gurney if one of its managers and 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. Moreover, there is little correlation between the Overend connection and the observable measures of bank health, indicating that observably weaker banks are not more likely to have personnel invested in Overend (Table C2).⁴⁴

3.4 Correlation between location characteristics and bank failures

The identification strategy is only threatened if the London headquarter failures were either caused by characteristics or events in the banks' subsidiary locations that could affect exports or if they were exposed to simultaneous shocks that were correlated with both failure and lower exports. I follow Borusyak, Hull and Jaravel (2021) and test the exogeneity of bank-level failure rates to location-level characteristics by calculating each bank's exposure to those characteristics and correlating them with the bank failure rates.⁴⁵

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 num-

⁴¹£35 in 1866 is equivalent to £4,193 in 2020.

⁴²See Appendix C.3 for contemporary documentation of the crisis and the demand for the shareholder list.

⁴³The shareholder list was found at the Royal Bank of Scotland archives in Edinburgh, Scotland. January, 1866 was the last list that was compiled before the firm declared bankruptcy.

⁴⁴Overend'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.

⁴⁵The 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.

ber of ships from the *Lloyd's List*), importance of the United Kingdom as a destination, geodesic distance to London, latitude, number of destinations, availability of non-British financing, and whether the port is a capital city.⁴⁶ At the country-level, observable characteristics include total value of exports, value of exports by industry, share of commodities in the composition of exports, monetary system, and whether the country was engaged in conflict. These characteristics help to capture heterogeneity in size and trade patterns. Each bank's share-weighted average exposure \bar{X}_b to these pre-crisis characteristic X_l is calculated as $\bar{X}_b = \frac{\sum_l z_{lb} \times X_l}{\sum_l z_{lb}}$ where larger weights are given to locations more dependent on bank b . The normalized individual bank failure rates are regressed on the transformed location-level characteristics \bar{X}_b :⁴⁷

$$\mathbb{I}(\text{Failure}_b) = \alpha + \beta \bar{X}_b + \varepsilon_b \quad (4)$$

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 B5. 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 F12, I provide balance checks for all remaining SITC industries. The location-level characteristics that are correlated with bank failure rates are included as controls in the main empirical specifications to address their potentially confounding effects.

⁴⁶Results are similar using sailing distance (without access to the Suez Canal) instead of geodesic distance to London. Figure B4 shows the strong positive correlation between the two types of distances.

⁴⁷The 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 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 a high enough degree of substitutability among producers.⁴⁸ The sunk cost of establishing a trade relationship with a given destination (which must be repaid if the exporter exits) covers expenses such as learning about the local market and setting up communication and distribution networks. They are considered sunk because losing contact with the local market will require the exporter to embark on the costly process of re-establishing those relationships.

Production is modeled following Melitz (2003) with heterogeneous firms and monopolistic competition. Firms produce differentiated varieties and face a marginal cost to export comprised of three elements: the cost of producing, iceberg trade cost τ , and financing cost which scales with volume.⁴⁹ The entry costs imply that there are increasing returns to scale and not all firms will be productive enough to export to a given destination. Consumers have constant elasticity of substitution (CES) preferences which generate the standard optimal pricing strategy of a constant markup over marginal costs. Lower financing costs or higher productivity allow firms to lower prices, thereby gaining a larger market share and generating higher profits. There is a free entry condition within each destination so that firms will enter new markets as long as the sum of their discounted profits covers the sunk entry cost. Appendix E contains the full model specification, extensions, and discussion.

I illustrate the dynamics of a financial shock over three periods: a pre-shock equilibrium T_0 , a temporary shock T_s , and a post-shock equilibrium T_1 . Figure E1 plots the productivity cutoffs necessary for firms to profitably export to a destination while Figure E2 plots the price index of traded goods there. This framework generates five empirical predictions.

Prediction 1: Lower intensive margin of exports.

The intensive margin of exporters is composed of incumbent firms that have already paid the entry cost and are already exporting to a destination. Incumbents exposed to bank failures

⁴⁸In the classic Baldwin (1988) framework, firms produce a homogeneous good and are completely substitutable and 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.

⁴⁹Positive iceberg trade costs of $\tau (< 1)$ imply that if 1 unit is shipped, only $1/\tau$ arrives. The financing cost is assumed to scale with the shipment size to capture the exporter's working capital requirements. All firms within a location (measured either as ports or countries) are assumed to have access to the same amount of external financing, and therefore will be symmetrically exposed to bank failures, so financing is not directly tied to productivity.

will face higher production costs which they partially pass on to the prices they charge at the destination. This makes them more expensive and causes them to export relatively less than unexposed incumbents.

Prediction 2: Lower extensive margin of exports.

Holding fixed productivity, the higher financing costs will make exposed firms more expensive, have lower market share, and be less profitable. Their lower profitability will decrease their likelihood of beginning to trade with new destinations. However, exposed incumbents may not necessarily exit from destinations since there is option value to not repaying the sunk cost.

Prediction 3: Persistent market share losses.

During the shock, the higher prices by exposed incumbent exporters will raise the price index of traded goods in the destination ($P_s > P_0$) which reduces the productivity necessary to profitably export to the destination. This will induce entry by new firms until a new equilibrium is reached. Since exposed incumbent firms do not necessarily exit, the greater mass of firms and varieties in a destination will imply that the post-shock price index is lower ($P_1 < P_0$). All else equal, this change in the destination market competitiveness makes it more difficult for firms to enter afterward. Note that additional forces endowing firms with first-mover advantages, such as progressively tailoring products to a destination will make these initial market share gains more persistent.

Prediction 4: Larger effects among substitutable exporters.

Firms exporting more similar goods (higher elasticity of substitution σ) will differentially benefit when their competitors are exposed to the shock. Since higher σ raises the pass-through of marginal costs to final prices, for a given financing shock, the price difference between exposed and unexposed exporters is higher when the goods are more similar.

Prediction 5: Smaller effects when financing shock is dampened.

Characteristics of exporters (such as alternative sources of financing) or of the trade relationship (such as having a lower trade cost) that reduce the price impact of exposure will also reduce the intensive and extensive margin losses immediately and in the long-run.

5 Results

The conceptual framework in Section 4 generates several empirical predictions. I provide evidence of the short-run effects on the intensive and extensive margins of exports (predictions 1 and 2) using the port-level data, which aggregates exports (proxied by the number

of ships) from each port p in each country o during the pre- and post-shock periods t . I then turn to the country-level panel of bilateral exports to analyze the long-run effects (prediction 3). 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.⁵⁰

5.1 Intensive margin effects

I examine the immediate impact of bank failures on the intensive margin of exports volumes 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:

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

S_{ot} is the total number of ships departing a country per period. β is the coefficient of interest, and $Fail_o$ is an exporting country's exposure to bank failures calculated according to Equation 2 using country-level shares of pre-crisis dependence on individual banks. $Post_t$ 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 interacted with $Post_t$ that can be included as additional controls. Country fixed effects γ_o absorb all time-invariant differences in levels of shipping, including those correlated with their exposure to bank failures. Regressions are weighted by the pre-crisis size of ports, measured by shipping activity. Standard errors are clustered by the country of origin.

Table 4 Column 1 shows the baseline effect without any additional country-level controls. The coefficient of -.51 implies that countries with the average exposure of 11% 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.

While the coefficients are large and statistically significant throughout, it is possible that unobserved country-level shocks are partly accounting for the results, so next I identify

⁵⁰In neo-classical trade models featuring perfect competition in production, the micro-to-macro aggregation always holds. Redding and Weinstein (2017, 2019) show that with nested CES demand and no assumptions about the structure of production, the macro patterns of trade (cities, countries) can be decomposed into terms reflecting the more micro levels (firms, sectors) plus adjustments that reflect a Jensen's inequality term (since the sum of logs is not the log of the sum). In this institutional setting, the adjustment terms are either irrelevant or else work in the same direction as the price effects, so the firm-level predictions carry over to higher levels of aggregation. Appendix E provides more discussion of this point.

the effects using within-country variation from port-level shipping activity which 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 .⁵¹

Each port in the panel is matched to the closest city of financing by geodesic distance, and its exposure to bank failures $Fail_{po}$ is assumed to come from that city.⁵² Ports more than 500 km from the nearest city of financing are given an exposure of 0, and I include an indicator for these ports (interacted with the post time period) so that there is a control group of completely unexposed ports.⁵³ 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 post-crisis. X_{po} are pre-crisis port characteristics that can be included as additional controls. Port fixed effects α_p absorb all time-invariant port-specific differences in levels of shipping, and origin-country-period fixed effects γ_{ot} flexibly control for all observed and unobserved characteristics at the country-level that affect shipping. Including these fixed effects means β is identified by comparing ports from the same country and year.⁵⁴ Standard errors are clustered by the country of origin to allow for heteroskedasticity and within-country spatial correlations.

Table 4 Columns 2–7 presents the baseline results with controls added individually. The coefficient of -0.64 in Column 7 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. I further address the concern that bank failures are correlated with other factors that are responsible for the decline in exports by including observable port-level characteristics as controls in the baseline regression. These 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.⁵⁵ The coefficients in Table 4 Columns 3–7 after including these controls remain stable

⁵¹The estimating equation is $\ln(S_{pot}) = \beta(Fail_{po} \times Post_t) + \Gamma' X_{po} \times Post_t + \alpha_p + \gamma_{ot} + \varepsilon_{pot}$

⁵²For example, the port of Piraeus in Greece is designated as receiving its funding from Athens.

⁵³The 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 B6 for the coefficient plot for the baseline specification estimated using different distance cutoffs.

⁵⁴Countries with only one port are effectively dropped from this estimation. These account for 16 of the 578 observations (2.8 percent). These come from 8 ports, which reduces the effective number of countries in the estimation from 54 to 46. There are on average 5 ports per country.

⁵⁵Bank-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.

and statistically significant.

Implementing the recommended bounds in Oster (2019) shows that selection on location-level unobservable characteristics is minimal. These bounds are calculated using changes in the magnitude of the coefficient and the R^2 after controlling for observable characteristics. β^* is the inferred true coefficient if the unobserved bias is as large as the observed bias, and δ is the inferred bias that could induce the estimated β to be zero. I report these as β^* and δ in the last two rows. These calculations show that β^* is almost identical to the estimated β , and that the degree of unobservables bias would have to be at least 40 times larger than the degree of observables bias.

5.1.1 Robustness

I provide a variety of robustness checks for the immediate effects in Appendix F.1. First, I expand the control variables to include all country and port-level characteristics, even ones that do not appear statistically significantly correlated with bank failures (Tables A4 and F1, respectively). Second, I show that the baseline effects are not due to demand shocks both specifically from the UK and more generally (Table F2). Third, I exclude cotton-exporting countries individually and as a group because they may have experienced a correlated shock due to the end of the American Civil War (Table F3). Fourth, I use the time-series granularity of the *Lloyd's List* and estimate the effects allowing for communication lags from London to cities around the world (Figure F1 and Table F4). Fifth, I account for potential mismeasurement in the outcome variable by re-estimating all the results using count data methods, limiting the sample to well-traveled routes to further diminish the impact of outliers along routes, and showing similarity to annual regressions using country-level values of exports (Figure F2, Tables F5 and A6). The results are robust to all of these checks.

5.2 Extensive margin effects

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 at all.

First, I estimate the changes to the extensive margin number of destinations using the specification in Equation 5 with the log of the number of unique destinations as the dependent variable. This specification compares the number of destinations that locations more exposed to the banking shock traded with relative to those less exposed. Table 5 Columns 1–2 reports the effects for countries and ports. A one standard deviation increase in exposure reduces the number of unique destinations by 7.7% and 8.1% respectively.

The second test of extensive margin effects measures the likelihood that a port is engaging in international trade at all. Here, 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 Entry or Exit and standard errors are clustered by the origin-country:

$$\Pr(E_{po}) = \alpha + \beta Fail_{po} + \gamma_o + \Gamma' X_{po} + \varepsilon_{po} \quad (6)$$

Table 5, Columns 4 and 6 present the within-country likelihood of $Entry_{po}$ and of $Exit_{po}$, respectively. As predicted by the conceptual framework, the shock is a statistically significant deterrent to the extensive margin ability to trade with foreign markets, but it does not significantly impact $Exit$ by those who had already paid the sunk costs. A one standard deviation increase in exposure reduces a port’s probability of beginning to export by 5.3%.⁵⁶

5.3 Persistent effects

I have shown that there are large effects immediately after the crisis. I now turn to the third prediction that the shock will have persistent effects on export volumes. To do so, I now focus on the sample of country-level bilateral trade from 1850–1914.

5.3.1 Total exports

There is a striking divergence in cumulative growth of total exports across countries in the raw data which shows that exposure to the banking crisis has permanent level effects. 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 1 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 B7 plots the coefficients

⁵⁶Similarly, 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.

and standard deviations from the equivalent regression (including country and year fixed effects) where the average difference in exposure between groups is 23.8 pp. 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.

The permanent divergence in levels arises from a short-run jump in the growth rates of exports of unaffected countries from 1867–1870. In Figure B8, I plot the annual growth rate of exports and show that they are very similar pre-crisis, diverge after the crisis in 1867, and then completely reconverge to the same pattern by 1880. In the pre-crisis period from 1850–1865, the average annual growth rates are 11.6% for both the less exposed (solid line) and more exposed groups (dashed line).⁵⁷ In 1867 the less exposed group (solid line) grew 14% while the more exposed group (dashed line) grew 9%, and in 1868 the growth rates were 22% and 10% respectively. The cumulative difference in the annual growth rates between the two groups after the first two years is 17.2%. This initial difference in export growth rates is the main driver of the average annual difference in growth rates of 1.6% per year between groups from 1867–1914, and it is consistent with the prediction in the conceptual framework that unexposed exporters are able to grow due their competitors’ shock.⁵⁸

5.3.2 Bilateral exports & market-share effects

The divergence in total values of exports between more and less exposed countries could be driven in part by the importing country’s demand. In particular, if more-exposed countries tend to have stronger exporting relationships with countries who experienced slower imports growth after the crisis, their exports would be affected, but not through the financing constraint channel. I address this possibility by estimating the effect of bank failure exposure on bilateral exports volumes, which allows me to control for annual demand shocks:

$$\ln(EX_{odt}) = \beta_t Fail_o + \Gamma' X_{ot} + \gamma_o + \gamma_{dt} + \theta_t \ln(\text{dist})_{od} + \varepsilon_{odt} \quad (7)$$

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. X_{ot}

⁵⁷In the immediate pre-crisis period from 1860–1865, the average annual growth rates were 6.7% and 6.5%, respectively, and the p-value for the difference in means is 0.97.

⁵⁸The average annual growth rates from 1867–1913 are 4.5 and 2.9% for the less exposed and more exposed groups of countries, respectively. This is calculated using the 1913 values of exports, which were 8.11 and 3.86 times the values in 1866 for the two groups, respectively.

includes contemporaneous characteristics or pre-crisis origin-country characteristics that are interacted with year dummies, which control for macroeconomic differences among countries. Origin-country fixed effects γ_o control for time-invariant country characteristics and restrict the source of variation to the change in exports within each country between periods. As in the port-level estimation, I control for the effect of the origin country not having any British banks at all in 1866, which separates the effect of any exposure from the degree of exposure to failed banks.⁵⁹ Destination-country year fixed effects γ_{dt} control for demand shocks by restricting the identifying variation to being across exporters, within-destination-year. 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.⁶⁰

Equation 7 is the fixed effects estimation of a structural gravity model standard in the international trade literature (Head and Mayer, 2014) which relates the volume of trade flows to the sizes of the importing and exporting countries and the inverse of the distance (geographic and institutional) between them. This specification flexibly controls for both origin-country and destination-country changes in GDP, so these effects should be interpreted as the differential impact on exports beyond just country size and economic activity (see Appendix F.2 for further discussion). I control for the distance between countries $dist_{od}$ measured either geodisically or as the fastest travel time.⁶¹ Allowing θ_t to vary by year flexibly controls for shocks to the effective distance between countries due to technological advances. The one departure from the standard fixed effects estimation using panel data is the absence of origin-country year fixed effects because those are collinear with the treatment.

I allow β_t to vary annually and at five-year intervals ($[1850, 1855], \dots, [1911, 1914]$). β_t should be interpreted as the semi-elasticity of the relative volumes a given importer in a given year buys from the average exporting country exposed to bank failures. It is expected to be negative if increases in the exposure to bank failures reduces exports.

Figures 3a and 3b plot the estimated β_t coefficients annually and at five-year intervals, where β_{1866} and β_{1861-5} are the omitted years in each specification, respectively.⁶² β_t 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. 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 con-

⁵⁹These countries accounted for 2% of the value of exports in 1866, and results are robust to not controlling for the non-exposed group.

⁶⁰Results are robust to different ways of clustering in Table A6.

⁶¹I use Pascali (2017) for distance by either sail or steam.

⁶²Point estimates are in Table F8 (column 2).

cern 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 are 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-difference because good places with low exposure would already be growing before 1866, but this is not the case.

The persistence is striking: destination countries imported significantly less from exporters that had been exposed to bank failures for almost 40 years. The average estimated coefficient from $\beta_{1866-1870}$ to $\beta_{1986-1900}$ is -1.41 log points. This magnitude implies that the average importer bought 21% less from an exporter with one standard deviation exposure (17 pp) relative to an unexposed exporter. $\beta_{1901-05}$ is the first period when the effect is only borderline statistically different from zero. However, the average magnitude of the coefficients after 1900 is -0.81, which is still 57% of the average effect before 1900. The average estimated coefficient from 1867–1914 is -1.23 log points, and given the average exposure of 11 pp, implies that the (partial equilibrium) disruption in world exports for the post-crisis period until WWI was 13% per year.

5.3.3 Robustness

I provide robustness checks for the long-run effects in Appendix F.2. First, I control for a wide variety of initial and contemporaneous macro-economic conditions that could potentially have a confounding effect on the effects. In Table F8 columns 3–8, I show robustness to origin-country controls, including the pre-crisis characteristics that are correlated with bank failures. In Table F9, 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 F10); industry composition of exports pre-crisis and initial trade intensity with the UK (Tables F11 – F13); excluding the cotton-exporting countries (Table F14); financial crises like sovereign debt, domestic debt, stock market crashes both contemporaneous and in 1865 (Table F15 and F16); ability to issue long-term debt or equity in London (Table F17); and the number of banks of different nationalities after the crisis (Table F18). 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 F3, 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

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 7 using the simulated data. I plot the distribution of the coefficients for each group of five years in Figure F4. 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.

5.4 Discussion of magnitudes

The results presented so far indicate that the effect on aggregate export levels is permanent and that the market-share losses by exposed exporters last for several decades. The rest of the paper presents several ways to understand these empirical findings in context. First, it is important to recognize that the long-run effects reflect relative exports losses across countries each year rather than negative country-level growth rates or a decline in total world exports. Second, I benchmark the total losses in exports and GDP against estimates in the trade and macroeconomic literatures and show they are highly consistent. Third, the changes in the patterns of trade (Section 6) show significant amounts of heterogeneity which further contextualize the average results.

⁶³Note that the immediate effects are estimated using within-country variation, which already indicates this is unlikely to be the case.

5.4.1 Effect on aggregate world exports

The baseline difference-in-differences estimations of the impact of the crisis on exporter market shares unfortunately do not provide insight on the general equilibrium aggregate effects on *global* trade. In particular, large and persistent losses by exposed exporters relative to unexposed ones may not lead to reductions in world trade if importers reallocate their demand to unexposed exporters. I analyze this possibility by estimating the effect of the bank failures on a country’s imports.

I estimate the impact of the crisis on a country’s imports using the baseline specification in Equation 7, replacing the key regressor of the exporting country’s exposure with the exposure to bank failure in the importing country.⁶⁴ As in the baseline, I saturate the estimation with fixed effects to account for exporter supply shocks (γ_{ot}) and the importer’s overall size (γ_d). I present these results in Table A7. 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.⁶⁵

Given the symmetry between world imports and exports, the lack of effect on imports indicates that the crisis could not have had an aggregate effect on world trade. This lack of effect on aggregate global trade can be only be reconciled with the market share losses by exposed countries if non-exposed countries are able to compensate by exporting more to each destination market, and I discuss evidence supporting these patterns in Section 6.

5.4.2 Benchmarking the magnitudes

The long-term effects of the shock on a country’s total exports and output 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

⁶⁴Given 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, $\ln(IM_{dot}) = \beta_t \text{Fail}_o + \varepsilon_{odt}$, is equivalent to $\ln(EX_{odt}) = \beta_t \text{Fail}_d + \varepsilon_{odt}$. Note that in the notation IM_{dot} , the goods are traveling from country o to country d at time t .

⁶⁵Directly 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.

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.⁶⁶ 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.

I find a similar pattern for GDP. In Figure B9, I plot aggregate GDP for the same two groups of countries, binned by above and below average exposure to bank failures. The four-year cumulative difference in average output between countries with no exposure versus those with any exposure is 7.6%. Cerra and Saxena (2008) use a panel of 190 countries to find that the output loss in the four years after a banking crisis is 10%, after which the differences in growth rates are minimal. Over the post-crisis period to WWI, the average difference is 0.6% for GDP (compared to 1.6% for exports).⁶⁷

Second, as in the case with exports, there is no compensating growth, leaving a permanent effect on GDP levels. These dynamics are entirely consistent with the empirical evidence crises permanently lower the levels of output relative to a no-crisis counterfactual.⁶⁸ While there are instances of countries exhibiting higher growth after a shock to recover losses in levels (notably in the US), this pattern is the exception rather than the norm.

6 Changes in the patterns of trade

In the final section, I turn to predictions 4 & 5 in the conceptual framework which say that exporters will gain market share at the expense of their competitors and that factors dampening the financing shock such as other sources of financing and lower physical trade costs will reduce the immediate and long-term effects.

⁶⁶In other words, a 1% increase in physical distance between two countries reduces the trade flows between them by 1.1%. 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 A5 reports the estimates and robustness to controlling for gravity measurements of bilateral resistance.

⁶⁷This 0.6% difference is incidentally equivalent to the pre- versus post-2008 average growth rate in the United States, which has not displayed the historical tendency to recover lost levels of output. The average annual GDP growth from 2000–2007 was 2.7% and from 2010–2017 was 2.1%. As in the Great Trade Collapse of 2008, the difference in exports is much larger than the difference in GDP, so the trade-specific losses cannot be driven by productivity declines that also affect output. Given that the macroeconomic estimates do not account for the endogeneity in experiencing a crisis in the first place, the lower magnitudes in this paper appear highly plausible.

⁶⁸Cerra, Fatás and Saxena (2020) provides an in-depth review and synthesis of this long literature.

6.1 Comparing substitutable exporters

As I showed in Sections 5.1 and 5.2, a trade cost shock between parties leads importers to increase the amount they buy from unexposed relationships and to source from new relationships. In the 19th century, most countries exported commodities, which were often produced by multiple other countries, suggesting there was a high degree of substitutability across countries. For example, a country importing sugar could choose among a number of 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 larger 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 exports pre-crisis, categorized by two-digit SITC codes, to test for importers substituting among similar countries. I estimate the baseline specification in Equation 7 with time-varying industry controls where each country is assigned the SITC industry of its biggest exports in 1865.⁶⁹ The SITC industry controls mean that β_t should be interpreted as the loss of market share into a given destination in a given year by an exporting country *relative* to other countries whose exports 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 F8 column 6 and plotted in Figure B10a).

Next, I broaden the measure of a country's exports 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 exports categories by SITC codes and calculate the fraction of the total value of exports from the region that fall into those categories.⁷⁰ This fraction is equivalent to an exports-weighted average of the cross-country exports concentration within the top three categories. Figure B11 shows that this fraction is above 0.5 for all regions and averages 0.73 across regions, indicating that the industry composition of exports is very similar within region.

I compare the countries within regions to each other by including origin-country region-year fixed effects in the baseline specification in Equation 7. The additional controls restrict the variation such that β_t is estimated off comparisons of countries in the same

⁶⁹The global value of exports by SITC is shown in Figure B5.

⁷⁰Each region has at least two countries, and the primary exports for all countries outside of Northwest Europe are raw commodity goods.

geographic area exporting to the same destination in the same year. Figure 4 (Table F8 column 8) shows that there is no recovery in this setting implying that countries that are more exposed to bank failures experience exports losses 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 F8 column 7 and plotted in Figure B10b).

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.⁷¹ 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 B12 shows that negative shocks to a country's competitors does benefit its own exports into a given market.⁷² 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.⁷³

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.⁷⁴ As in the 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 F5. 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.

⁷¹The specification is: $\ln(EX_{odt}) = \psi_t \text{Region Fail}_{o,t} + \beta_t \text{Fail}_o + \gamma_o + \gamma_{dt} + \theta_t \ln(\text{dist}_{od}) + \varepsilon_{odt}$ where $\text{Region Fail}_{o,t}$ is the average exposure of other countries in a region for a given country o .

⁷²This 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.

⁷³Bilateral industry-level exports is systematically unavailable in the historical trade data.

⁷⁴This measure is calculated by removing each city of financing's contribution from the country-level 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: $\ln(S_{pot}) = \beta \text{Fail}_{po} \times \text{Post}_t + \psi \overline{\text{Fail}}_{\text{other},o} \times \text{Post}_t + \alpha_p + \Gamma' X_{pot} + \varepsilon_{pot}$.

6.2 Lower financial needs

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 after the bank failures.

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.⁷⁵ Figure 5 plots the baseline effect of exposure β_t in orange and the additional effect of failure for close relationships $\theta_{t,close}$ in blue. β_t is very similar to the baseline effect in previous estimations. $\theta_{t,close} > 0$ indicates that conditional on exposure to bank failures, exports to closer destinations are positively affected. The main effect for exports to close destinations is given by $\theta_{t,close} + \beta_t$, which is close to zero. The results are robust to constructing the variable over all years or at the end of the sample in 1914. The qualitative interpretation is that a country’s exports losses are borne by more distant trading partners.

6.3 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.⁷⁶ ϕ 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 ($\phi > 0$). At the port-level, there is no correlation between the pre-crisis number of non-British banks and bank failure, so this result is not driven by any trends correlated to non-British banks. The magnitude of ϕ 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

⁷⁵The specification is: $\ln(EX_{odt}) = \theta_{t,close} \text{Fail}_o \times \mathbb{1}(\text{Close}_{od}) + \beta_t \text{Fail}_o + \lambda_t \mathbb{1}(\text{Close}_{od}) + \Psi' X_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}$

⁷⁶The specification is: $\ln(S_{pot}) = \beta \text{Fail}_{po} \times \text{Post}_t + \phi \text{Fail}_{po} \times \text{non-Brit}_{po} \times \text{Post}_t + \alpha_p + \gamma_{ot} + \Gamma' X_{pot} + \varepsilon_{pot}$

by using the nationalities and identities of the multinational banks within each city in the five year windows from 1850–1914.⁷⁷ 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*” ($\mathbb{I}(\text{EB}_o)$) that takes the value of 1 when the exporting country has 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.⁷⁸ Figure 6 plots β_t in orange and θ_t in blue. Interacting $\mathbb{I}(\text{EB}_o)$ 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 without access to other financing networks are the ones driving the main losses seen in Figure 3.

7 Conclusion

Despite the prevalence of financial shocks and their known impact on short-run outcomes, there is little causal evidence showing how long these temporary events can have long-run impacts on international trade. 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.

⁷⁷Data are from Kisling, Meissner and Xu (2020).

⁷⁸The specification is: $\ln(\text{EX}_{odt}) = \theta_t \text{Fail}_o \times \mathbb{I}(\text{EB}_{od}) + \beta_t \text{Fail}_o + \lambda_t \mathbb{I}(\text{EB}_{od}) + \Psi' X_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}$

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Tables

Table 1: Summary statistics: ports and countries

	<u>Ports</u>			<u>Countries</u>		
	mean	median	sd	mean	median	sd
Exposure to failed British banks	0.07	0.00	(0.19)	0.11	0.03	(0.17)
Exports	127.99	32.00	(231.05)	12.49	2.15	(32.96)
Fraction exports to UK	0.39	0.30	(0.34)	0.62	0.69	(0.37)
Destinations (# countries)	7.60	5.00	(7.28)	3.95	2.00	(8.32)
Distance to destination (km k)	5.31	5.12	(3.48)	6.12	5.26	(3.51)
Banks	6.03	3.00	(7.54)	5.27	1.00	(9.96)
Non-British banks	0.60	0.00	(1.06)	2.97	0.00	(8.74)
Fraction in British Empire	0.34	0.00	(0.47)	0.33	0.00	(0.47)
<i>N</i>	289			55		

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)

	All		Not Failed		Failed		Diff	
Capital, authorized (£m)	1.48	(1.06)	1.42	(1.06)	1.75	(1.05)	-0.33	(0.29)
Capital, paid up (£m)	0.59	(0.38)	0.61	(0.38)	0.47	(0.41)	0.14	(0.10)
Deposits (£m)	2.22	(2.73)	2.29	(2.82)	1.85	(2.37)	0.44	(1.14)
Reserve fund (£m)	0.13	(0.12)	0.13	(0.11)	0.16	(0.17)	-0.03	(0.04)
Total size (£m)	4.81	(6.11)	5.04	(6.41)	3.83	(4.65)	1.21	(1.89)
Leverage ratio	0.23	(0.11)	0.24	(0.11)	0.23	(0.11)	0.01	(0.04)
Reserve ratio	0.06	(0.07)	0.06	(0.07)	0.06	(0.06)	0.01	(0.03)
Liquidity ratio	0.14	(0.11)	0.14	(0.09)	0.12	(0.15)	0.02	(0.03)
<i>N</i>	95		77		18		95	

Panel B: Other characteristics (all banks)

	All		Not Failed		Failed		Diff	
OG connection	0.18	(0.39)	0.15	(0.36)	0.33	(0.48)	-0.18	(0.1)**
Trade finance (£th)	105.79	(246.77)	111.78	(263.41)	75.30	(133.34)	36.48	(59.0)
Age (years)	36.24	(53.15)	40.65	(56.38)	13.76	(21.34)	26.89	(12.5)**
Cities (#)	13.76	(22.88)	14.79	(24.54)	8.48	(9.97)	6.32	(5.5)
Countries (#)	7.63	(8.89)	7.85	(9.24)	6.52	(6.94)	1.33	(2.1)
Asia (£th)	46.01	(170.09)	48.93	(184.16)	31.13	(60.75)	17.80	(40.7)
Africa (£th)	8.17	(25.08)	7.06	(21.86)	13.83	(37.69)	-6.76	(6.0)
N. America (£th)	13.38	(44.37)	15.25	(47.99)	3.85	(13.37)	11.39	(10.6)
S. America (£th)	6.99	(34.12)	7.83	(37.07)	2.70	(9.21)	5.13	(8.2)
Australia (£th)	6.41	(17.25)	7.08	(18.52)	2.98	(7.54)	4.10	(4.1)
Europe (£th)	12.25	(27.40)	10.80	(25.33)	19.62	(36.03)	-8.82	(6.5)
Brit. Emp. (£th)	48.24	(149.39)	53.11	(161.79)	23.46	(47.13)	29.65	(35.7)
UK (£th)	12.59	(40.28)	14.83	(43.72)	1.19	(2.73)	13.63	(9.6)
<i>N</i>	128		107		21		128	

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. 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

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

Panel A: Port characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ships	0.0324 [0.0362]							
Steam ships		0.0188 [0.0336]						
Fraction to UK			0.161*** [0.0324]					
Distance to London				-0.0583 [0.0499]				
Latitude					0.0719 [0.0381]			
Non-Brit banks						-0.0345 [0.0260]		
Destinations							-0.0550 [0.0325]	
Capital city								-0.126*** [0.0376]
N	122	122	122	122	122	122	122	122

Panel B: Country characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exports values	0.109 [0.0594]						
Fraction commodities		-0.0257 [0.0303]					
Gold			-0.0669 [0.0358]				
Silver				-0.00144 [0.0413]			
Bimetallic					0.0580 [0.0362]		
Conflict: any						0.0517 [0.0441]	
Conflict: interstate							-0.0104 [0.0395]
N	128	128	128	128	128	128	128

Panel C: Country characteristics: exports composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cotton, raw	-0.0250 [0.0726]							
Cotton, manufactured		-0.160 [0.116]						
Grains			0.157 [0.107]					
Bullion				-0.0678 [0.108]				
Sugar					0.105** [0.0456]			
Coffee						-0.0128 [0.0403]		
Alcohol							-0.0247 [0.0366]	
Tobacco								-0.123 [0.126]
N	128	128	128	128	128	128	128	128

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 $\mathbb{I}(\text{Failure}_b)$, 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. 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. In Panel A column 9, there are only 73 banks that advertised their management personnel. These are smaller banks, and excluding them entirely makes no difference for the country-level results either.

Table 4: Intensive margin effect of bank failures on shipping

$$\ln(S_{pot}) = \beta \text{Fail}_{po} \times \text{Post}_t + \Gamma' X_{po} \times \text{Post}_t + \alpha_p + \gamma_{ot} + \varepsilon_{pot}$$

	Country		Port				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fail _{po} × post	-0.514** [0.225]	-0.707*** [0.154]	-0.722*** [0.157]	-0.618*** [0.163]	-0.686*** [0.180]	-0.731*** [0.179]	-0.637*** [0.201]
Capital city × post			Y				Y
Age of banks × post					Y		Y
Fraction to UK × post				Y			Y
OG connection × post						Y	Y
Country _o × post FE		Y	Y	Y	Y	Y	Y
Port _p FE		Y	Y	Y	Y	Y	Y
N	108	578	578	578	578	578	578
Ports		289	289	289	289	289	289
Clusters	54	54	54	54	54	54	54
β*	-.515	-.714	-.729	-.621	-.692	-.739	-.641
δ	40.37	50.67	53.14	47.64	40.01	52.21	39.99

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. Fail_{po} is the share of the port's banks that failed during the crisis. The mean of Fail_{po} is 0.07, and the standard deviation is 0.2. Post is a dummy for the 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. Results from implementing the Oster (2019) test of selection on unobservable characteristics are reported in the last two rows. β* is a bound on Fail_{po} × post if selection on unobservable is as large as selection on observables (δ = 1). δ is the degree of selection on unobservables necessary for the estimated coefficient to be 0. 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

	Country destinations	Port destinations	I(Port Entry)		I(Port Exit)	
	(1)	(2)	(3)	(4)	(5)	(6)
Fail _o × post	-0.484*** [0.163]					
Fail _{po} × post		-0.426*** [0.135]				
Fail _{po}			-0.159** [0.0648]	-0.281*** [0.0876]	0.159 [0.120]	0.0602 [0.162]
Port controls × post		Y				
Port controls			Y	Y	Y	Y
Port _p FE		Y				
Country _o × post FE		Y				
Country controls × post	Y					
Country _o FE	Y			Y		Y
N	108	574	331	331	318	318
Ports		286	331	331	318	318
Clusters	54	54	55	55	54	54

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, “ $\mathbb{I}(\text{Port Entry})$ ” 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, “ (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

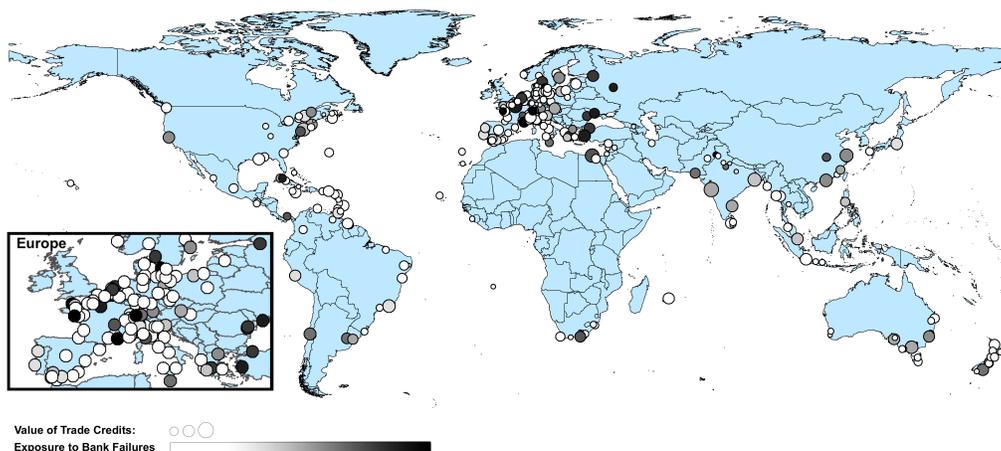
	(1)	(2)	(3)	(4)	(5)
Fail _{po} × post	-0.711*** [0.248]	-0.819*** [0.255]	-0.645** [0.277]	-0.936*** [0.227]	-0.906*** [0.279]
Fail _{other, po} × post	0.0912 [0.0715]	0.139* [0.0745]	0.270** [0.110]		
non-Brit banks × Fail _{po} × post				0.290*** [0.111]	0.287*** [0.110]
Port controls × post		Y			Y
Destination _d × post FE			Y		
non-Brit banks × post				Y	Y
Port _p FE	Y	Y	Y	Y	Y
Country _o × post FE				Y	Y
N	578	578	2532	578	578
Ports	289	289	262	289	289
Clusters	54	54	51	54	54

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

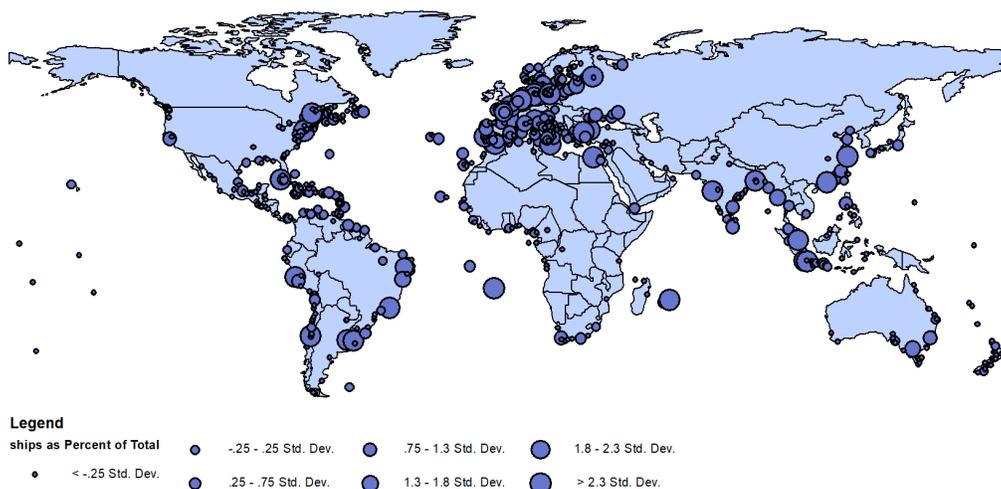
Figures

Figure 1: Geography of banking and trade

(a) British multinational bank lending and failures



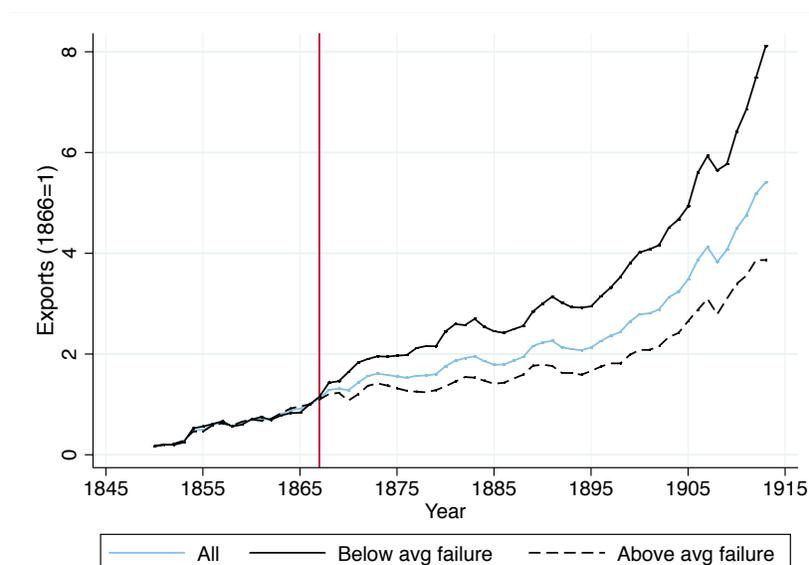
(b) Port-level trade activity



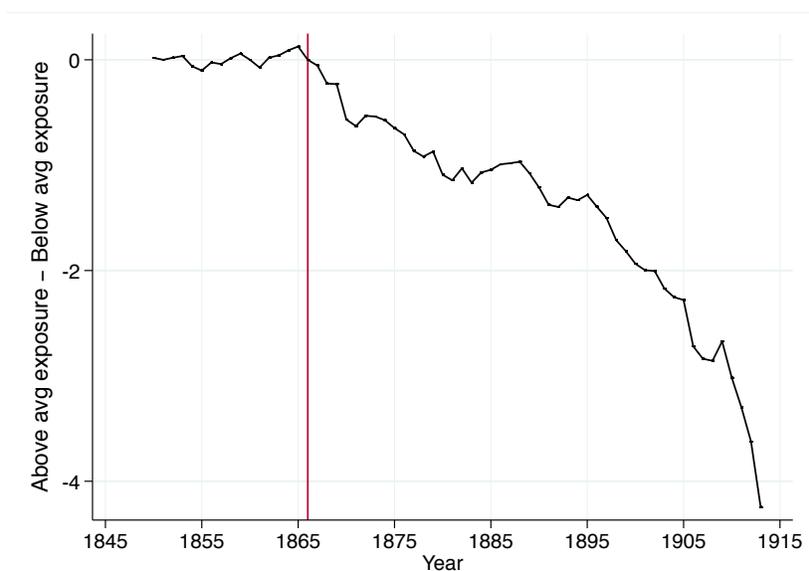
Notes: Figure 1a maps the distribution of the city-level exposure to bank failures $Fail_l$. The size of the points denote the log value of total credit at each city and the color gradient denotes the exposure to failure, ranging from 0 to 1. Figure 1b maps the distribution of shipping activity at ports 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



(b) Difference between groups

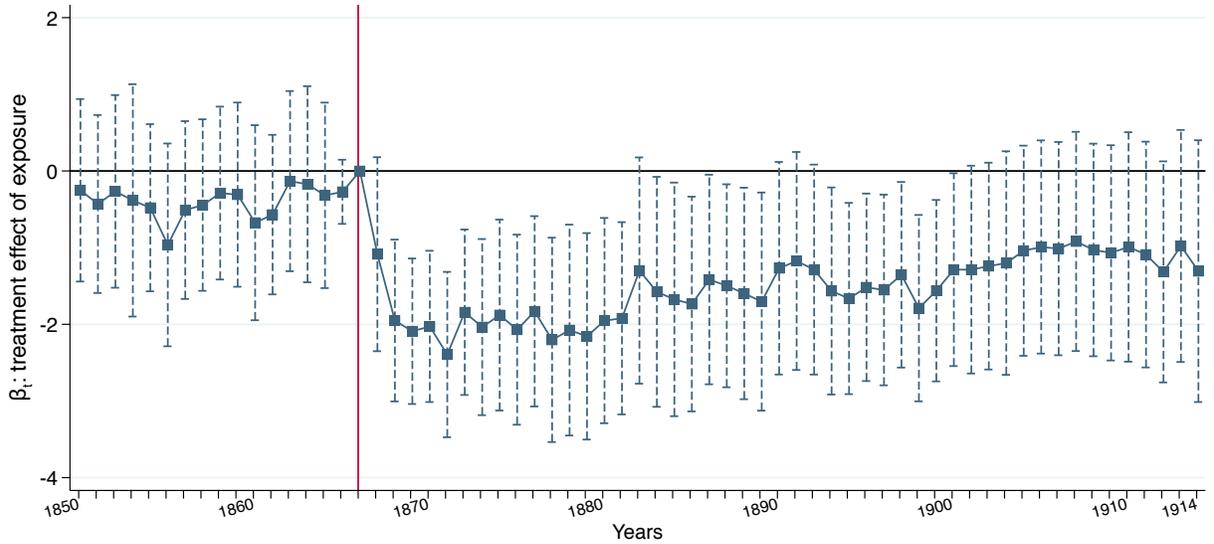


Notes: Figure 2a plots the raw data for the total value of exports by groups of countries from 1850–1914. Countries are binned into two categories: “Below avg failure” refers to countries that experienced below average exposure to bank failures in London, where the average rate was calculated in the 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 B7 plots the coefficients and standard errors from the equivalent regression.

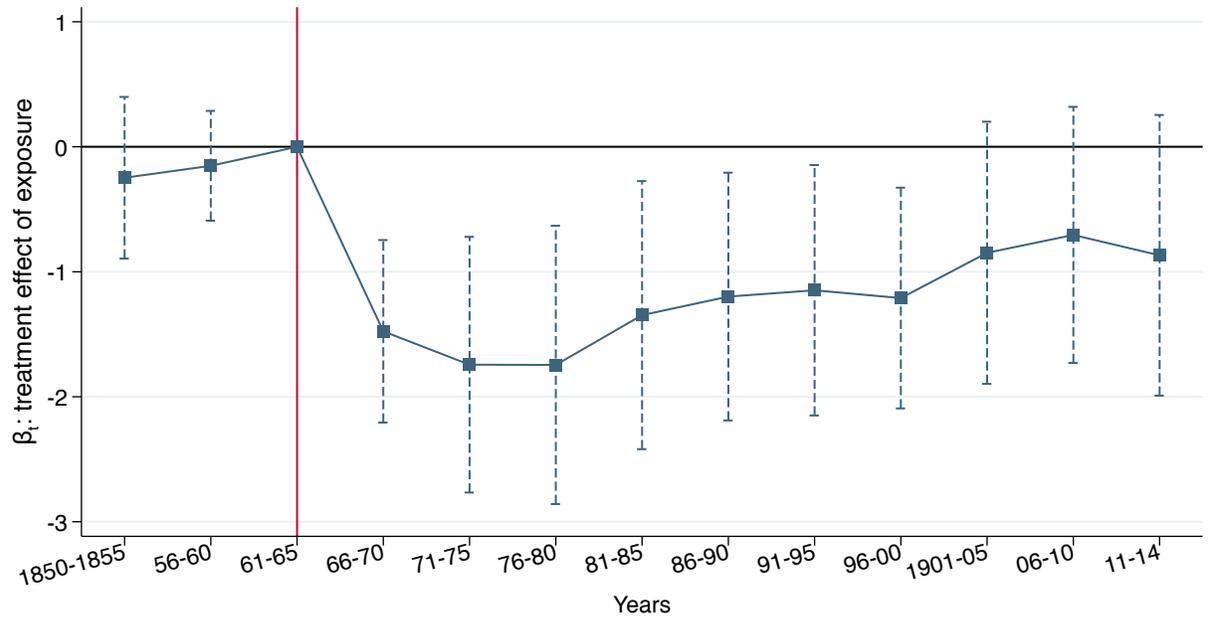
Figure 3: Persistent effect of financing shock on market share

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

(a) β_t estimated annually



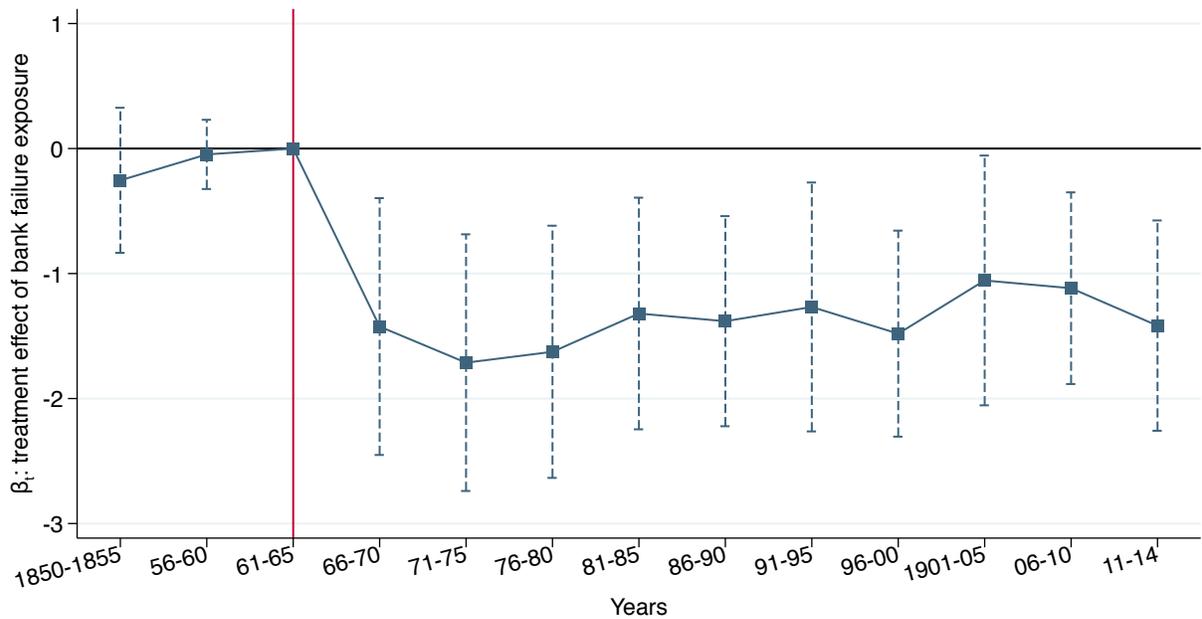
(b) β_t estimated every 5 years



Notes: Figure 3 plots the β_t point estimates and 95 percent confidence intervals for the specification given in equation 7 estimated on the country-level panel of trade. 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. β_t is the treatment coefficient on the effect of exposure to failed banks on exports in each group of years. Standard errors are clustered by the origin country. See Table F8 column 1 for the point estimates. $N = 67,378$.

Figure 4: Persistent effects within groups of countries with similar exports

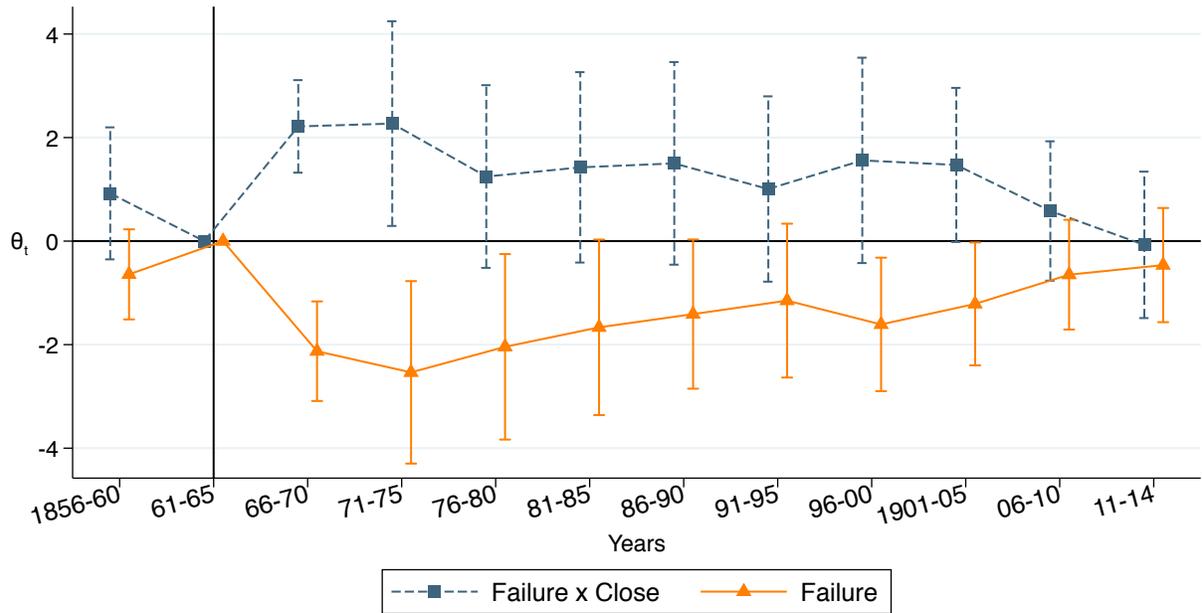
$$\ln(\text{EX}_{odt}) = \beta_t \text{Fail}_o + \Gamma' X_{ot} + \gamma_o + \gamma_{dt} + \psi \text{Region}_{ot} + \theta_t \ln(\text{dist})_{od} + \varepsilon_{odt}$$



Notes: Figure 4 plots the point estimates and 95 percent confidence intervals for the specification given above estimated on the country-level panel of trade. 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. β_t is the treatment coefficient on the effect of exposure to failed banks on exports in each group of years. Standard errors are clustered by the origin country. See Table F8 column 8 for the point estimates. $N = 67,378$.

Figure 5: Heterogeneity of recovery by trade distance

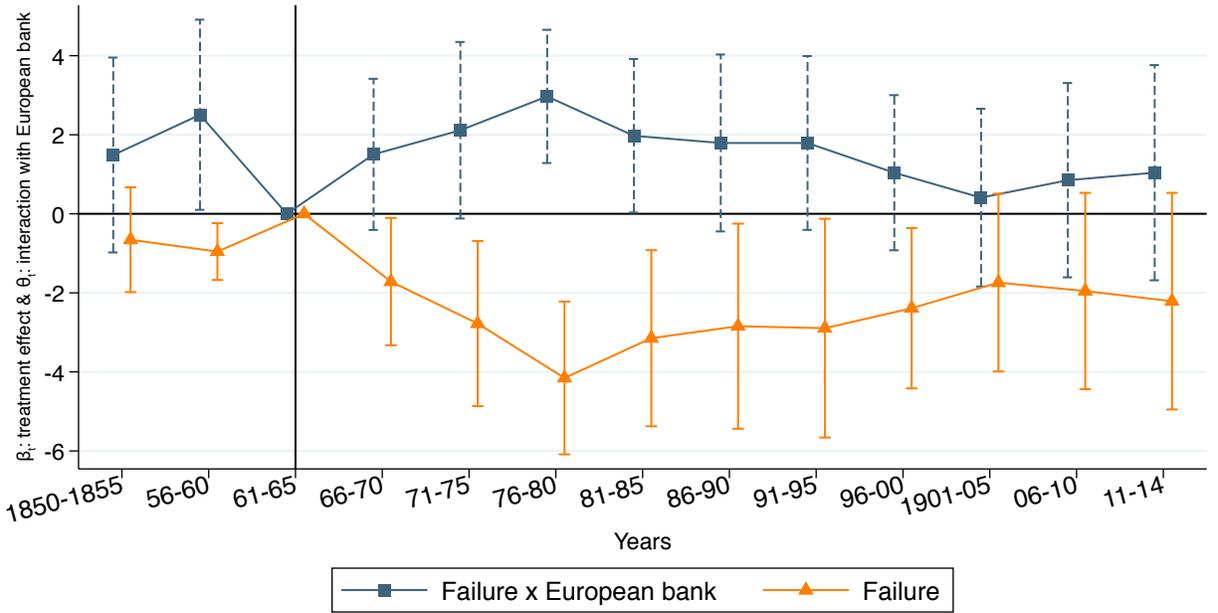
$$\ln(\text{EX}_{odt}) = \theta_{t,close} \text{Fail}_o \times \mathbf{1}(\text{Close}_{od}) + \beta_t \text{Fail}_o + \lambda_t \mathbf{1}(\text{Close}_{od}) + \Psi' X_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}$$



Notes: Figure 5 plots the θ_t and β_t point estimates and 95% confidence intervals from the country-level panel of trade in the specification given above. The dependent variable is the log value of exports. The specification includes origin country o FE, destination country-year dt FE, and time-varying indicators for common land border, common European colony, and common language. “Failure \times Close” is the treatment coefficient on the effect of exposure to failed banks on exports to countries that are less than the average distance away from the destination country, where the average is measured by 1865 bilateral trade flows. Standard errors are clustered by the origin country. $N = 66,791$.

Figure 6: Heterogeneity of recovery by access to other banks

$$\ln(\text{EX}_{odt}) = \theta_t \text{Fail}_o \times \mathbb{I}(\text{EB}_{od}) + \beta_t \text{Fail}_o + \lambda_t \mathbb{I}(\text{EB}_{od}) + \Psi' X_{od} + \gamma_o + \gamma_{dt} + \varepsilon_{odt}$$



Notes: Figure 6 plots the β_t and θ_t point estimates and 95 percent confidence intervals for the specification given above estimated on the country-level panel of trade. The dependent variable is the log value of exports. The specification includes origin country o FE, destination country-year dt FE, time-varying controls for the bilateral distance between countries, and time-varying indicators for common land border, common European colony, and common language. “Failure \times European banks” is the interaction effect of exposure to failed banks on exports in countries with access to other European banks. “Failure” is the treatment effect of exposure to bank failures for all countries. Standard errors are clustered by the origin country. $N = 67,378$.