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IN THE UNITED KINGDOM 1870-1913 AND THE UNITED STATES 1920-30

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ABSTRACT

Do international trade and finance flow together? In theory, trade and finance can be substitutes or complements, so the matter must be resolved empirically. We study trade and financial flows from the United Kingdom from 1870 to 1913 and the United States in the interwar years. Trade and finance are robustly correlated, even after allowing for simultaneity. Evidence from the British Empire casts doubt on the idea that trade is a punishment device in the event of a default.

Alan M. Taylor
Department of Economics
University of California
One Shields Avenue
Davis, CA 95616
and NBER
amtaylor@ucdavis.edu

Janine L. F. Wilson
Department of Economics
University of California
Davis, CA 95616
jlflathmann@ucdavis.edu

Recent research on economic globalization, both in its present form and its past incarnations, has vastly expanded our understanding of the evolution of the global economy and the empirical contours of its long run development. Studies of international markets in the nineteenth and twentieth centuries tell a story of two globalizations and broader analytic narratives embed these phenomena in a discussion of political economy forces and shifting constraints on national economic policymakers (see, e.g., Eichengreen 1996; O'Rourke and Williamson 1999; Bordo, Taylor, and Williamson 2003; Obstfeld and Taylor 2004).

Although this research agenda has expanded our knowledge of the integration and disintegration of goods and factor markets across time and space, one less studied dimension is the interrelationships between all of the different facets of globalization that have ebbed and flowed together at various times. One might ask the question: is economic globalization a collection of disparate and disjoint phenomena in various markets that happen to coincide, or does it reflect, in whole or in part, the actions of deeper structural forces that work in concert to stimulate greater economic interdependence? In this paper we make a first step toward a historical understanding of one such linkage as we examine whether trade and financial integration evolve as distinct processes, or whether they bear some symbiotic relationship to one another. In other words, we ask: do countries that trade more with one another also tend to be countries with larger bilateral financial flows?

The gross features of the data over the last century or two can motivate the question further and strongly hint at some kind of simultaneity. Figure 1(a) shows the ratio of international trade (exports plus imports) to world GDP from 1800 to 2000 based on various standard sources. Figure 1(b) shows, for the same period, the ratio of international investment (foreign asset stocks) to world GDP. The correlation of the two time series is striking, and

conforms to the conventional wisdom: in the long run globalization has proceeded in a nonlinear fashion in both goods and capital markets, with periods of globalization 1800–1914 and 1945–present punctuated by an era of deglobalization during the turbulent interwar years 1914–45. So what? Although these gross correlations are interesting on a heuristic level, such coarse data can offer only the weakest *prima facie* evidence of simultaneity.

Our paper will therefore have to call upon better data and econometric techniques to answer the question: “do trade and finance flow together?” We attempt the first unified systematic study of both pre–1914 and interwar financial flows from the two main creditor countries in each period: the United Kingdom from 1870 to 1914 and the United States in the interwar years. Britain and the United States were also major trading nations in the respective periods, and therefore form a natural test case. As such, we focus our attention on the two hegemons—the most influential lenders and the center countries in the world capital market in each epoch. Kindleberger (1984) famously noted the loss of hegemonic power by Britain and traced it to the burdens of World War One; he also noted the reluctance with which the United States assumed the mantle of leadership in the 1920s. He and others have supplied ample narrative evidence for this shift. Our study can address several questions related to the much debated transfer of hegemonic power from London to New York, as leadership passed from one financial center to another.¹

Our paper will also speak to the question of whether the correlation of trade and financial flows merely stems from a common factor, or omitted variable. Several candidates come to mind

¹ Quantitative evidence on the hegemonic transfer is hard to come by, and meaningful hypotheses difficult to formulate, but Obstfeld, Shambaugh and Taylor (2004) find that after 1914 the benchmark “base” interest rate for countries pegging to the gold standard was increasingly the United States (and possibly France) rather than the rate in the British money market.

for such a variable or variables, the most obvious being those that affect transaction costs in both markets: for example, changes in technology (innovations that simultaneously affect the costs of arbitrage in each market) or in policy (a shifting political landscape that simultaneously changes the restrictions in each market). Theory suggests many possible detailed channels of this sort. As we shall see, they are relatively easy to enumerate, but rather difficult to distinguish empirically; but the mere possibility of their existence in any form does pose an empirical challenge. If trade and capital controls rise and fall together due to a common factor, then the correlation of the two variables would not allow us to infer any structural relationship between the two.² One answer to this problem is to note that the two test cases will allow us to test for the link between trade and finance in two very different eras: a prewar era of virtual laissez faire in goods and capital markets, and an interwar era of increasing frictions in both (Estevadeordal, Frantz, and Taylor 2003; Obstfeld and Taylor 2004). Another answer to this problem is to address issues of simultaneity more formally using econometric techniques, a strategy we shall adopt here.

Our empirical strategy poses the question: did the hegemons send larger capital flows to the countries with whom they traded more, all else equal? All else equal will mean that we include controls for level of development, colonial status, currency regime, defaults, and institutions. Various studies have found these factors to be important determinants of trade and financial flows in these (and other) eras, although a simultaneous model of both trade and finance has not previously been attempted in the literature.³ Our conclusion is that trade and

² However, as described by Obstfeld and Taylor (2004), the postwar liberalization of trade has, in general, proceeded much more quickly and smoothly than the liberalization of financial markets, as the postwar trends in Figure 1 suggest.

³ The closest relative to our paper is the study by Clemens and Williamson (2004). Indeed, they allowed us to use their pre-1914 data on British capital exports, and their help is gratefully acknowledged. Our interwar data comprise a newly-coded source that has not been previously

finance *did* tend to move together, at least in the case of major capital exporters from the 19th century up to World War Two.

This is an important finding because traditionally theories of trade and finance have been treated as completely separable; indeed, the two subjects are almost always taught in a disjoint fashion. Only recently have researchers empirically probed the trade-finance nexus and the early findings are suggestive of a significant linkage between trade flows and financial flows. The few papers that have spoken to this question, directly or indirectly, have used contemporary data: examples include Portes and Rey (2003); Koren (2003); Lane and Milesi-Ferretti (2003); Aizenman (2003); Aizenman and Noy (2004); and Rose and Spiegel (2004).

The finding that trade and finance do follow each other over such long periods and in such different samples of data, does suggest that this is a stylized fact and one worthy not just of further empirical scrutiny, but also in need of theoretical explanation. Our results provide the first test of the trade-finance linkage in an historical setting. Moreover, whilst this additional historical evidence then provides a higher motivation for the development of a valid theoretical explanation, the new evidence from different historical periods may also supply enough additional contextual information (e.g. on political arrangements or transaction costs) to help us parse the set of candidate theories and whittle them down to a smaller set of viable theories, a challenge we take up in what follows.

used. Clemens and Williamson (2004) do not formally include trade in their determinants of capital exports from Britain, although they include distance (echoing the gravity-type approach, like Portes and Rey 2002). They also test the explanatory power of certain real fundamentals (proxies for human capital, resources and labor). We do not have as large a set of regressors for our interwar U.S. sample as they have for the late 19th century, so we do not follow their specification here to preserve comparability across samples and to keep the focus on our trade-finance hypothesis.

Empirical Strategy

This section describes and motivates the key hypothesis we wish to test. We address the idea that countries with larger trade flows will also tend to have larger financial flows, all else equal. We consider this hypothesis to embody the idea of a trade-finance nexus.

On the face of it, this is a hypothesis that may seem like common sense, at least based on past experience. For example, the idea underlies a great deal of our historical narratives concerning the development of international economic relations in the early modern period. The expansion of merchant empires by the Dutch and British from 17th to 19th centuries saw the simultaneous growth of trade and financial linkages between these financial centers and their respective peripheries. Indeed, the historical term “merchant banker” hints at a direct connection between the two activities as they were pursued by a small set of private entrepreneurs. In this way a nexus could develop between trade and financial activities, starting even in the very thin markets of the pre-modern era—as exemplified, say, in Shakespeare’s *Merchant of Venice*.

Nonetheless, some of the benchmark models of international economics predict the exact opposite correlation. The classic work of Mundell (1957) laid out the fundamental proposition that, under standard assumptions, trade and factor flows (including capital flows) should be substitutes, not complements. The simplest version of this argument can be seen in the 2x2 Heckscher-Ohlin model. There, opening up to trade leads to factor price equalization, negating any incentive for factors to move; conversely, factor migration can bring autarky factor prices (and hence goods prices) into equalization, negating any motive for trade. Subsequently, theorists developed more complex models where the correlation can go the other way (e.g. Markusen 1983; Wong 1986). Historical evidence on this proposition has thus far been confined to the relationship between trade flows and labor flows. Collins, O’Rourke, and Williamson

(1999) find that the data strongly come down in favor of complementarity. Ours is the first study to examine trade and financial flows, but our conclusion leads in the same direction.

By way of a formal model, we may note that several different theories lead us to expect a strong positive correlation between trade and finance in the data. A list of possible hypotheses include the following:

1. *Finance as an input in trade.* Larger capital flows between countries result from a strong bilateral financial infrastructure leading to lower transaction costs or reduced risks; but financial development of this sort also helps trade flourish due to cheaper trade credit.⁴
2. *Information spillovers.* Trade linkages via merchant activity enhance the information flow between two economies, but this in turn also stimulates larger financial flows as information frictions are overcome; conversely, the reverse causation is imaginable: increased financial interaction may lead to greater information exchange, which might encourage more trade.⁵ Information may also spill over between the public and the private sector.
3. *Sovereign default and trade punishment.* Gains from trade serve as punishment devices in case of default; if the (possibly repeated) game between creditors and debtors is such that punishments provide the only reason for credit to be extended,

⁴ On the institutional basis for financial development and the expansion of capital markets see, for example, Neal (1990; 2000), Rousseau and Sylla (2003), and Ferguson (2003). The importance of cheap trade credit is seen clearly once the significant time costs of trading goods is understood; see Hummels (2001).

⁵ On information flows and trade see Rauch (1999) and Rauch and Trindade (2003). On information at capital flows see Portes, Rey, and Oh (2001); Portes and Rey (2003).

- then larger trade volumes can support larger financial flows in equilibrium, all else equal.⁶
4. *Reputational spillovers.* Sustained low-risk, honest trading in contracts for goods (instantaneously or with a settlement horizon of near zero length) might develop reputations that, in turn, enhance the sustainability of longer-horizon trades (that is, trade in financial assets). In other words, a financial reputation for long-term borrowing may be zero for a new borrower, but it could be developed through good behavior by the borrower in repaying short-term contracts.⁷ The reverse spillover from finance to trade is conceivable too, in principle.
 5. *Risk sharing and production diversification.* Financial development and integration between countries enables improvements in risk sharing that encourage more specialized production patterns according to comparative advantage, which, in turn, lead to a greater volume of trade.⁸
 6. *Foreign direct investment.* FDI may be motivated by a desire to establish an “export platform” or “vertical” FDI may be driven by differences in factor prices. Here more FDI can go hand in hand with more trade.⁹ Conversely, in a world of high transport

⁶ This is the argument of Rose and Spiegel (2004). See the discussion by Wright (2004). Mitchener and Weidenmier (2004) test this hypothesis for the period 1870–1914, and find only weak evidence.

⁷ Tomz (2001) and Mitchener and Weidenmier (2004) see strong evidence for reputational effects on foreign lending in the late nineteenth century. Theory has grappled with this idea: see Bulow and Rogoff (1989); Greif (1993); and Kletzer and Wright (2000).

⁸ See, for example, Koren (2003).

⁹ On exports and FDI see Helpman, Melitz, and Yeaple (2004). A general discussion of FDI and trade is given by Feenstra (2004, chapter 11). Evidence on the FDI-trade linkage and its sign is mixed: see Katseli (1993); Goldberg and Klein (2001); Blonigen (2001); Swenson (2003).

costs (or high tariffs), more “horizontal” FDI, in the form of local plants to serve the local market, could be predicted, a case where FDI substitutes for trade.

One or more (or all) of these explanations might be true. There may be several other plausible explanations, but in some respects this only goes to show how far our theory is ahead of our empirics, especially quantitative historical analysis.

Based on any of these theories, and given the availability of data on bilateral trade and financial flows, we might postulate a testable relationship in the data of the following form which could be applied to capital outflows from a source country to multiple destination countries (indexed by i) over several time periods (indexed by t):

$$\ln(\text{Financial Flow/GDP})_{it} = a + b \ln(\text{Trade Flow/GDP})_{it} + c X_{it} + u_{it}, \quad (1)$$

where the financial and trade flows are appropriately normalized by the size of the partner country, X is a vector of other variables that might influence financial flows between the pair, and u is an error term, which is assumed to be well behaved.

Our unit of observation will be the receiving region (of British or U.S. capital exports) in a given period, using period-average data. Panel data will be employed consisting of multiple receiving regions across different time periods. Our dependent variable will be chosen to measure bilateral financial inflows (gross flows in the form of new issues, whether total, private, or public) relative to the receiving region’s GDP. By analogy, our trade variable will also measure trade (export plus imports) relative to the receiving region’s GDP. Our control variables X will include income per capita, currency regime, colonial status, default status, and institutional quality (of the receiving region). These variables are typically thought to have strong

impacts on either trade flows or financial flows or both.¹⁰ In some specifications we also include some of the controls used by Clemens and Williamson (2003)¹¹ in order to test the robustness of the trade-finance linkage.

We propose to estimate (1) initially by ordinary least squares (OLS) but, recognizing the possibility of simultaneity between flows of trade and finance, we will subsequently apply an instrumental variables (IV) approach that may be more robust. It is quite possible that financial links promote trade as well as vice versa, for any of the reasons outlined above. Our strategy here will be to use the tried and trusted method of employing distance as an instrument for trade, following the insights of the gravity model of trade.¹²

Data

We examine two eras and two countries. From 1870 to 1913 we examine trade and capital outflows from the U.K. (the “prewar” era); between 1920 and 1930 we study trade and capital outflows from the U.S. (the “interwar” era). Summary statistics for the entire dataset are shown in Table 1. Details of the data are given in the Appendix.

The data are broken down into eight periods which make up the panel. Six of the time periods are in the prewar era. The core of the prewar dataset was generously provided by

¹⁰ On trade, see Estevadeordal, Frantz, and Taylor (2003); Flandreau and Maurel (2001); and Meissner and Lopéz Córdova (2003). See also the discussion of commodity price convergence in Jacks (2004). On financial flows see Clemens and Williamson (2003); Obstfeld and Taylor (2004). See also the discussion of country risk and the gold standard in Bordo and Rockoff (1996).

¹¹ The data from Clemens and Williamson (2003) can be found at

<http://www.res.org.uk/economic/datasets/readmefile/april04/vol114iss495.asp>

¹² This strategy was followed by Rose and Spiegel (2003). It would be an invalid identification if distance directly affected trade (Portes and Rey 2002). There are reasons, however, to believe a priori that “weightless” financial flows would be insensitive (or only weakly sensitive) to distance, in contrast to traded commodities.

Clemens and Williamson (2003). The dataset includes gross capital flows from Britain to 34 destination countries based on the work of Irving Stone.¹³ The gross capital flows are new issues in London, and they can also be disaggregated in two subcategories. Public gross capital flows are flows from Britain to the public sector of a destination country and private gross capital flows are flows to the private sector of a destination country. Our dataset also includes bilateral trade between Britain and each destination country based on López-Córdova and Meissner (2003). In our analysis, a normalization is applied as follows: both the capital flows and trade volume are divided by the GDP of the country receiving the British capital inflow, to correct for differences in the scale of the receiving country. The nautical distance between London and each country is used to instrument for trade in the instrumental variables regressions in the analysis and is also taken from Clemens and Williamson (2003), as are additional control variables as reported below.

In the two periods of our “interwar” era, 1920–1930, we trace the flow of capital out of the United States to various destination countries. We compile a new dataset to measure the flow of capital out of the United States through bond issues to 51 destination countries as tabulated by Lewis (1948).¹⁴ Once again, this capital flow is a gross flow although it does not include equity or FDI (unlike the prewar flow). The interwar flow is then also split into two variables,

¹³ Argentina, Australia, Austria-Hungary, Brazil, Burma, Canada, Ceylon, Chile, China, Colombia, Cuba, Denmark, Egypt, France, Germany, Greece, India, Indonesia, Italy, Japan, Mexico, New Zealand, Norway, Peru, Philippines, Portugal, Russia, Serbia, Spain, Sweden, Thailand, Turkey, United States and Uruguay.

¹⁴ Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Bulgaria, Canada and Newfoundland, Chile, China, Colombia, Costa Rica, Cuba, Czechoslovakia, Danzig, Denmark, Dominican Republic, Dutch East Indies, Estonia, Finland, France, Germany, Great Britain, Greece, Guatemala, Haiti, Honduras, Hungary, Ireland, Italy, Japan, Liberia, Lithuania, Luxemburg, Mexico, Netherlands Nicaragua, Norway, Palestine, Panama, Peru, Philippine Islands, Poland, Romania, Russia, Salvador, Spain, Sweden, Switzerland, Uruguay, Venezuela, and Yugoslavia.

government bonds and corporate bonds, to disaggregate the flows to public and private borrowers from the United States capital market. Bilateral trade between the United States and the countries receiving the capital is based on 1928 trade data from Irwin and Terviö (2000). In the interwar era, we again normalize the capital flow and trade flows by destination country GDP. Great circle distance between Washington D.C. and the destination country capital cities is used as the relevant bilateral distance measure.

In both epochs, GDP per capita of the destination country was taken from the standard source, namely Maddison (1995, 2003). Also, dummy variables for whether or not a destination country is on the gold standard or is considered a colony of British Empire are constructed using data from Clemens and Williamson (2003) and Estevadeordal et al. (2003), based on Meissner (2002) and other sources.

Results

OLS Estimates

The OLS estimates of (1) using the prewar and interwar datasets can be found in Table 2. The results are separated into total capital flows, capital flows to the public sector of a destination country, and capital flows to the private sector of a destination country. These results assume that trade is exogenous and, hence, a valid regressor. This assumption will be dropped shortly.

The analysis of the prewar era confirms that increased trade volumes are associated with increased financial flows from Britain to her partner countries. Construed as an exogenous variable, trade has a positive and highly significant effect on capital flows out of Britain. A 1% increase in trade flows (as a share of GDP) in the partner country would encourage 0.8% more in gross foreign investment flows (as a share of GDP) out of Britain to that same country. Trade

seems to have had marginally more influence on public investment than investment in the private sector. A 1% increase in trade would result in a 0.96% increase in flows to a country's public sector and a 0.74% increase in flows to the country's private sector, but the differences do not appear to be highly statistically significant.

In the interwar period, Americans were also investing more in their trading partners. Trade seems to have had more influence on public investment than investment in the private sector. A 1% increase in trade flows (as a share of GDP) in the partner country would encourage 0.54% more in gross foreign investment flows (as a share of GDP) out of U.S. to that same country. Again, the effect on flows to the public sector appears stronger and this time the differences are quite dramatic. A 1% increase in trade with the U.S. would result in a 0.75% increase in U.S. investment into a country's public sector but a statistically insignificant change in flows to the country's private sector.

Robustness Check: Other Controls

The estimates in Table 2 are based on a particularly simple specification. Is this the true model of capital outflows? Other models have been estimated for the prewar period (but few for the interwar period). The most recent and exhaustive empirical examination was by Clemens and Williamson (2003). They did not include the trade-finance nexus in their estimation, but they did run regression that included a wide range of variables intended to capture "economic fundamentals" that might attract capital to recipient countries. The explanatory variables they found to be statistically significant, and robustly so, were proxies for endowments of other factors that might be complementary to capital flows in to the recipient country: labor force growth (proxied by population growth rates and immigration rates), human capital accumulation (proxied by schooling measures), and natural resources (proxied by the resource intensity of

exports). They interpreted the importance of these variables in the regression analysis as offering support for an “efficient markets” view of the 1870–1913 global capital market centered on London.

Could it be that these economic fundamentals mattered more than the trade-finance nexus in explaining prewar capital flows? To check the sensitivity of our coefficient on trade, we added the four key additional independent variables which Clemens and Williamson (2003) found to have a significant influence on investment flows.¹⁵ Using this specification we can check the robustness of our model.¹⁶ Table 3 shows that the positive sign and statistical significance on the trade coefficient are robust in the OLS specification in the prewar period, even when the additional controls are added. The trade-finance linkage is still evident.

The trade-finance link offers a complementary—and not necessarily rival—explanation for these capital flows as compared to the Clemens-Williamson model. The Clemens-Williamson variables do have a significant effect on private investment in the prewar period, while not affecting public investment. Conversely, colony appears to strongly affect public investment flows, but not the private flows. This is a reasonable result: surely empire membership mattered more for public works funding from London driven by administrative pressures, whereas economic fundamentals should have mattered more for private sector decisions more subject to market pressures.

It is harder to extend this robustness check to U.S. investment in the interwar period, because the Clemens-Williamson data do not extend past 1913 and do not encompass all of the

¹⁵ The four variables include schooling, the important primary products as a total of exports, the rate of immigration, and the growth rate of population.

¹⁶ The values of the four variables in the final period of the prewar period are used to test the model in the interwar period.

countries in the Lewis dataset. Nonetheless, we applied the same robustness check to the U.S. interwar dataset using the lagged 1913 values from the Clemens-Williamson data as regressors for all the available countries. In these results, shown on the right hand side of Table 3, the fit is very poor and the additional controls do not appear significant. Despite the likely misspecification and noise, however, the trade-finance linkage is still present, at least for total capital flows, although not for the private and public components of the disaggregated flows. The four additional controls do not affect private investment but the primary products variable is significant for public investment.¹⁷

Robustness Check: IV estimation

We repeated our benchmark analysis of Table 2 using instrumental variable (IV) methods to correct for the potential endogeneity of trade, that is, the reverse causation from finance to trade. The IV estimator is our preferred specification.

The results of IV estimation where we use distance as an instrument for trade, found in Table 4, tell a similar trade story. Under this, more robust, specification we continue to find that trade encourages investment out of Britain in the prewar period and out of the U.S. in the interwar period. Trade relationships have a positive and significant effect on both private and public investment in the prewar period though the effect on the private sector investment is now a little larger than the effect on public sector investment. These differences remain statistically insignificant.

However, the overall effects of trade on both public and private investment in the prewar period are larger when we instrument for trade than when we use the OLS specification. In the

¹⁷ We note that the fact that a country is on the gold standard influences private investment in the prewar period in this specification, whereas the coefficient on British Colony was important in our original specification. The correlation of these two variables lies behind this result.

IV regressions in Table 4, a 1% increase in trade as a portion of the trading partner's GDP here leads to a 1.2% increase in Britain's investment in the public sector and a 1.6% increase in Britain's investment in the private sector of the trading partner. This is significantly larger than the increases of 1% and 0.7% found for the public and private sectors respectively in the OLS model of Table 2. When we use the IV specification in the interwar period, trade appears only to encourage more public sector investment. Again, the magnitude of the effect of trade on public sector investment is larger than with the OLS specification. A 1% increase in trade with the U.S. as a proportion of GDP would bring in 1.2% more investment into the public sector from the U.S.

We conclude from these results that the trade-finance linkage in our sample is robust to any endogeneity arising from reverse causality from finance back to trade.

Robustness Check: Default and Institutions

The results in Table 5 include default status as an additional control variable in the IV regression. The default variable is taken from Tomz (2001). Default is a dummy variable and is treated as exogenous, and the working hypothesis is that a country in default will receive less capital inflow, all else equal. In all specifications the sign on the default variable is negative, but only in column 6 (interwar, private flows) is the effect statistically significant. However, these results do not overturn the argument that trade and capital flows are complementary. The coefficient of trade remains significant in all regressions in Table 5.

Still, these results could be consistent with the view that the impact of default of capital flows is felt through the trade channel. Compared to Table 4, colony effects look very similar, as do income per capita effects. Moreover, the trade variable (instrumented by distance) is now statistically significant in *all six* regressions. Why? In Table 3 trade was not significant in the

final column (interwar, private); here it is, and this is the one sample for which the default variable is negative and highly significant.

Table 5 also adds an “institutional quality” proxy as an additional control variable in the IV regression. The variable is XCONST (constraints on the executive, min 0, max 7) from the Polity IV dataset, and is included as a proxy for expropriation risk and property rights. It is not an ideal measure, but no other property rights data are available in this era, and absent such direct measures, researchers have been led to use the Polity type measures circa 1900 to capture institutional quality (e.g., Acemoglu et al. 2001). Our key results are robust to this specification change also: trade (instrumented by distance) remains a significant determinant of capital flows, and the coefficients are not dissimilar to those seen in Tables 4 and 5. The institutional quality variable is not found to be a significant determinant of capital flows in these regressions. (If the institutions variable is omitted, the results with the default variable only are similar.)

Obviously, this need not imply that institutional quality was irrelevant to capital flows, since the impacts might have been indirect. It has been argued that countries in the British Empire inherited better institutions, whether via the legal system or other channels (Ferguson 2003; Glaeser and Shleifer 2002). Institutions and Empire membership were also correlated with high trade flows, which also encouraged more capital flows. We also note that Table 6 contains controls for log income per capita and its square. In the interwar period, these terms are statistically significant for private and total investment. No doubt, these terms are capturing some impact of institutional quality too.

However, for our purposes, we are mainly concerned with the trade-finance nexus and the message from Table 5 is that our key results are not sensitive to the inclusion of the default and institutions variables, and, indeed, are strengthened.

Model Testing: Trade-Empire Interaction and the Punishment Theory

Finally, Table 6 extends Table 5, dropping the insignificant institutions variable, and adding a variable that interacts colonial status with trade for the prewar sample. The coefficient on this variable tells us whether the impact of trade on financial flows is stronger or weaker in colonies versus independent countries. Our interest in this interaction lies in its ability to help us test some of the theories proposed to explain the trade-finance nexus—theories that may function differently for colonial and noncolonial country pairs.

Specifically, if theories based on punishment are correct, then trade and finance ought to be strongly correlated in independent countries, but the same should not be true in colonies. This follows for the simple reason that in colonies there is no sovereignty, and hence no need of a trade-related punishment device: imperial power alone should suffice to guarantee debt service and repayment. (It is clear why we could not extend this analysis to the interwar period: the U.S. had no colonies in our data set.)

With respect to this test of the punishment hypothesis, Table 6 offers no support. The interaction terms are not significantly different from zero—meaning that the impact of trade on finance is no weaker in colonies than in non-colonies. This is true in all six columns of Table 6, for both OLS and IV estimation, and for total, private, and public flows. (In columns 4 and 6 the coefficient is qualitatively large—but of the “wrong” sign.) We conclude that trade and finance were equally strongly correlated in colonies and noncolonies, and argue that this casts doubt on the punishment story as applied to this era.

Discussion

Our analysis shows that there was a link between trade flows and financial flows in the prewar and interwar periods for the hegemons. The nature of that link can also be seen to have changed between the two eras. Several observations can be made based on our preferred results in Table 5, some of which throw light on the question of which among the candidate theories might best explain these historical patterns.

We first address the fact that interwar U.S. investment was not as strongly lured by trade to the private sector as was prewar British investment (the coefficient on trade in column 6 is one third smaller than that in column 3). Why was this so? It seems to be generally accepted that trade was more insecure and subject to higher transaction costs in the interwar period than in the prewar period (Kindleberger 1984) This was due to various factors including the persistent reduction of trade following a war (Glick and Taylor 2004), the collapse of the gold standard, higher trade barriers, and increased transaction costs (Estevadeordal et al. 2003). It is possible that these frictions were so high that the channels creating a private-sector nexus between trade and finance were weakened. Many decades of world leadership in finance had given the prewar British data enough time to show such cumulative forces; but the interwar data, representing the earliest years of U.S. hegemony might not yet have allowed such a cumulation to appear. This weakening of the linkage in the emerging years of a new financial hegemon offers some support for theories which build in cumulative causation, e.g., information or reputational spillovers.

We next address the emergence of strong income effects in the interwar period. These income effects for private and total investment (Table 5, columns 4 and 6) suggest that the new hegemon was operating in quite a different environment. If reputation was weak or short-lived, and information hard to get in the interwar period, then investors in the U.S. would probably

look for an alternative signal of a “good debtor.” That is, income per capita might have been used as a signal that a country was a reliable place to do business, a factor that seems to matter in addition to the powerful trade-finance nexus we have identified.

Figure 2 illustrates the income effect for interwar U.S. private investment (using Table 5, column 6 estimates) by graphing the fitted value of the regression against $\ln(\text{GDP per capita})$ with all other right-hand side variables held constant (at zero). As a result of the quadratic term, the income effect has an inverted U-shape. This pattern might be interpreted as a mix of a conventional institutional proxy effect (being rich signals good institutions, all else equal) and a conventional neoclassical effect (being rich means you are less capital scarce). As Figure 2 shows, these effects work strongly at opposite ends of the income spectrum to choke off capital flows in the richest and poorest countries. Richer countries had good institutions but had less need of foreign capital, discouraging flows; poorer countries had high demand, but capital was repelled by poor institutions; between these extremes only “emerging markets” in the middle survived these two filters and received large private flows of U.S. capital.

We now address the strong colony effect in both the British prewar data (Table 5, columns 1–3) and sometimes for the U.S. interwar data (column 6). In the prewar period, the results show that a country would tend to get more British investment if it was a British Colony.¹⁸ In the prewar period we find in Table 5 that a typical colony of Britain would experience British investment (as a share of GDP) about 100% ($e^{0.7}-1=1.01$) higher than other similar countries. However, this impact was statistically significant for public investment, as expected; but it was not statistically significant for private sector investment, suggesting that

¹⁸ These results are consistent with the findings of Clemens and Williamson (2003). They also find a positive and significant sign on the colony dummy coefficient when they run their regressions using all of the six prewar periods.

market based decisions were not as strongly influenced by colonial linkages. This makes sense. Britain had a direct role in the government and economic policies of her colonies. Public investment would be encouraged since Britain, as the governing power, would be providing all infrastructure in the colonies and issuing the necessary bond finance, mostly at home in London. Curiously, we also find in Table 5, column 6, that the British Colony dummy was significant in the interwar period too, in the case of the U.S. investment in the private sector. The U.S. invested more in the private sectors of countries that were colonies of Britain. This impact of British colonial status on U.S. interwar private investment also seems understandable, on reflection. After the destruction of World War I, Britain's resources and financing capabilities were limited. Britain continued to support the public sectors of her colonies with financing. However, the private sectors of the colonies were subject to rather more of the same capital scarcity shock that affected the world as a whole. The private sectors of the British colonies, rather more than the public sectors, would then turn their demand for investment funds to other sources, notably the U.S. Yet the British colonies still enjoyed the benefits of high quality institutions—rule of law, property rights, all underwritten by the British Empire (Ferguson 2004). Hence U.S. investors might have had more confidence in private firms in these countries (compared to non-British Empire countries) because of the confidence in their imperially-derived institutional stability. Recent literature in international trade on foreign direct investment in the post-WWII era has found positive links between high quality institutions and foreign investment.¹⁹ Thus, like income per capita, colonial status offered U.S. investors another clear sign of safety.

We finally address the fact that the gold standard dummy is insignificant in all regressions in Table 5, except for column 6. On the face of it this suggests that gold was not the

¹⁹ See Fischer and Sahay (2000).

prewar “Good Housekeeping Seal of Approval,” that Bordo and Rockoff (1996) asserted. In our study, a trading relationship and colony status were more encouraging to investors than the monetary regime of the potential investment destination country. A possible reason why our findings are not consistent with those of Clemens and Williamson (2003), who found the coefficient on their gold standard dummy positive and significant, is that we have introduced trade into our model. Several researchers have shown that joining the Gold Standard promoted trade. In this case, the Gold Standard effect may be captured in our trade variable. (Estevadeordal et al. 2003, López-Córdova and Meissner 2003, and Flandreau and Maurel 2001). Another possible explanation is that many of the countries participating in the gold standard were colonies of Britain. By including both the colony dummy and the gold standard dummy in the same regressions, we could be absorbing some of the effect of the gold standard into the coefficient on British colony. It is notoriously difficult to separate the impact of the gold standard and British colonial status in this period simply because every British colony was on gold. However, it is yet again interesting to see that U.S. interwar private investment (Table 5, column 6) did see the gold standard as a positive signal for investment. The multiple signals that are statistically significant in column 6 (gold, colonial status, default status, income effects) tell an important story. In the 1920s, at a time when the U.S. was emerging as the new banker to the world, in a world economy beset by instability and uncertainty, potential private sector investors were sensitive to many more indicators of creditworthiness than their British predecessors had been before 1914.

Conclusion

Our overall finding is that the trade-finance nexus was alive and well in the 1870–1913 and 1920–29 periods. It affected both public and private borrowing in London before WWI and in New York in the interwar period. The more robust, instrumental variables specifications with additional controls for default and institutions (Table 5) confirm that increased trade led to increased investment from the two hegemons: the bankers to the world, Britain before WWI and the U.S. after WWI.

We take this to show that the trade-finance linkage, demonstrated numerous times in contemporary data, is also supported by historical patterns from the more distant past. But our results, viewed comparatively and in their historical context, also offer insights as to how we might parse the various theories proposed to explain the empirical relationship between trade and finance.

The theories regarding information or reputational spillovers from trade to investment may draw support from the differences in our analysis between the two hegemons. Spillovers were probably stronger in the prewar period given that the traders and borrowers dealing with Britain had time to develop relationships and reputations, whereas the link was probably weaker for the U.S. in the interwar period, since the U.S. was the newly established hegemon of the interwar period with less of a track record as a leader in international trade and finance. Consistent with this logic, we do find that the trade-finance coefficient was smaller in the interwar period for private investment.

However, our analysis of the two epochs appears inconsistent with the theory that trade could lead to more investment due to the risk of trade revocation as punishment for sovereign default. If this were true, it ought to work as a powerful driver of trade with independent

countries, but operate only weakly (if at all) between the mother country and her colonies. Our results show that this was not the case in prewar Britain. The trade-finance nexus was equally strong for colonies and noncolonies alike. We conclude that in the trade-finance nexus it is probably the spillover effects that were much more powerful than the punishment effect, and that these explained most of the volume of financial flows. This would be consistent with the arguments, based on quite different evidence, presented by Mitchener and Weidenmier (2004).

Further research is certainly warranted and other trade-finance channels (such as FDI) remain to be explored in these historical epochs. But this first attack on the question shows that the trade-finance linkage is not merely a property of contemporary data, but a feature evident in historical data as well—a quite universal pattern. Hence, explanations of this linkage might focus on theories that apply across all epochs, and should employ exogenous variation in politics, empires, institutions, transport costs, and other variables to better sift the competing theories. In this vein, our paper shows how the contrasting experiences of the two hegemons, Britain and the United States, can illuminate the problem.

Data Appendix

Period Descriptions

Period 1: 1870-1877; Period 2: 1878-1885; Period 3: 1886-1893; Period 4: 1894-1901; Period 5: 1902-1906; Period 6: 1907-1913; Period 7: 1920-1924; Period 8: 1925-1930.

GDP

Prewar: Annual average in 1990 U.S. Dollars. The source of the data is Clemens and Williamson (2003) and can be found at:

<http://www.res.org.uk/economic/datasets/readmefile/april04/vol114iss495.asp>.

Interwar: GDP in 1990 U.S. dollars in 1913. From Maddison (2001).

ln(total/GDP)

Prewar: The gross flow of capital as a fraction of GDP, annual average. Capital flows computed from Stone's data on capital issues in London. From Clemens and Williamson (2003). The destination countries include Argentina, Australia, Austria-Hungary, Brazil, Burma, Canada, Ceylon, Chile, China, Colombia, Cuba, Denmark, Egypt, France, Germany, Greece, India, Indonesia, Italy, Japan, Mexico, New Zealand, Norway, Peru, Philippines, Portugal, Russia, Serbia, Spain, Sweden, Thailand, Turkey, United States, and Uruguay.

Interwar: The gross flow of bond capital as a fraction of GDP, annual average. Capital flows computed from Lewis's data on bond issues in New York. From Lewis (1948). The destination countries include Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Bulgaria, Canada and Newfoundland, Chile, China, Colombia, Costa Rica, Cuba, Czechoslovakia, Danzig, Denmark, Dominican Republic, Dutch East Indies, Estonia, Finland, France, Germany, Great Britain, Greece, Guatemala, Haiti, Honduras, Hungary, Ireland, Italy, Japan, Liberia, Lithuania, Luxemburg, Mexico, Netherlands Nicaragua, Norway, Palestine, Panama, Peru, Philippine Islands, Poland, Romania, Russia, Salvador, Spain, Sweden, Switzerland, Uruguay, Venezuela, Yugoslavia.

ln(public/GDP)

Same as ln(total/GDP) except that only issues by governments are included (municipal, state, or national).

ln(private/GDP)

Same as ln(total/GDP) except that only issues by the private sector are included.

ln(trade/GDP)

Prewar: Bilateral trade flows in millions of 1990 U.S. dollars. From López-Córdova and Meissner (2003). Because this source gives values for bilateral trade only every 5 years in period 1, we use bilateral trade in 1870, in period 2, we use bilateral trade in 1880, in period 3, we use bilateral trade in 1890, in period 4, we use bilateral trade in 1900, in period 5, we use bilateral trade in 1905 and in period 6, we use bilateral trade in 1910.

Interwar: Bilateral trade flows in 1928 in millions of gold dollars, expressed as the sum of exports and imports with another country. From Irwin and Terviö (2000).

ln(GDP/pop)

Prewar: Log of per capita. From Clemens and Williamson (2003).

Interwar: Log of per capita GDP in 1913, in 1990 US dollars. From Maddison (2001).

Gold

Prewar: A dummy variable taking on the value of 1 if the destination country was on the gold standard for most of the period and 0 if it was not. From Clemens and Williamson (2003) and based on Meissner (2002).

Interwar: A dummy variable taking on the value of 1 if the destination country was on the gold standard for most of the period and 0 if it was not. From Estevadeordal et al. (2003) and based on Meissner (2002) and other sources.

Colony

Prewar: This variable takes on the value of 1 if the destination country was a colony of the British Empire for the majority of the period and 0 if it was not. From Clemens and Williamson (2003).

Interwar: This variable takes on the value of 1 if the destination country was a colony of the British Empire in 1913. From Clemens and Williamson (2003).

ln(distance)

Prewar: Distance from London in thousands of nautical miles. From Clemens and Williamson (2003)

Interwar: The great circle distance between Washington D.C. and the capital city of the destination country. The measure was also taken from Rose (2000), downloaded from <http://www.haas.berkeley.edu/arose>. Countries that were not in his dataset were approximated using the nearest capital city to the destination country.

Some distances between countries were unavailable and were estimated using the distance to their nearest neighbor as follows: Finland for Latvia, Estonia and Lithuania, France for Luxemburg, Yugoslavia for Serbia and Israel for Palestine.

ln(school)

Schooling is the fraction of the population that is aged 14 years or less enrolled in primary school in the specified year, lagged by 15 years. The value of the regressor in the final period of the prewar period is used for the two periods of the interwar era. From Clemens and Williamson (2003).

ln(ppexport)

Primary product exports is the fraction of total exports that are primary products. The value of the regressor in the final period of the prewar period is the regressor used for the two periods of the interwar era. From Clemens and Williamson (2003).

Migration

Migration was constructed as an annual index (lagged by ten years) taking an integer value between -3 and $+3$. The value $+3$ signifies large net immigration during that year and -3 signifies large net emigration. Index numbers are constructed for each country in each of five periods of the prewar period. The value of the regressor in the final period of the prewar period is the regressor used for the two periods of the interwar era. From Clemens and Williamson (2003).

Population Growth Rate

Population growth rate is the percent change in population during the first year of the relevant period. The value of the regressor in the final period of the prewar period is the regressor used for the two periods of the interwar era. From Clemens and Williamson (2003).

Default

Dummy variable equal to one when a country is in a state of full or partial default. Based on unpublished data from Tomz (2001) kindly provided by Michael Tomz.

Institutional Quality

This variable is XCONST from the Polity IV Dataset and measures “The extent of institutionalized constraints on the decision-making powers of chief executives, whether individuals or collectives.” This variable ranges from 1 to 7 where 1 is unlimited power of the executive and 7 is executive parity or subordination. This dataset can be found at <http://www.cidcm.umd.edu/inscr/polity/index.htm>.

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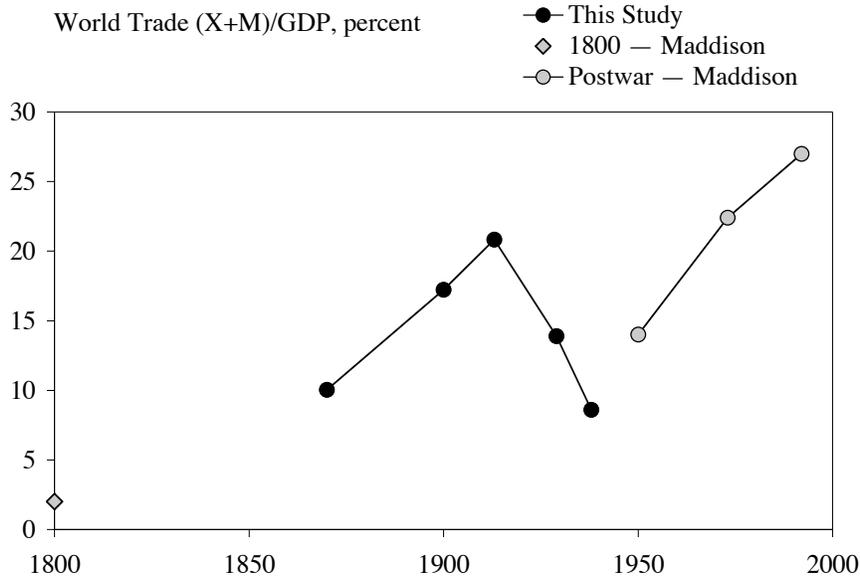
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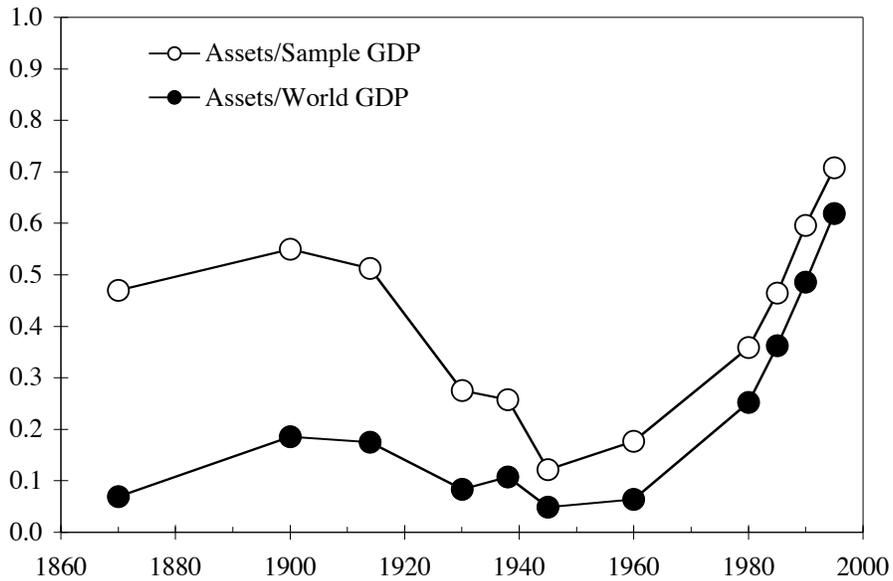
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Figure 1
Globalization of Trade and Finance

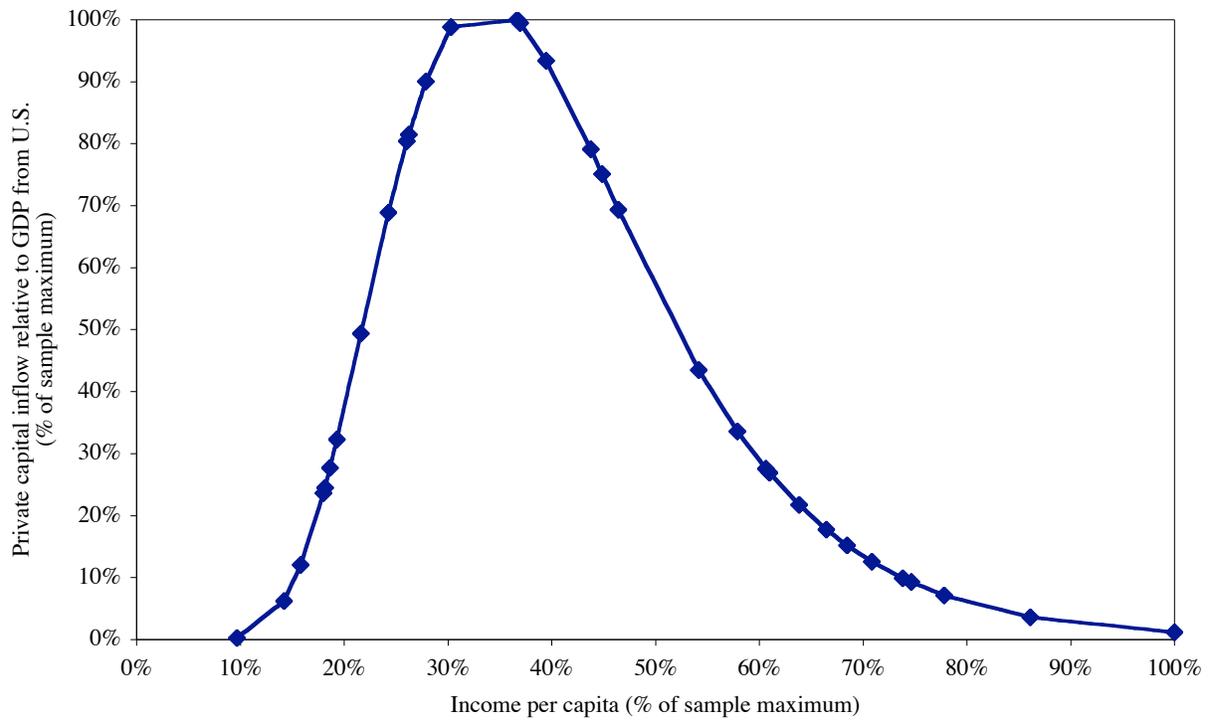


(a) Ratio of world trade to world GDP, 1800–2000
Source: Estevadeordal, Frantz, and Taylor (2003)



(b) Ratio of world foreign capital stocks to world GDP, 1820–2000
Source: Obstfeld and Taylor (2002)

Figure 2
Effect of Per Capita Income of Recipient on Interwar U.S. Private Capital Inflows



Notes: Based on fitted values from Table 5, column 6, where income variables take their actual values and all other variables are held constant (set to zero). Income per capita and predicted capital flows are converted to level (from log) form, and then shown as a fraction of the maximum value in the sample.

Table 1
Summary Statistics

<i>(a) Prewar Data</i>					
	Observations	Mean	Std. dev.	Min	Max
ln(flow/gdp)	191	-9.995	2.005	-16.291	-5.869
ln(public/gdp)	127	-9.188	1.977	-16.601	-5.344
ln(private/gdp)	189	-10.172	1.977	-15.742	-6.142
ln(trade/gdp)	175	-21.570	1.431	-24.577	-18.694
ln(gdp/pop)	204	7.162	0.622	5.603	8.588
ln(gdp/pop) ²	204	51.682	-9.018	31.392	73.754
Gold	204	0.505	0.501	0	1
Colony	204	0.216	0.412	0	1
ln(distance)	204	1.206	1.059	-1.483	2.487
lnschool	204	6.047	1.274	1.386	7.604
lnppexport	204	-0.162	0.273	-1.280	0
Migration	204	0.392	2.127	-3	3
Pop. Growth Rate	204	1.471	1.424	-3.786	12.983
Institutions	160	3.763	2.524	0	7
Default	204	0.142	0.350	0	1
Colony * ln(trade/gdp)	175	-4.003	8.523	-24.413	0
Colony * ln(distance)	204	0.415	0.826	0	2.487
<i>(b) Interwar Data</i>					
	Observations	Mean	Std. dev.	Min	Max
ln(total/gdp)	57	-6.005	1.744	-11.582	-3.469
ln(public/gdp)	50	-6.016	1.800	-13.354	-3.784
ln(private/gdp)	39	-7.602	1.889	-11.582	-4.279
ln(trade/gdp)	64	-19.307	1.283	-22.137	-17.030
ln(gdp/pop)	66	7.680	0.603	6.314	8.651
ln(gdp/pop) ²	66	59.338	9.136	39.863	74.837
Gold	92	0.272	0.447	0	1
Colony	104	0.096	0.296	0	1
ln(distance)	104	8.312	0.500	6.981	9.154
lnschool	50	6.571	0.808	4.405	7.483
lnppexport	50	-0.222	0.329	-1.154	-0.000
Migration	50	0.040	2.312	-3	3
Pop. Growth Rate	50	1.427	0.860	0.183	3.591
Institutions	92	4.598	2.391	0	7
Default	104	0.096	0.296	0	1
Colony * ln(trade/gdp)	64	-1.758	5.525	-20.343	0
Colony * ln(distance)	104	0.818	2.529	0	9.154

Table 2
OLS Estimates

	Prewar Capital Flows from Britain			Interwar Capital Flows from U.S.		
	(1) ln(Total/ gdp)	(2) ln(Public/ gdp)	(3) ln(Private/ gdp)	(4) ln(Total/ gdp)	(5) ln(Public/ gdp)	(6) ln(Private/ gdp)
ln(trade/gdp)	0.808 (9.23)**	0.965 (10.50)**	0.738 (8.08)**	0.544 (2.85)**	0.747 (3.92)**	0.343 (1.36)
ln(gdp/pop)	-6.409 (1.48)	-5.156 (1.22)	-5.501 (1.23)	17.231 (1.28)	13.359 (0.88)	39.461 (2.40)*
ln(gdp/pop) ²	0.472 (1.58)	0.364 (1.24)	0.418 (1.35)	-1.110 (1.25)	-0.842 (0.83)	-2.587 (2.38)*
gold	0.434 (1.54)	-0.324 (1.06)	0.175 (0.60)	-0.746 (1.19)	0.230 (0.34)	-0.399 (0.57)
colony	1.116 (3.17)**	1.020 (2.96)**	1.049 (2.86)**	0.311 (0.30)	-0.952 (0.85)	2.108 (1.71)+
Observations	167	117	165	55	48	39
Overall R-squared	0.44	0.50	0.38	0.17	0.32	0.22

Notes: Absolute t-statistics given in parentheses. We denote significance levels at the 10%, 5%, and 1% with +, *, ** respectively. Fixed period effects are included.

Table 3
OLS Estimates with Additional Controls

	Prewar Capital Flows from Britain			Interwar Capital Flows from U.S.		
	(1) ln(Total/ gdp)	(2) ln(Public/ gdp)	(3) ln(Private/ gdp)	(4) ln(Total/ gdp)	(5) ln(Public/ gdp)	(6) ln(Private/ gdp)
ln(trade/gdp)	0.456 (4.77)**	0.889 (7.91)**	0.397 (4.09)**	0.674 (1.68)	0.397 (0.94)	1.041 (1.65)
ln(gdp/pop)	-10.163 (2.22)*	-6.579 (1.21)	-2.521 (0.53)	-30.582 (1.26)	15.350 (0.55)	9.814 (0.29)
ln(gdp/pop) ²	0.629 (2.04)*	0.432 (1.19)	0.142 (0.45)	2.025 (1.27)	-0.942 (0.52)	-0.674 (0.31)
gold	0.785 (3.13)**	-0.192 (0.58)	0.621 (2.44)*	-1.006 (1.43)	0.516 (0.63)	-1.155 (1.22)
colony	0.300 (0.92)	0.865 (2.35)*	0.220 (0.66)	-2.479 (1.62)	-1.757 (1.09)	1.634 (0.84)
ln(school)	0.822 (4.05)**	0.191 (0.76)	0.396 (1.86)+	0.932 (1.13)	0.092 (0.09)	0.408 (0.33)
ln(ppexport)	1.495 (3.03)**	0.214 (0.34)	1.141 (2.28)*	0.547 (0.50)	2.340 (2.04)+	-1.234 (0.81)
migration	0.269 (4.05)**	0.052 (0.58)	0.349 (5.17)**	-0.151 (0.74)	-0.020 (0.09)	-0.381 (1.31)
pop. growth	0.335 (2.52)*	0.146 (0.96)	0.351 (2.59)*	0.724 (1.66)	0.614 (1.35)	0.232 (0.38)
Observations	167	117	165	41	37	31
Overall R-squared	0.59	0.51	0.57	0.40	0.54	0.22

Notes: Absolute t-statistics given in parentheses. We denote significance levels at the 10%, 5%, and 1% with +, *, ** respectively. Fixed period effects are included.

Table 4
Instrumental Variables Estimates

	Prewar Capital Flows from Britain			Interwar Capital Flows from U.S.		
	(1) ln(Total/ gdp)	(2) ln(Public/ gdp)	(3) ln(Private/ gdp)	(4) ln(Total/ gdp)	(5) ln(Public/ gdp)	(6) ln(Private/ gdp)
ln(trade/gdp)	1.556 (4.69)**	1.216 (3.58)**	1.551 (4.56)**	-0.336 (0.64)	1.249 (2.60)**	-0.146 (0.20)
ln(gdp/pop)	-3.078 (0.57)	-3.557 (0.73)	-1.970 (0.35)	14.436 (0.89)	16.332 (0.98)	43.694 (2.39)*
ln(gdp/pop) ²	0.247 (0.66)	0.249 (0.74)	0.180 (0.46)	-0.928 (0.87)	-1.038 (0.94)	-2.871 (2.37)*
gold	0.040 (0.11)	-0.449 (1.27)	-0.257 (0.64)	-0.666 (0.88)	0.228 (0.31)	-0.357 (0.48)
colony	1.611 (3.39)**	1.218 (2.77)**	1.580 (3.18)**	0.915 (0.70)	-1.252 (1.02)	2.718 (1.75)+
Observations	167	117	165	55	48	39
Overall R-squared	0.41	0.49	0.35	0.01	0.31	0.15

Notes: Absolute Z-statistics given in parentheses. We denote significance levels at the 10%, 5%, and 1% with +, *, ** respectively. Fixed period effects are included and ln(distance) is the instrument for ln(trade/GDP).

Table 5
Instrumental Variables Estimates with Controls for Default and Institutions

	Prewar Capital Flows from Britain			Interwar Capital Flows from U.S.		
	(1) ln(Total/ gdp)	(2) ln(Public/ gdp)	(3) ln(Private/ gdp)	(4) ln(Total/ gdp)	(5) ln(Public/ gdp)	(6) ln(Private/ gdp)
ln(trade/gdp)	1.879 (5.39)**	1.149 (3.38)**	1.924 (5.12)**	0.860 (2.79)**	1.232 (3.16)**	1.205 (2.66)**
ln(gdp/pop)	-10.773 (1.40)	-5.406 (0.88)	-4.256 (0.50)	36.554 (2.96)**	24.101 (1.47)	57.155 (3.39)**
ln(gdp/pop) ²	0.779 (1.48)	0.363 (0.87)	0.376 (0.64)	-2.377 (2.92)**	-1.523 (1.41)	-3.780 (3.41)**
gold	-0.319 (0.72)	-0.356 (1.03)	-0.696 (1.43)	0.036 (0.06)	0.965 (1.09)	1.443 (1.64)
colony	0.599 (1.01)	0.725 (1.70)+	0.766 (1.16)	0.832 (0.94)	-0.899 (0.78)	2.118 (1.74)+
default	-0.941 (1.50)	-0.085 (0.15)	-0.487 (0.71)	-5.546 (4.90)**	—	-4.838 (3.61)**
institutions	-0.041 (0.51)	0.009 (0.12)	-0.109 (1.20)	-0.074 (0.68)	-0.139 (1.05)	0.070 (0.47)
Observations	143	101	141	50	44	35
Overall R-squared	0.43	0.53	0.35	0.52	0.41	0.54

Notes: Absolute Z-statistics given in parentheses. We denote significance levels at the 10%, 5%, and 1% with +, *, ** respectively. Fixed period effects are included and ln(distance) is the instrument for ln(trade/GDP).

Table 6
Testing the Punishment Theory:
Empire and the Trade-Finance Nexus in the Prewar Era

	Prewar Capital Flows from Britain: OLS Estimates			Prewar Capital Flows from Britain: Instrumental Variables Estimates		
	(1) ln(Total/ gdp)	(2) ln(Public/ gdp)	(3) ln(Private/ gdp)	(4) ln(Total/ gdp)	(5) ln(Public/ gdp)	(6) ln(Private/ gdp)
ln(trade/gdp)	0.824 (7.35)**	1.010 (8.38)**	0.690 (5.94)**	1.394 (2.73)**	1.259 (2.38)*	1.188 (2.00)*
ln(trade/gdp)*colony	-0.168 (0.80)	-0.170 (0.79)	-0.011 (0.05)	0.805 (0.74)	-0.197 (0.17)	1.807 (1.39)
ln(gdp/pop)	-4.738 (1.07)	-4.283 (0.95)	-2.984 (0.65)	-3.337 (0.57)	-3.682 (0.78)	-1.068 (0.15)
ln(gdp/pop) ²	0.369 (1.21)	0.313 (1.01)	0.254 (0.80)	0.234 (0.57)	0.266 (0.82)	0.053 (0.11)
gold	0.506 (1.78)+	-0.269 (0.86)	0.250 (0.85)	-0.117 (0.27)	-0.389 (0.88)	-0.592 (1.16)
colony	-2.395 (0.52)	-2.617 (0.55)	1.055 (0.22)	19.165 (0.80)	-3.090 (0.12)	41.121 (1.45)
default	0.611 (1.59)	0.261 (0.57)	0.913 (2.28)*	-0.267 (0.43)	-0.007 (0.01)	-0.089 (0.12)
Observations	167	117	165	167	117	165
Overall R-squared	0.45	0.50	0.40	0.38	0.50	0.29

Notes: Absolute Z-statistics given in parentheses. We denote significance levels at the 10%, 5%, and 1% with +, *, ** respectively. Fixed period effects are included, ln(distance) is the instrument for ln(trade/GDP) and ln(distance)*Colony is the instrument for ln(trade/GDP)*Colony in the instrumental variables results.