IDB WORKING PAPER SERIES No. IDB-WP-512

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July 2014

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Inter-American Development Bank

2014

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Adams-Kane, Jonathon.
Foreign bank behavior during financial crises / Jonathon Adams-Kane, Julián Caballero, Jamus Lim.
p. cm. — (IDB Working Paper Series ; 512)
Includes bibliographic references.
1. Banks and banking, Foreign. 2. Financial crises. 3. Bank loans. I. Caballero, Julián. II. Lim, Jamus Jerome. III. Inter-American Development Bank. Department of Research and Chief Economist. IV. Title. V. Series.
IDB-WP-512

http://www.iadb.org

Abstract[1]

This paper studies whether lending by foreign banks is affected by financial crises. The paper pairs a bank-level dataset of foreign ownership with information on banking crises and examines whether the credit supply of majority foreign-owned banks that underwent home-country crises differs systematically from that of other foreign banks. The baseline results show that banks exposed to home-country crises in 2007 and 2008 exhibit changes in lending patterns that are lower by between 13 and 42 percent than their non-crisis counterparts. This finding is robust to potential alternative explanations and also holds, though less strongly, for the 1997-98 Asian crisis.

JEL classifications: G21, G01, F34
Keywords: Foreign bank ownership, Financial crisis, Bank lending

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http://siteresources.worldbank.org/INTKNOWLEDGEFORCHANGE/Resources/491519-1386966650966/PRWP6590.zip.
The paper has benefited from comments from Samuel Berlinski, Steve Kaplan, Andrew Powell, and Jasmine Xiao. Yueqing Jia and Mauricio Pinzón helped in collecting and processing the data.

1 Introduction

On April 25, 1821, then-Prince Regent Dom João VI set sail from Brazil to Portugal in an attempt to deal with a revolution that was underway in the latter, carrying with him a large part of the deposits of the Banco do Brasil, the colony's major financial institution. The bank, which was already in crisis as a result of its close ties with the Portuguese Crown, was left bankrupt. Dom João's actions two centuries ago are but one reminder that foreign banks' financing may flee when their home countries experience difficult times.

This risk must be weighed against potential liquidity and growth benefits of foreign bank presence. Thus, policymakers in developing countries seeking to liberalize their financial sectors are routinely called on to decide whether foreign banks are to be allowed into their domestic financial markets and, if so, to what extent such banks have the freedom to operate vis-à-vis domestic banks.

This paper contributes to the literature on evaluating foreign bank presence in developing countries by asking whether foreign banks do indeed make different credit provision choices when their home economies are undergoing hard times. In particular, we examine whether the lending behavior of majority foreign-owned financial institutions that experienced a crisis in their home countries differs systematically relative to foreign-owned institutions that did not, within the setting of the global financial crisis of 2007-08. We also probe the generality of our main findings with an extension to the Asian crisis of 1997/98.

Whether foreign-owned banks choose to scale back on their lending activity under such circumstances is far from obvious. A foreign subsidiary experiencing a crisis in its home country may face a contraction in the banking group's internal capital market, or it may need to repatriate capital to an ailing parent bank. But it is just as plausible that the parent bank reallocates its asset portfolio toward markets relatively less affected by the crisis. The issue of how foreign bank lending changes during financial crises is thus, ultimately, an empirical question.

Our empirical exploration seeks to answer this question by relying on a quasi-experimental difference-in-difference (DiD) approach. Our baseline sample draws on a unique bank ownership dataset collected across countries and over time, and comprises 361 foreign-owned banks based in 51 developing countries (and headquartered in one of 66 countries) over the course of the recent 2007-08 global financial crisis, and in the immediate pre and post-crisis years. We define our crisis "treatment" as a banking crisis (Laeven and Valencia, 2013) experienced in the home country of the foreign-owned bank.[2] Crucial for our identification strategy is the fact that, while financial crises experienced in the home economy may have been closely tied to the performance of banks based there, *foreign subsidiaries* of these banks are unlikely to have contributed to the crisis itself.

[2] This treatment is robust to alternative banking crisis definitions, such as those of Reinhart and Rogoff (2009).

From the perspective of these subsidiaries, then, the crisis event was essentially exogenous, just as it was for other foreign banks in the host economy. The crucial difference lies in how the former group may subsequently be subject to constraints resulting from the home-country crisis, which the latter group—not facing similar shocks—would not experience.

We exploit this exogenous variation to identify the effect of a home-country crisis on foreign bank lending in our baseline difference-in-difference specification. We further refine our baseline estimate by comparing pairs (or small groups) of particularly comparable foreign banks via a DiD design that matches them on a number of observables. In a host of robustness checks, we consider alternative strategies designed to isolate the causal effects of the crisis treatment, such as the inclusion of additional bank and country-level controls, falsification tests that consider whether alternative non-crisis mechanisms may be driving the results, examining potential channels of transmission for the crisis effect, and exploring various dimensions of heterogeneity in the crisis effect among the foreign banks. These complementary methodologies thus strengthen our confidence in our identification of a causal effect of crises, as well as provide some sense of whether certain bank or country-specific characteristics may have contributed to the estimated average treatment effect on the treated.

Our main result is that foreign banks owned by countries experiencing crises do in fact experience a post-crisis change in their lending that is relatively lower—by between 13 and 42 percentage points in our baseline—compared to non-crisis foreign banks. Thus, while foreign banks have, on average, been a force for financial stability in developing countries facing local financial crises (e.g., Clarke et al., 2003; de Haas and van Lelyveld, 2010; Martínez Pería et al., 2005), this is not the case when the crisis originated from the foreign bank's home country. In this case, rather than expanding lending in an attempt to diversify away from the shock experienced at home, such banks either repatriate capital to shore up the liquidity of their parents, or endure contractions in liquidity from their parents. When we explore the issue of heterogeneity among foreign banks further, we find additional evidence suggesting that non-crisis foreign bank lending may have helped offset reductions in post-crisis lending by crisis-stricken foreign banks and domestic banks. We also find that the crisis experienced by foreign banks in Eastern Europe was especially severe. Finally, although our evidence in favor of a negative crisis effect is somewhat weaker for the Asian crisis, our findings for this alternative case broadly corroborate those in our baseline.

The empirical literature on bank ownership and economic outcomes has grown dramatically over the past decade. However, in part due to data limitations, much of the literature tends to study a given country or region. Some of these studies have, like this one, been concerned with foreign bank behavior during crises.[3] For example, Peek and Rosengren (1997, 2000) document

[3] Other studies have explored the behavior of foreign banks in developing countries during normal times, or across the business cycle (e.g., Claessens et al., 2001; Clarke et al., 2005; Gormley, 2010; Khwaja and Mian, 2008).

a reduction in lending by Japanese banks in the United States after the bust of the Japanese stock market in 1990, while Schnabl (2012) presents evidence of negative spillovers via foreign banks of the Russian crisis of 1998 to firms in Peru. Aiyar (2012) considers UK banking activity during the crisis of 2007-08, and Cetorelli and Goldberg (2012b) do the same for U.S. banks. What is common among these papers is that they have been limited to a single country and/or crisis episode. The upshot is that it has been difficult to confidently distinguish idiosyncratic results from insights of a more general nature. In contrast, our study—which examines two distinct crisis episodes and includes economies spread across all regions of the world—offers the opportunity to draw broader implications regarding the causal effects of crises on foreign bank lending.

A number of papers have considered the influence of foreign bank ownership on credit across a wider range of countries (e.g., Cetorelli and Goldberg, 2011; Martínez Pería et al., 2005). In these papers, however, foreign bank presence in an economy is typically measured at an aggregate level, rather than the bank level we employ.

To the extent that some papers have worked with bank-level data, their bases for comparison have been different: de Haas and van Lelyveld (2010), for example, restrict their analysis to only subsidiaries of the 45 largest multinational banks, while Galindo et al. (2010) focus on Latin American host countries, and de Haas and van Lelyveld (2006) and Popov and Udell (2012) focus on Eastern European countries. Similarly, Cull and Martínez Pería (2013), Claessens and van Horen (2014), and de Haas and van Lelyveld (2014) are concerned with benchmarking lending by foreign subsidiaries of multinational banks against that of domestic banks; while Ongena et al. (2013) compare the lending of foreign-owned and internationally-borrowing domestic banks with that of locally-funded domestic ones. Because we are interested in the effects of a crisis in the home country on lending activity by foreign banks, our study restricts itself to only foreign-owned banks, since we believe that doing so helps us isolate our treatment effect against the most appropriate control group. In addition, our approach differs from the existing literature in that foreign banks are typically not mapped to their home countries, as we do, and which is essential for establishing a causal narrative involving home-specific shocks.

To our knowledge, the papers that are closest in approach to this paper are Wu et al. (2011), and three sets of papers by Peek and Rosengren (1997, 2000), de Haas and van Horen (2012a,b), and Giannetti and Laeven (2012a,b). Like this paper, Wu et al. (2011) consider foreign ownership at the bank level across emerging economies, and whether such banks respond differentially to exogenous shocks. That paper, however, paper is primarily concerned with the effect of monetary shocks in the *host* economy, while our focus is instead on shocks experienced in *home* economies. The Peek and Rosengren (1997, 2000) papers *are* concerned with shocks experienced in foreign banks' home countries, but the papers are essentially case studies of a pair of major developed economies (Japanese banks in the United States), as opposed to our broader developing country focus. Finally, while the concerns of the final four papers do overlap with ours, these analyses have relied on data of a more limited nature (for example, on cross-border syndicated lending only rather than overall lending, or a sample of banks limited to one geographical area). Just as important, most of these papers are not focused on addressing causal concerns in a systematic fashion, which we regard as a central contribution of this paper.

The paper is organized as follows. In the following section, we discuss the relevant theory underlying foreign bank lending during crises. This is followed by a description of our dataset and its main stylized features of banks during the financial crisis of 2007-08 (Section 3). Section 4 then outlines our econometric setup, along with a discussion of identification issues. Our baseline results are reported in Section 5, and robustness checks in Section 6. In Section 7 we explore heterogeneity among banks, for foreign relative to domestic banks, and between foreign banks. Section 8 extends our basic framework to the 1997-98 Asian crisis. A final section concludes with policy implications and avenues for future research.

2 Bank Lending during a Financial Crisis

In this section we discuss the main mechanisms by which foreign and domestic banks may differ in their lending behavior, and the channels by which a crisis may affect lending activity.

There is no single, well-established theory of how foreign banks' characteristics or lending decisions can be expected to differ systematically from those of domestically owned banks, nor of how a crisis in a foreign bank's home country can be expected to influence its lending. However, some mechanisms by which shocks are transmitted through international banking have been discussed in the literature. One important consideration is that subsidiary banks—whether they are part of a multinational or domestic banking group—typically do not operate completely independently of their parent company. In the case of a multinational banking group, this has two main implications in regards to the transmission of a shock within the group, as emphasized by Morgan et al. (2004).[4]

On one hand, when a foreign-owned bank is hit by a crisis in its parent's home country, the shock in the home country may be cushioned by repatriation of capital from the bank to its parent, or by capital reallocations from other subsidiary banks within the group that were relatively less exposed to the shock (termed a "support effect" by de Haas and van Lelyveld, 2010). Similarly, when the parent faces liquidity problems, the parent may pass these on by supplying less liquidity

[4] Although the model of Morgan et al. (2004) is applied to studying crises in host rather than home countries, the mechanisms in the model are applicable to the case of a crisis in the home country. de Haas and van Lelyveld (2010) and Cetorelli and Goldberg (2011) present similar ideas from the perspective of the balance sheet of a multinational bank. Goldberg (2009) provides an overview of key international spillovers through banking, including some mechanisms of crisis transmission.

to the subsidiary via the group's internal capital market (Cetorelli and Goldberg, 2012a).[5] This is analogous to a wealth or income effect.

On the other hand, when the parent bank faces a less favorable risk-return tradeoff in its home country, there is a substitution effect as well. The parent has an incentive to reallocate its portfolio of assets toward countries less affected by the crisis; that is, to reallocate liquidity to its subsidiaries in relatively safe havens so that more loans can be made there, rebalancing the group's loan portfolio in favor of these countries, and helping to shore up its overall balance sheet.

Which of these two effects dominates determines the net effect of a crisis in the home country on a foreign bank's access to liquidity and/or its solvency position, and thus the net effect on its lending behavior, which is therefore ambiguous.[6] The answer to this question is ultimately empirical.

One strategy for addressing this question is to compare the lending behavior of foreign banks which face a crisis in their home countries to the lending behavior of domestic banks, and infer whether there are any systematic differences between post-crisis lending activity in the two groups. But this could be problematic, since foreign banks likely differ from domestic banks in systematic ways in terms of their pre-crisis characteristics, which could also make a difference in terms of how a crisis affects their lending.

For example, some domestic banks are state-owned, and such banks may have a political mandate to cushion the economy from shocks by lending countercyclically (Brei and Schclarek, 2014). In the context of asymmetric information, which is especially relevant for developing countries, foreign banks may also face greater costs of acquiring information about borrowers, potentially leading to "cherry picking" the most attractive, or largest, clients (Dell'Ariccia and Marquez, 2004; Detragiache et al., 2008). Domestic and foreign banks may also differ in size (as shown in Subsection 3.2), capital structure, sources of funding, pursuit of longer client relationships versus "transaction-by-transaction" lending, and degree of lending to foreign versus domestic firms.

All these differences between foreign and domestic banks also suggest that the two groups likely face different demand schedules for loans in any given host economy. Thus, there is little reason to expect that lending by domestic and foreign banks in developing countries would have shared similar trends in lending between 2006 and 2009 had the crisis not occurred. This is the main reason why, in answering the question of how foreign banks' lending is affected by a crisis in its home country, we consider it more appropriate to compare these crisis-stricken foreign banks

[5] A similar mechanism, operating via the solvency channel, may be at work. The home crisis may weaken the parent bank's balance sheet, which reduces its creditworthiness and compromises its (and its subsidiaries') ability to raise funds from the wholesale market.

[6] Indeed, Cetorelli and Goldberg (2012c) present evidence that the operation of internal capital markets of U.S. banks in 2007 and 2008 was quite heterogeneous and depended on bank and host-country specific conditions. Some host markets operated as "funding" markets for some banks, seeing larger net flows to parent banks, while other host economies operated as "investment" markets, with increased net internal flows from parent to subsidiary.

to other foreign banks—those with *non-crisis* home countries of ownership—rather than domestic banks. This is the crux of the empirical strategy that we adopt in the paper.

3 Foreign Banks and the 2007-08 Financial Crisis

3.1 Data Source and Description

The dataset used in this paper is based on an extensive data collection effort on the evolution of bank ownership in developing countries for the period of 1995–2010. This dataset, in turn, builds on a dataset compiled by Claessens et al. (2008), which spans 1995–2005 for a smaller set of developing countries (about two-thirds of the current coverage).[7]

The coverage is for 4,496 commercial banks, savings banks, cooperative banks and bank holding companies in 131 developing countries.[8] The information sources used to build the dataset include *Bankscope* (the primary source), supplemented by individual banks' websites and annual reports, banking regulation agencies' publications and announcements, parent companies' reports, and news articles.

A bank is defined as foreign-owned if 50 percent or more of its shares are directly owned by foreign entities. Majority ownership is assessed annually based on shareholder information at the end of the year, or as close as possible to the end of the year when sufficient data are available. Nationality of ownership is based on direct ownership, except in certain cases when ultimate ownership is used.[9] If the majority of shares of a bank are held by foreigners but no single nationality accounts for a majority, then the foreign country with the highest share is considered the nationality of ownership.

Our definition of a banking crisis relies on the database of Laeven and Valencia (2013), in which a crisis is identified as a *systemic banking crisis* when two conditions are met:[10] first, there are significant signs of financial distress in the banking system (as indicated by significant bank runs, losses in the banking system, and/or bank liquidations); and second, significant banking policy intervention measures were undertaken in response to losses in the banking system. Because the quantitative thresholds used in this definition of systemic banking crises are *ad hoc*, events that almost meet these thresholds are classified as "borderline." By these criteria, Laeven and Valencia (2013) identify 147 crises in 115 countries for the period 1973–2009. Of these crises, 13 events are classified as borderline.

[7] The dataset has also been independently updated by Claessens and van Horen (2014), to the year 2009. The coverage of their database is substantially similar to ours.

[8] The observations are at legal entity level. However, the definition of a branch versus a subsidiary may vary between countries (Cerutti et al., 2007), and so we refrain from drawing a strong distinction between them in our paper.

[9] Additional detail on the rules used to construct this variable is provided in the Appendix.

[10] A detailed description of the construction of this variable is provided in the Appendix.

We combine these ownership and banking crisis variables to construct our crisis treatment effect, which is our main independent variable of interest. We define *crisis treatment* as an indicator variable for every foreign-owned bank that takes on the value of unity when its main country of ownership experienced a banking crisis in the years 2007-2008, and zero otherwise. The baseline definition includes all systemic crises identified by Laeven and Valencia (2013) for this period.[11] To avoid confounding home and host country crises, we also exclude from the pool of host countries all crisis-stricken countries.[12] The baseline definition yields a total of 17 systemic banking crises.

To assure a high-quality working sample, we refine the data in several ways. First, we consider only host countries where there is at least one operating bank from a crisis-stricken country and at least one foreign bank from a non-crisis country, so that a comparison can be made between these two groups (which is necessary for our research design). Second, we drop from the sample all host countries that have less than five operating banks (after excluding the cases mentioned before), so that our results are not driven by unrepresentative outliers. Finally, we exclude foreign banks that change their main country of ownership between 2006 and 2009, so that the country effect is well defined. The resulting sample comprises 361 foreign banks from 66 home countries, operating in 51 host countries. Of these banks, 208 are *treated* banks (their main country of ownership is one of the 17 countries that faced a systemic banking crisis in 2007-08), and the remaining 153 banks are controls.[13]

We merge our crisis treatment variable with additional information drawn from the *Bankscope* database, which includes year-by-year balance sheet and performance information for each bank. Our main dependent variable is a bank's total outstanding loans, net of reserves for impaired or nonperforming loans. The core set of bank-level controls includes bank size, solvency, the interest margin, and income-to-loan ratio; these are measured in standard ways. Size, for example, is proxied with total assets, while the income-to-loan margin is the net income share of total loans. Several commonly-employed bank-level variables—such as loan loss provisions (bank weakness)—are treated as non-core bank covariates (because they capture analogous concepts to the core variables, and/or suffer from weaker data availability; we test for sensitivity to this choice in robustness checks).

[11] The baseline definition also includes the borderline crises of France, Portugal and Slovenia, because the banking systems of these Eurozone countries are highly integrated with those of other Eurozone countries that experienced non-borderline systemic crises, such as Austria, Germany, Italy, Spain and the United Kingdom. We also include Nigeria, which is the only country that was coded as experiencing a crisis in 2009, because many analysts trace the genesis of the crisis to 2008. Our baseline results are robust to relaxing either of these assumptions, and are available on request.

[12] Including the few host countries that did experience crises do not qualitatively affect our results; indeed, doing so marginally raises the magnitude of our crisis treatment effect in most specifications. These additional results are available on request.

[13] Tables A.2 and A.3 in the Appendix provide additional information on the sample, organized by home and host countries.

The core set of (host and home) country-level variables consists of (lagged) real GDP growth, real GDP per capita, consumer price inflation, and the current account balance from the World Bank's *World Development Indicators* (WDI). Additional country-level covariates used in our robustness checks include trade openness and financial exports from the WDI; and the aggregate capital to assets ratio and ratio of banks' nonperforming loans to total gross loans from the World Bank's *Financial Development and Structure* database (Beck et al., 2000). Additional details on the definitions and sources of all variables are in the Appendix, and Table A.5 of the Appendix provides summary statistics for the main variables of interest.

3.2 Stylized Features of Banks in the 2007-08 crisis

To gain a better understanding of the research design, it is useful to consider several stylized facts present in the data. These concern the lending patterns of foreign vis-à-vis domestic banks, and the lending patterns of foreign banks with home countries that experienced a crisis relative to those with non-crisis home countries of ownership.

First, foreign banks with home countries experiencing a crisis do differ, on average, in their amounts of outstanding loans as compared to non-crisis foreign banks (*Fact 1*); in 2006, the mean for loans from the former group was $2.4 billion, versus $651 million for the latter.[14] Foreign banks exposed to the crisis in their home countries represent 61 percent of the sample of foreign banks—221 out of 361 banks—which provides some assurance that any estimated treatment effects are unlikely to be driven by outliers or small-sample problems.

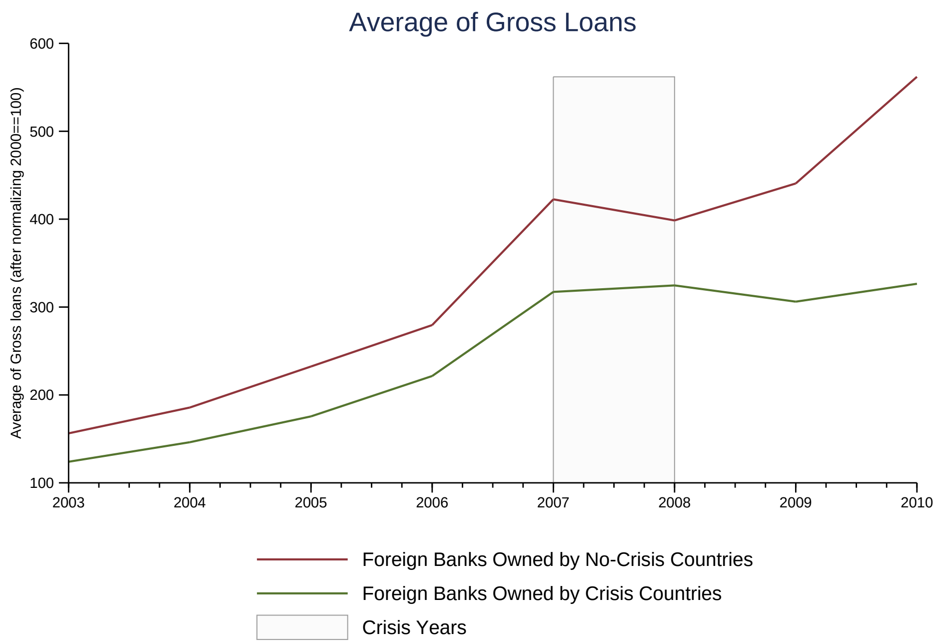
Next, a crucial feature of the data is that lending by both groups of foreign banks essentially followed the same trend up to the eve of the crisis (Figure 1): The three-year change in (log) average lending between 2004–06 is statistically indistinguishable between crisis and non-crisis banks (*Fact 2*).[15]

Taken together, these stylized facts argue strongly in favor of our difference-in-difference approach: the methodology allows for initial differences to exist between the two groups of interest (*Fact 1*), while the coincident pre-crisis trends (*Fact 2*) suggest that the two groups in question would likely have shared parallel trends in the absence of a crisis (the common trend assumption). Finally, as a preview of the results to follow, it is useful to compare the extent to which lending for each group changed between 2006 and 2009. For foreign banks that experienced home country crises, post-crisis average lending increased by 41 percent (by $972 million, from $2.4 billion), whereas banks that did not experience crises increased their average post-crisis lending by 56 percent (by $366 million, from $651 million). Thus, the recovery in lending for non-crisis foreign

[14] A matrix summarizing the means, dispersions, and differences for crisis-treated and non-treated foreign banks is provided in Table A.4 of the Appendix.

[15] Just as important, despite a small break in 2007–08, the trend in *non*-crisis banks' loans remained unchanged following the crisis. The three-year change in (log) average lending between 2004–06 and 2008–10 are statistically indistinguishable for non-crisis banks.

Figure 1. Trends in Average Gross Loans



This figure presents trends in average gross loans disaggregated by crisis treatment and nontreatment foreign banks for the period 2003–2010. For comparison purposes, the average for each group is normalized to 100 for 2000 (these trends are substantially unaltered by the normalization; see Figure A.1 of the Appendix). The crisis period is demarcated as 2007–2008. Similar rising trends for both groups are evident until 2007, and the divergence in trends following 2008 is striking.

banks was far sharper than that of crisis-stricken foreign banks; the difference in this change in lending patterns between the two groups is large and statistically significant, and constitutes our stylized *Fact 3*.[16]

4 Econometric Methodology

4.1 A Difference-in-Difference Design

The point of departure in our empirical analysis is a straightforward difference-in-difference setup:

$$l_{ijk,t} = \alpha + \gamma_0 crisis_k + \gamma_1 post_t + \delta\left(crisis_k \cdot post_t\right) + \epsilon_{ijk,t}$$

where the dependent variable $l_{ijk,t}$ is total lending of bank $i = 1, \ldots, I$ in host country $j \in J$ at time $t = \{2006, 2009\}$. Each foreign bank i also has as an attribute its home country of ownership $k \in K$. $crisis_k$ is an indicator variable that takes on the value of 1 when country k experiences a

[16] These results are also tabulated formally in Table A.5 of the Appendix.

systemic banking crisis in 2007-08 (the crisis treatment). $post_t$ is an indicator variable that takes the value 1 if $t = 2009$ (the post-crisis period). ϵ is an idiosyncratic error term, which, depending on the specification, may be clustered along host j and/or home k.

In an application with only two periods, there is a well-known correspondence between the simple difference-in-difference estimator above and its differenced form, where the equation above is identical to the regression

$$\Delta l_{ijk} = \beta + \tilde{\delta} crisis_k + \varepsilon_{ijk}, \tag{1}$$

where the operator Δ denotes the change between two periods. The coefficient of interest, $\tilde{\delta} = \delta$, captures the difference in the average change in lending for treated vis-à-vis untreated banks. Identification of the treatment effect hinges crucially on our common trend assumption (Imbens and Wooldridge, 2009). As shown in Subsection 3.2, this assumption is fulfilled in our baseline sample.

Given the wide variation in foreign bank types operating across different developing countries, bias in $\tilde{\delta}$ may be reduced, and the fit of the model improved, by introducing additional controls to (1). Country-level effects, for both the home and host, may be important in practice. For example, banks from Spain may adopt a different operational model for subsidiaries as compared to banks based in the United States, and as a consequence Spanish-owned banks may react differently to a crisis than U.S.-owned banks. Likewise, banks operating in different countries need not react similarly after a crisis in a foreign country, as they face distinct economic environments (for example, different monetary or regulatory policy regimes).

Accounting for these effects amounts to including bank and country-level fixed effects in the basic difference-in-difference setup:

$$\begin{aligned} l_{ijk,t} = {} & \alpha' + \gamma_0' crisis_k + \gamma_1' post_t + \delta' \left(crisis_k \cdot post_t \right) + \alpha_i + \alpha_j + \alpha_k \\ & + \gamma_2 \left(\alpha_j \cdot post_t \right) + \gamma_3 \left(\alpha_k \cdot post_t \right) + \epsilon_{ijk,t}', \end{aligned}$$

where α_i captures a bank-specific effect, and α_j and α_k represent country effects for the *host* and *home* countries, respectively.[17] Note that we have allowed for period-specific country effects (γ_2 and γ_3), but have constrained the coefficient on bank effects to be constant across the two periods.[18] The above specification can be rewritten as

$$\Delta l_{ijk} = \beta' + \tilde{\delta}' crisis_k + \alpha_j' + \alpha_k' + \varepsilon_{ijk}'. \tag{2}$$

[17] Including cluster-specific fixed effects should reduce, but not eliminate, within-cluster correlation in the residuals (Cameron et al., 2011). Consequently, we retain clustered standard errors alongside fixed effects in order to ensure both unbiased point estimates and accurate standard errors.

[18] In principle, a fully-saturated specification would allow for an additional interaction term $\alpha_i \cdot post_t$. In practice, however, doing so would give rise to degrees-of-freedom issues that would inhibit estimation.

Since the bank fixed effect α_i is time-invariant, it drops out of the first-differenced specification.[19] To the extent that accounting for period-specific bank effects can further improve the efficiency of our estimate of δ', we may wish to explicitly introduce additional bank-specific controls into (2). More specifically, we can estimate

$$\Delta l_{ijk} = \beta'' + \tilde{\delta}'' crisis_k + \alpha_j'' + \alpha_k'' + \boldsymbol{\beta}_1 \mathbf{B}_i + \varepsilon_{ijk}'', \tag{3}$$

where $\mathbf{B}_i$ is a vector of bank-specific characteristics. Populating $\mathbf{B}$ with additional (observable) bank controls then allows us to capture potential period-specific idiosyncratic bank effects.

Although including additional controls in (2) and (3) does mean that time-varying factors at the bank as well as the country level are accounted for, there are two problems with doing so. There is the possibility that introducing additional covariates may lead to a violation, rather than a strengthening, of our common trends assumption.[20] In addition, introducing period-specific coefficients for observable vectors of characteristics may also violate the exogeneity assumption, and hence result in biased estimates (Lechner, 2010). Consequently, $\tilde{\delta}'$ and $\tilde{\delta}''$ may capture only a relatively crude estimate of the average crisis treatment effect, whether conditioned on unobservable or observable controls.

But if we are reasonably confident of the identification of the treatment effect, comparing crisis-treated foreign banks with non-treated banks that share very similar observable characteristics—a matching difference-in-difference (matching DiD) specification—can further improve the quality of our estimate of δ (Abadie and Imbens, 2006).[21] Let

$$\Delta \hat{l}_{ijt}^{crisis} = \begin{cases} \frac{1}{M} \sum_{-i \in \Im_M(i)} \Delta l_{-ijt} & \text{if } crisis_k = 0, \\ \Delta l_{ijt} & \text{if } crisis_k = 1; \end{cases}$$

$$\Delta \hat{l}_{ijt}^{noncrisis} = \begin{cases} \Delta l_{ijt} & \text{if } crisis_k = 0, \\ \frac{1}{M} \sum_{-i \in \Im_M(i)} \Delta l_{-ijt} & \text{if } crisis_k = 1, \end{cases}$$

where $\Im_M(i)$ is the set of M matching indices. These changes in lending outcomes correspond to, respectively, foreign banks exposed to the crisis treatment and those that were not. Then the matching difference-in-differences estimator of Abadie and Imbens (2006) generates our coefficient of

[19] In other words, all time-invariant effects are implicitly accounted for in a first-differenced specification.

[20] In particular, including controls implies the assumption of parallel trends conditional on the *linear combination* of all covariates, rather than the less-restrictive assumption of parallel trends (Lechner, 2010).

[21] Note that in contrast to propensity score matching difference-in-differences, the matching estimator of Abadie and Imbens (2006) is not effected to determine *selection* into the crisis treatment; rather, the algorithm ensures comparability of treated and untreated banks. We compute the Abadie and Imbens (2006) matching estimator following the implementation described in Abadie et al. (2004), which performs matching with replacement, and with the bias correction suggested in Abadie and Imbens (2011).

interest given by

$$\tilde{\delta} = \frac{1}{I} \sum_{i=1}^{I} \left\{ \Delta \hat{l}_{ijt}^{crisis} - \Delta \hat{l}_{ijt}^{noncrisis} \right\}, \tag{4}$$

which we implement with the nearest-neighbor (Mahalanobis) metric. Note that the identification of the treatment effect in the matching DiD estimator depends on the assumption of *unconfoundedness*, which requires, conditional on covariates, that there be no unobservables associated with both the treatment and with the potential outcomes (Imbens and Wooldridge, 2009). As we argue below, our treatment—a home-country crisis—is plausibly independent of the lending activities of these countries' foreign subsidiaries in developing countries, thereby satisfying this assumption.

4.2 Identification of the Crisis Treatment

The estimation described in Subsection 4.1 hinges on whether, conditional on our sample, the crisis treatment is well identified. In this subsection, we discuss why we believe that this is the case.

First, it is worth noting that only banks that were majority foreign-owned were considered in our setup. As discussed in Subsection 3.2, this is because foreign banks with home countries that did *not* experience a crisis are the most appropriate comparison group for estimating the effect of the crisis treatment. Moreover, the difference-in-difference approach allows initial differences to exist between the two groups in question.

To further establish identification, it is necessary for the crisis treatment to satisfy the exogeneity assumption. We make this case in three steps. First, we observe that, by and large, developing country-based subsidiary banks of foreign multinationals are dwarfed by the size of their home country banking systems. Consequently, the likelihood that they influenced their respective home-country crises is extremely small.[22] Second, only certain home—typically, high-income—countries underwent a systemic banking crisis in 2007-08, and consequently, only a subset of the foreign-owned banking subsidiaries in our sample were exposed to the crisis treatment. It is this exogenous variation in home-country experiences that we exploit to identify the effect of a home-country crisis on foreign bank behavior in our baseline DiD specification.

The final issue involving identification is with regard to relevance: that is, whether our banking and financial crisis treatment, as captured by our sample of home crisis countries, is capturing the effect of a crisis *per se*, or whether other country-level macro factors are responsible for the observed treatment effects. In our robustness checks, we test this condition by attempting to rule out the possibility that some other possible channels may be responsible for the observed treatment effect.

[22] In addition, since we exclude host countries experiencing crises from the analysis, there is no concern that these banks induced crises in their hosts, either.

One concern that may arise regarding our working sample is the possibility of survivorship bias. Although there is undoubtedly attrition in our sample between 2006 and 2009, we view this issue as mainly a red herring. There is little reason to believe that, after conditioning on fixed effects, there would be any systematic variation in bank failures between the two groups *that are not directly attributable to the crisis*. Indeed, if anything, the magnitude of our estimate of the crisis effect would be biased *downward* by such attrition.[23]

5 Empirical Results

5.1 Baseline Difference-in-Differences

Our baseline results are shown in Table 1. In the first column (B1) we report the baseline difference-in-difference specification in (1) with no fixed effects. Below the estimated coefficient we report robust standard errors clustered either by home country, host country, or both.[24] The coefficient on the crisis treatment is statistically significant at the conventional levels, and negative; thus, the results indicate that foreign banks owned by entities in countries that experienced a financial crisis tended to reduce their lending more (or raise their lending less) than foreign banks owned by entities in non-crisis-stricken countries. As discussed in the introduction and Section 2, while this result strikes us as reasonably intuitive, the alternative outcome (of increased lending) is an *a priori* theoretical possibility.

One objection to this simple benchmark is that pooling all foreign banks, regardless of host country, may fail to account for heterogeneity in changing host country conditions. For example, since foreign banks generally extend loans in domestic currency, an appreciation of the currency could cause an increase in our dependent variable (loans measured in U.S. dollars). However, there is no reason to necessarily expect such host country effects to distort our estimate of the treatment effect. Provided that foreign banks experiencing the crisis treatment are just as likely as non-treatment foreign banks, *ex ante*, to locate in those host countries (in the example, those with appreciating exchange currencies), the host country effect would introduce no bias to the residual.

Moreover, any host or home-country-specific factors that give rise to initial differences between the lending of the treatment and non-treatment groups of banks are controlled for by

[23] Nevertheless, as an additional credibility check, we compute the (observable) means and standard deviations of the main dependent and independent variables for the subsample of banks that had observations in 2006 but not 2009, and compare these moments against those of our working sample. Only six banks were subject to attrition. Standard t- and F-tests for comparison of means and standard deviations indicate that, by and large, there are no significant differences between moments for observables in the two samples.

[24] Unlike the inclusion of fixed effects, there is generally less consensus on the appropriate treatment of clustered errors, especially since such corrections in the presence of a small number of groups can lead to downward-biased standard errors in DiD settings (Donald and Lang, 2007). In our application, we cluster errors by host and/or home country (rather than treatment), so the number of groups is reasonably large, which mitigates this concern. Still, since the decision typically involves a tradeoff between robustness and efficiency, we report all three possible permutations of clustering in the baseline results.

the DiD strategy, as long as they do not also give rise to differences in *changes* in the lending behavior of the two groups over the crisis period. Any time-invariant bank-specific effects are also captured by even this simple specification. In sum, as long as the identifying assumptions described in Subsections 4.1 and 4.2 hold, we are assured that the estimates in column (B1) are unbiased (Imbens and Wooldridge, 2009).

Even so, if we believe that efficiency is enhanced by allowing for period-specific country effects, it is straightforward to introduce these into the baseline setup as in equation (2). One concern is to control for demand-side effects on changes in lending, which can be done by adding host country fixed effects. Accordingly, column (B2) reports results when we allow for these. Doing so (unsurprisingly) substantially improves the fit of the model, and slightly raises the point estimate for the treatment effect. This is not the case when we control for only home fixed effects, where—as shown in column (B3)—the magnitude of the coefficient falls, although the lower R^2 alongside the mostly smaller standard errors strongly suggest that this result is due to omitted variable bias. Column (B4) reports the fullest articulation of equation (2), where we include period-specific fixed effects for both home and host economies. The estimated crisis effect in this case is even stronger than in (B1), and is significant at the 5 percent level or better, regardless of our choice of error clustering.

Table 1. Baseline Difference-in-Difference Regressions for Bank Lending, 2006 and 2009

	B1	**B2**	**B3**	**B4**
Crisis effect	-0.316	-0.364	-0.127	-0.420
	(0.13)**	(0.12)***	(0.00)***	(0.16)***
	(0.14)**	(0.16)**	(0.39)	(0.21)**
	(0.14)**	(0.16)**	(0.10)	(0.17)**
Fixed effects				
Home	No	No	Yes	Yes
Host	No	Yes	No	Yes
Adj. R^2	0.021	0.307	0.245	0.490
Clusters (countries)	66, 51	66, 51	66, 51	66, 51
Estimation	OLS	OLS	OLS	OLS
N (banks)	361	361	361	361

The dependent variable is in log differenced form. Heteroskedasticity and intragroup correlation-robust standard errors are reported in parentheses; the rows correspond to standard errors: (1) clustered by home country; (2) clustered by host country; (3) with two-way clustering. A constant term was included in the regressions, but not reported. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Fixed effects for home and host are period-specific. Cluster sizes are reported for home and host, respectively.

The magnitude of the coefficient is also economically significant: using the final specification (B4), foreign banks exposed to the 2007-08 financial crisis in their home countries pared back on their lending in their developing country hosts by an average of 42 percent (relative to foreign banks whose home countries did not experience a crisis). This means that for a crisis-stricken foreign bank with an average change in lending of $935 million—the actual average 2006–09 change for crisis-stricken banks—the bank would hypothetically have lent $\$\left[935/\left(1-0.42\right)\right]=\$1.6$ billion instead in the absence of its home-country crisis.

Finally, it is worth considering what the period-specific country fixed effects mean for our estimated coefficients. Columns (B2)–(B4) essentially allow home and host country fixed effects to take on different slopes in the post-crisis period, capturing distinct country-specific responses to the crisis. The higher coefficient in column (B4)—as compared to (B1)—thus suggests that the effect of the crisis on lending might well have been greater absent crisis mitigation policies such as the expansion of central bank balance sheets (since the smaller estimated effect in column (B1) would be due to not controlling for these heterogeneous policies).[25]

5.2 Baseline with Additional Bank Covariates

In Table 2 we consider the inclusion of a set of covariates at the bank level, along the lines of equation (3). As noted earlier, expanding the set of covariates is not necessary if the treatment is well identified or if idiosyncratic bank effects are time-invariant. As discussed earlier, there are reasonable objections to the indiscriminate inclusion of additional controls; set against these potential disadvantages is the fact that including covariates in (3) allows us to capture the possibility that bank effects may be period-specific, as they well could be after a major shock such as a financial crisis. Our resolution of this latter problem is to include our covariates as they are observed in the pre-crisis period.

In Table 2, we incrementally introduce the six idiosyncratic bank-specific measures that we define as our core set of bank controls (these are described in Subsection 3.1, with further details provided in the Appendix). This core set is chosen to best capture important (observable) cross-bank heterogeneity that may potentially affect foreign bank lending behavior.

The main message from this set of results is that, compared to the bare-bones specifications in Table 1, the magnitude and significance of the crisis effect generally holds. Overall, the results here point to post-crisis lending by crisis-stricken banks that is 26 to 57 percent lower than that of their non-crisis counterparts. The point estimates here are also, on average, a hair larger than those in the baseline, accompanied by higher standard errors.[26] A more powerful way to control for

[25] Admittedly, this interpretation would only be definitive if we were willing to make the *ceteris paribus* assumption of unchanged demand conditions in each country. The general point about the crisis effect being underestimated in the simple DiD specification without fixed effects will continue to hold, however.

[26] To limit clutter, we report only standard errors that correspond to two-way clustering; analogous results are obtained when clustered by either home or host countries, and are available on request.

Table 2. Difference-in-Difference Regressions for Bank Lending, with Core and Additional Bank-Level Covariates, 2006 and 2009

	C1	**C2**	**C3**	**C4**	**C5**	**C6**
Crisis effect	-0.256	-0.571	-0.548	-0.508	-0.397	-0.361
	(0.14)*	(0.26)**	(0.24)**	(0.27)*	(0.22)*	(0.26)
			Core bank-specific characteristics			
Size	-0.110	0.028	0.029	0.017	0.008	0.012
	(0.11)	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)
Solvency		0.000	0.000	0.000	0.000	0.000
		(0.00)*	(0.00)*	(0.00)	(0.00)	(0.00)
Interest margin			-0.000	-0.000	-0.000	-0.000
			(0.00)	(0.00)	(0.00)	(0.00)
Income-to-loan				-0.007	0.247	0.253
				(0.01)	(0.04)***	(0.04)***
Wholesale					0.001	0.002
					(0.00)	(0.01)
Liquidity						0.005
						(0.01)
Fixed effects						
Home	Yes	Yes	Yes	Yes	Yes	Yes
Host	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.502	0.548	0.550	0.558	0.660	0.663
Clusters (countries)	66, 51	66, 51	66, 51	66, 51	66, 51	66, 51
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
N (banks)	361	361	361	361	344	343

The dependent variable is in log differenced form. Heteroskedasticity and intragroup correlation-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. All bank-level covariates enter with their values set in the pre-crisis period ($t = 2006$). Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

covariates is to follow a matching DiD strategy, which is the exercise we undertake in the following subsection.

5.3 *Matching Difference-in-Differences*

Table 3 reports results for the crisis treatment effect of equation (4), estimated by DiD estimates matched on the set of home and host country-specific covariates[27] and core bank covariates.[28] Since there is no agreement on an optimal number of matches that should be chosen (Imbens and Wooldridge, 2009), we present results for one through four matches in columns (M1)–(M4).[29]

Table 3. Matching Difference-in-Difference Regressions for Bank Lending, with Bank- and Country-Level Controls, 2006 and 2009

	M1	**M2**	**M3**	**M4**
Crisis effect	-0.210	-0.279	-0.317	-0.304
	(0.13)	(0.12)**	(0.11)***	(0.11)***
Core host covariates	Yes	Yes	Yes	Yes
Core home covariates	Yes	Yes	Yes	Yes
Core bank covariates	Yes	Yes	Yes	Yes
Estimation	Matching	Matching	Matching	Matching
Matches	1	2	3	4
N (banks)	328	328	328	328

The dependent variable is in log differenced form. Point estimates computed from matching with replacement based on the Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity-robust standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank and country-level covariates enter with their values set in the pre-crisis period ($t = 2006$).

The qualitative findings remain largely unchanged. The matching DiD estimates are in the same ballpark as the simple DiD regressions and those obtained when bank covariates are included, and the (statistically significant) crisis effect coefficients range from -0.28 to -0.32. In the fullest articulation of the baseline model with four matches—shown in column (M4)—foreign

[27] In lieu of host and home fixed effects we use a set of core country variables (GDP per capita, GDP growth, inflation, and current account balance) as matching variables. As was the case for simple difference-in-differences, there is a case against overfitting of covariates.

[28] In this baseline, we eschew exact matching (which we relax in Section 7 when domestic banks are included). With significant cross-country differences in the levels of the main variables and a nontrivial number of covariates, exact matching offers little improvement over the bias-corrected estimates, at the cost of complicating our interpretation of the matched entity (the percentage of exact matches falls as low as 47 percent). In any case, imposing exact matching yields qualitatively very similar results, as shown in Table A.6 of the Appendix.

[29] The choice of one match is entirely reasonable—we wish to compare only banks existing in the data, rather than synthetic comparators—and the choice of four matches has been shown to perform well in terms of minimizing mean-squared error (Abadie and Imbens, 2011). We also considered higher numbers of matches. In general, these decreased the magnitude of the estimated coefficient, but even for the (extreme) case of 20 matches, the coefficient remained statistically significant (see Table A.6 of the Appendix).

banks exposed to a financial crisis in their home countries have changes in lending that are 30 percent smaller, on average, than an otherwise comparable foreign bank whose home country did not experience a crisis.

Although the magnitudes of the point estimates are comparable to those reported in Tables 1 and 2, it is worth noting that matching DiD may in fact provide a superior estimate. In the simplest DiD implementation with no additional controls, all crisis and non-crisis banks are pooled together and identification of the average crisis treatment effect relies on the more-or-less random distribution of other characteristics across the sample. But such pooling may fail to accurately gauge the true extent of the crisis effect if banks' observable characteristics are correlated with both the treatment and error term, and including covariates may not completely resolve this.[30] In contrast, the matching estimator forces the comparison to occur either with an otherwise similar (at least along observable dimensions) bank, or with a synthetic equivalent. To the extent that matching on observables does not introduce any selection bias—and with a fairly comprehensive set of core bank controls there is little reason to think it would—the estimate renders a better apples-to-apples comparison.

6 Robustness Checks

6.1 Additional Controls and Alternative Measures

In this subsection we consider a range of robustness checks that offer variations on our choice of controls in the baseline. These are reported in the six columns on the left panel of Table 4.

We first replace two variables in the core set of bank covariates with alternative measures, bank weakness and profitability.[31] Specifically, we substitute interest margin with profitability, and liquidity with weakness. These results are given in columns (R1) and (R2) which build on, respectively, the DiD specification with bank covariates—a variation of specification (C6)—and the matching DiD equivalent, which is a variation of specification (M4).

Next, we allow for the possibility of period-specific effects that operate at the country-pair level (as opposed to independently at the country level). More specifically, we replace the home and host country fixed effects α'_j and α'_k in (2) with a fixed effect α'_{jk} for each unique home-host dyad. This approach will absorb greater unobservable heterogeneity insofar as pairwise effects—such as those arising from economic closeness at the bilateral level (de Haas and van Horen, 2012b)—are relevant to lending behavior. Since this approach to capturing fixed effects is fundamentally distinct

[30] If the covariate distributions differ substantially between the crisis and non-crisis groups, then the estimates depend on extrapolation out of sample, and become much more sensitive to parallel trends assumptions.

[31] The reason why we choose not to include all four additional covariates together is twofold: some of these variables capture very similar concepts, and so including them simultaneously may introduce multicollinearity; moreover, doing so would seriously erode the size of our sample (since the coverage of these additional controls does not overlap perfectly).

Table 4. Robustness of DiD and Matching DiD Regressions for Bank Lending, with Alternative and Additional Bank and Country-Level Controls, 2006 and 2009

	R1	R2	R3	R4	R5	R6	R7	R8	R9	R10
			t = 2006, t+1 = 2009					*t = 2005–06, t+1 = 2009–10*		
Crisis effect	-0.357	-0.456	-0.521	-0.296	-0.539	-0.563	-0.418	-0.630	-0.460	-0.493
	(0.26)	(0.12)***	(0.00)***	(0.31)	(0.13)***	(0.15)***	(0.19)**	(0.17)***	(0.18)***	(0.17)***
Fixed effects/core covariates										
Home	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Host	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Pair	No	-	Yes	Yes	-	-	No	-	No	-
Bank	No	No	No	Yes	Yes	No	No	Yes	Yes	No
Other covariates?										
Additional country-specific	No	No	No	No	Yes	Yes	No	No	No	No
Alternative set bank-specific	Yes	Yes	No	No	No	Yes	No	No	No	Yes
Adj. R^2	0.674		0.800	0.914			0.506		0.548	
Clusters (countries)	66, 51	-	66, 51	66, 51	-	-	66, 51	-	66, 51	-
Estimation	OLS	Matching	OLS	OLS	Matching	Matching	OLS	Matching	OLS	Matching
Matches	-	4	-	-	4	4	-	4	-	4
N (banks)	326	312	361	343	237	226	361	343	355	341

The dependent variable is in log differenced form. Matching estimates computed with Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity (all specifications) and intragroup correlation (OLS only)-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Fixed effects for home, host, and pair are period-specific. Core bank and country covariates are listed in the Appendix. The alternative set of covariates includes *profitability* in lieu of interest margin and *weakness* in lieu of liquidity. Additional country covariates are related to the banking system (bank capital, bank nonperforming loans) and economic openness (trade openness, financial services exports), and are listed in the Appendix. All bank and country-level covariates enter with their values set in the pre-crisis period ($t = 2006$). Cluster sizes are reported for home and host, respectively.

from even our simple augmented model (2), we show first the DiD results obtained without bank covariates altogether in column (R3), and then with all core bank covariates in (R4) (analogous to specifications (B4) and (C6), respectively).

Third, we add additional country and bank-level covariates (although with the same caveat as before that doing so tends to result in significant sample size reductions, which justifies our decision not to use them in the baseline). These additional covariates relate to country-level characteristics (related to the quality of the financial system and the openness of the economy) and to the alternative bank-level controls (weakness, profitability) introduced in the first two columns. The results for each are reported, respectively, in columns (R5) and (R6).

In the right panel we examine the robustness of our results to an alternative measure of our pre and post-crisis periods: rather than utilizing data from two individual years (2006 and 2009), we average observations from 2005 and 2006 for the pre-crisis period, and 2009 and 2010 for the post-crisis period.[32] Here, for reasons of space, we report only the DiD and matching DiD estimates when controlling for only country-level fixed effects/core covariates (columns (R7) and (R8), respectively), and with core and alternative bank covariates (columns (R9) and (R10)).

As evident from Table 4, our baseline results by and large survive this array of robustness checks. There is little variation in the magnitude of the estimated crisis treatment effect, although the range is broader: coefficient estimates are bound by $[-0.30, -0.63]$, and most retain their statistical significance at the 10 percent level or lower.

We make one final, brief remark regarding the estimates in Table 4: the stability of the coefficients across this broad array of specifications lends a fair amount of confidence that the crisis treatment effect is not only real, but also reliably estimated. Thus, even the most parsimonious DiD specification represented by equation (1) is likely sufficient for our central claim. With this in mind, we turn away from estimating the crisis treatment effect, and toward possible falsification tests in order to build our case that these estimates are indeed valid.

6.2 Falsification Tests for Alternative Channels

In this subsection we introduce a set of distinct placebo tests designed to rule out the possibility that the estimated effect of the crisis treatment may either be due to non-crisis-related trends in the two groups, or to other, distinct non-crisis shocks that occurred between 2006 and 2009 which were correlated with the crisis treatment.

Our first test alters the pre and post-crisis dates to an earlier period; we choose 2002 and 2005 as alternative years.[33] This falsification test is designed to rule out the possibility that trends in

[32] We perform the period averaging to avoid serial correlation problems that may arise from difference-in-difference treatments that span multiple time periods (Bertrand et al., 2004). Data limitations prevent us from using longer averages.

[33] These two years are chosen to maximize data coverage, as data availability for most bank-level controls is quite limited for years prior to 2002.

Table 5. Falsification Tests for Difference-in-Difference Regressions for Bank Lending

	F1	F2	F3	F4	F5	F6
	t=2002, t+1=2005		*treatment=trade*		*treatment=fiscal*	
Treatment effect	0.077	-0.330	0.889	0.649	0.517	0.463
	(0.32)	(0.31)	(0.33)***	(0.73)	(0.24)**	(0.27)*
Fixed effects/core covariates						
Home	Yes	Yes	Yes	Yes	Yes	Yes
Host	Yes	Yes	Yes	Yes	Yes	Yes
Bank	No	Yes	No	Yes	No	Yes
Adj. R^2	0.442	0.555	0.490	0.663	0.490	0.663
Clusters (countries)	49, 42	49, 42	66, 51	66, 51	66, 51	66, 51
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
N (banks)	265	250	361	343	361	343

The dependent variable is in log differenced form. Heteroskedasticity and intragroup correlation-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. All bank-level covariates enter with their values set in the pre-crisis period ($t = 2006$). Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

lending behavior in the two groups may already have been diverging prior to 2006. Consequently, if coefficient estimates for the crisis variable are *in*significant, we can more confidently assert that our crisis effect is capturing a genuine shock experienced between 2006 and 2009. The first panel of Table 5 reports the results of this first set of placebo tests for the augmented DiD specifications (2) and (3) (columns (F1) and (F2) corresponding to specifications (B4) and (C6), respectively). The insignificant estimated coefficients indicate that the baseline estimations of the treatment effect in the DiD model are indeed capturing an effect unique to the period between 2006 and 2009.[34]

Our second falsification exercise considers the other major non-financial crisis-related event that occurred in the intervening period: the great trade collapse of 2008-09 (Baldwin, 2009).[35] Of

[34] One concern that may arise is that the sample sizes in Table 5 are substantially smaller than those in our baseline estimates, and so what is being captured is due to changes in the sample, rather than a genuine insignificant effect. To allay this concern, we replicated the two specifications for two other subsamples: first, we repeat the exercise for this smaller subsample for 2006–09, and second, we re-estimate the 2002–05 placebo for the subsample resulting from the first step (due to incomplete data coverage, the first step shrinks the subsample even further). The crisis effect is significant in the reduced subsample from the first step, and the placebo is insignificant in the second step. Taken together, these additional tests strongly suggest that the results are not due to sample variations.

[35] It is reasonable to argue that the trade collapse occurred in 2008 as a direct consequence of the financial crisis, and so cannot be treated as an entirely separate event. However, the main mechanisms involved in each case are distinct: one is a real side shock that affects other economies via exports, while the other is a nominal shock via financial flows. Moreover, there is imperfect overlap between economies suffering trade contractions as opposed to financial crises. Both of these reasons suggest that a separate treatment of the issue is warranted.

course, financial crises and other economic crises are likely to be correlated, so home countries we identify as having experienced a banking crisis may have also undergone trade-related changes around the same time, which could in turn have affected their banks' subsidiaries' lending abroad.

For example, if Spain's imports from Mexico collapse, and Spanish-owned banks in Mexico tend to lend more to firms that export to Spain vis-à-vis other banks in Mexico (perhaps because such exporters are also Spanish-owned), then these banks will face a greater decrease in loan demand during the crisis than other banks. Thus, their lending will fall disproportionately more compared to lending by other banks in Mexico. If this effect is systematic across country pairs, it will show up as an effect of the home-country banking crisis, when the effect is actually that of a collapse in home-country import demand (and associated financing needs abroad).

To rule out this channel, the falsification test requires the construction of a new treatment variable that captures the effect of a trade collapse, a replacement of the crisis treatment with this dummy, and new estimates that rely on differences between the two sets of treatments to identify the trade contraction effect. The second panel of Table 5 shows the results of this second set of falsification exercises. In columns (F3) and (F4), we again estimate the augmented DiD specifications both with and without bank covariates, but replace the crisis treatment with a trade collapse treatment that takes on the value of unity when the contraction in the home country's total trade falls below the median of all declines in trade (that is, the 50th percentile of all decreases in home-country trade flows; full details for the construction of this treatment are provided in the Appendix).[36]

The coefficients in this case are *positive* and, in one of the two cases, statistically significant. This indicate that our crisis treatment effects are not driven by trade contractions; if anything, foreign banks from economies that experienced trade collapses lent relatively *more* than those that did not have trade collapses in their home countries. Thus, to the extent that trade collapses affected lending by foreign banks from crisis economies at all, this effect operated in the opposite direction as the crisis treatment, and would have diminished its estimated effect.

The final falsification exercise that we implement turns to the possibility that fiscal stimuli, introduced as a result of weak global economic conditions in 2008, may have supported growth in economies receiving such a positive real shock, and subsequently allowed parent banks in these countries to systematically expand the lending activities in their home economies. To the extent that this expansion crowds out lending by their developing country subsidiaries (improved liquidity and economic conditions at home leads a bank to substitute away from increased lending by its subsidiaries abroad), the treatment is capturing a beggar-thy-neighbor spillover from fiscal policy expansion rather than a financial crisis effect.

[36] Our qualitative findings do not depend on this choice of cutoff; Table A.7 reports results with cutoffs at the 30th and 70th percentiles.

We thus code economies as having a fiscal stimulus treatment as those who exceeded the median expenditure among all countries that adopted fiscal stimuli (variable construction details are in the Appendix). The two columns in the third panel, (F5) and (F6), perform this falsification test when we replace our crisis treatment with this fiscal stimulus one. As in the case of the trade collapse, we find no evidence that the fiscal stimulus explanation is driving our results obtained earlier; if anything, the positive and significant coefficients point to a bias *against* our estimated crisis effect, and expansionary fiscal policy may have led to an expansion of bank lending not just at home but also in their foreign subsidiaries (so that fiscal policy is prosper-thy-neighbor).

6.3 Potential Channels of Transmission for the Crisis Effect

In this subsection we attempt to pin down the transmission channels for the crisis effect. In contrast to the previous subsection—where we sought to falsify the crisis channel by introducing alternative treatment constructs that tested whether the crisis effect would hold under varying placebos—our strategy here is to examine whether specific sources of variation are routinely associated with our crisis treatment, *per se*.

Consequently, our strategy here is to restrict our analysis to home countries that *did* experience crises, and to explore whether *changes* in bank-level liquidity or solvency are correlated with changes in bank lending. Both liquidity (Bernanke, 1983) and solvency (Bernanke and Gertler, 1989; Freixas and Jorge, 2008) channels have been routinely identified as key channels for monetary transmission (in general) as well as shocks that can affect credit provision (in particular). These channels are therefore prime candidates for the transmission of the home-country banking crisis.[37]

Adding changes to liquidity and solvency (rather than initial levels, as in our baseline) introduces new technical considerations. First, since both are measured as ratios to total assets, normalizing the dependent variable with a common denominator enables a cleaner comparison. In addition, this normalization allows us to circumvent an indeterminacy problem that could arise with changes in the solvency measure.[38] We accordingly replace Δl_{ijk} with $\Delta \frac{l_{ijk}}{a_{ijk}}$, where a_{ijk} are the total assets of bank i hosted in country j with home country k. Specifically, we regress

$$\Delta \frac{l_{ijk}}{a_{ijk}} = \beta''' + \Delta \frac{C_{ijk}}{a_{ijk}} + \alpha_j''' + \alpha_k''' + \varepsilon_{ijk}''', \tag{5}$$

[37] During banking crises, the external finance premium often rises. This increase may limit banks' ability to raise additional financing, and hence lower their available supply of intermediated credit (a liquidity shock). The higher premium can also erode the value of assets held on banks' balance sheets, lowering their net worth, and hindering their access to wholesale funds; this may likewise induce a reduction in bank lending (a solvency shock).

[38] In particular, a decrease in equity (the numerator) may be offset by an even greater decline in assets (the denominator)—an entirely possible outcome after a crisis—so that the ratio paradoxically *rises*. Normalizing by a common denominator rules out such potentially contradictory changes.

where $k \in K^c \subset K$, $k \in K^c \ \forall \ crisis_k = 1$, and C_{ijk} is a channel variable that is a measure of either solvency or liquidity.

Table 6. OLS Regressions for Change in Bank Loan-to-Asset Ratio with Changes in Channel Variables, 2006 and 2009

	X1	**X2**	**X3**	**X4**	**X5**	**X6**
Δ Liquidity	-0.559	-0.607			-0.566	-0.617
	(0.06)***	(0.09)***			(0.07)***	(0.09)***
	(0.07)***	(0.10)***			(0.07)***	(0.10)***
	(0.06)***	(0.09)***			(0.07)***	(0.10)***
Δ Solvency			-0.294	-0.262	-0.333	-0.279
			(0.18)	(0.25)	(0.15)**	(0.23)
			(0.14)**	(0.21)	(0.13)**	(0.26)
			(0.17)*	(0.24)	(0.15)**	(0.25)
Fixed effects						
Home	No	Yes	No	Yes	No	Yes
Host	No	Yes	No	Yes	No	Yes
Adj. R^2	0.420	0.665	0.039	0.363	0.451	0.679
Clusters (countries)	17, 51	17, 51	17, 51	17, 51	17, 51	17, 51
N (banks)	194	194	208	208	194	194

The dependent and channel variables are in differenced form. Heteroskedasticity and intra-group correlation-robust standard errors are reported in parentheses; the rows correspond to standard errors: (1) clustered by home country; (2) clustered by host country; (3) with two-way clustering. A constant term was included in the regressions, but not reported. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Fixed effects for home and host are period-specific. Cluster sizes are reported for home and host, respectively.

It is also important to exercise caution in the interpretation of the coefficients. An increase in the ratio of liquid assets between 2006 and 2009 may reflect an improvement of a bank's already-strong liquidity position, but it is also consistent with a liquidity-starved bank that needed to significantly shore up its liquidity following the crisis. Our interest is therefore less in the *sign* of the coefficients, but their statistical significance and relative magnitudes (setting up a "horse race" between the two). Table 6 presents standardized coefficients for all specifications that we consider. For economy, we report only the difference-in-difference specifications without (odd-numbered columns) and with (even-numbered columns) country fixed effects and two-way clustering, although other variations are qualitatively similar and support our main conclusion drawn here.[39]

The results make a fairly convincing case that in the recent crisis, bank-level access to liquidity, rather than concerns over solvency, were the key transmission channel for banks that

[39] A number of these additional results are reported in Table A.8 of the Appendix.

scaled back on lending due to home country crises. The coefficients for changes in liquidity are consistently statistically significant, and twice the magnitude of those for solvency. Importantly, while potential endogeneity in the channels variables militates against making any causal claims, we can nevertheless infer that changes in liquidity are more consistently associated with changes in lending, and hence are a more likely transmission channel for the crisis effect.

7 Heterogeneity in the Crisis Effect

7.1 Comparing Foreign Banks to Domestic Banks

In Subsection 3.2 we made the case for why non-crisis foreign banks are the most appropriate comparison group for estimating the effect of the crisis treatment: introducing domestic banks into the working sample would be inappropriate from an econometric perspective when using the DiD estimator, since it would pool all control banks in the comparison group even though that domestic and foreign banks differ along important dimensions related to changes in lending outcomes (for example, facing different loan demand schedules).

However, much of the empirical literature (cited in the introduction) *is* interested in distinctions between domestic and foreign bank behavior, and it is informative to expand our analysis to include domestic banks. While the use of the DiD estimator remains circumspect for the purposes of obtaining a causal estimate of the crisis effect when domestic banks are included in the sample, the matching estimator offers an ideal solution to this problem: since matching uses the closest possible match(es) for each treated bank, we are assured that the control is, in fact, an appropriate counterfactual. A further advantage of this empirical strategy is that it allows us to expand to a larger pool of controls from which matches are chosen.

Table 7 presents the results of the matching estimator using this expanded sample, analogous to Table 3, which now allows both domestic banks and non-crisis foreign banks to be used to construct controls. In these regressions, we match treated banks only with non-treated banks that operate in the same host economy; accordingly, the only covariates used for matching—other than constraining matches to the same host country—are bank-level covariates.[40]

The results are in line with our baseline matching DiD (and simple DiD) estimates, though the magnitudes are slightly lower. The smaller coefficients obtained when allowing for matching with domestic banks suggest that the post-crisis recovery in lending for foreign non-crisis banks, as compared with domestic banks, may have been larger. This also suggests that total lending in the host countries may well have been lower in the absence of these non-crisis stricken foreign

[40] Note that the algorithm used in this case performs *exact* matching (or as exact as possible) on the country of operation. In practice, because there may not exist the full complement of M members among the non-crisis group for each crisis-stricken bank, fewer than M matches may be used. To provide a sense of how many exact matches exist (as a rough gauge of the quality of the matches), we report the percentage of exact matches possible in the sample, which ranges from 90 percent when using one matching non-crisis bank to 78 percent when matching with four banks.

Table 7. Matching Difference-in-Difference Regressions Using Exact Matching for Host Country with Sample Expanded to Include Domestic Banks, 2006 and 2009

	D1	**D2**	**D3**	**D4**
Crisis effect	-0.199	-0.175	-0.158	-0.177
	(0.07)***	(0.07)***	(0.07)**	(0.07)***
Core bank covariates	Yes	Yes	Yes	Yes
Exact host matching	Yes	Yes	Yes	Yes
Exact matches (%)	89.7	86.1	81.8	78.4
Estimation	Matching	Matching	Matching	Matching
Matches	1	2	3	4
N (banks)	1,021	1,021	1,021	1,021

The dependent variable is in log differenced form. Point estimates computed from matching with replacement based on the Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity-robust standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank-level covariates enter with their values set in the pre-crisis period ($t = 2006$).

banks, whose post-crisis lending likely exceeded that of domestic banks. This is important, as most studies so far compare domestic and foreign banks, with no distinction between crisis and non-crisis foreign banks (Claessens and van Horen, 2014; de Haas and van Lelyveld, 2014). Some of these studies are then led to conclude that foreign banks may have reduced their lending vis-à-vis domestic banks. Our findings, in contrast, point to important differences among foreign banks.

To explore this issue further, we compare lending by foreign banks headquartered in *non-crisis* countries with that of domestic banks. We assign treatment to the first group, and run similar DiD and matching DiD regressions as before.[41] The results in Table 8 verify that foreign banks from non-crisis countries *increased* their lending relatively more than domestic banks: the coefficients on the foreign non-crisis term are positive, and in a number of specifications, statistically significant.

7.2 Comparing Distinct Features among Foreign Banks

The heterogeneity among foreign banks that arose in the previous subsection hints at the potential value of further investigating how differences among foreign-owned banks are related to changes in lending when a bank's home country experiences a crisis. We disaggregate foreign banks along

[41] The same caveats we previously noted on the comparability of foreign banks vis-à-vis domestic banks apply to this exercise. We nevertheless present estimates with the simple DiD estimator for completeness, and to allow for comparisons with existing studies that run fixed effects regressions (which are similar in spirit to our basic DiD model).

Table 8. DiD and Matching DiD Regressions Comparing Non-Crisis Foreign Banks ($treat = noncrisis$) with Domestic Banks Using Exact Matching for Host Country, 2006 and 2009

	N1	N2	N3	N4	N5	N6
Noncrisis foreign bank effect	0.234	0.222	0.187	0.036	2.976	2.889
	(0.12)*	(0.27)	(0.28)	(0.10)	(0.08)***	(0.08)***
Fixed effects/core covariates						
Home	No	Yes	Yes	-	-	-
Host	No	Yes	Yes	-	-	-
Bank	No	No	Yes	Yes	Yes	Yes
Exact host matching	-	-	-	Yes	Yes	Yes
Exact matches (%)	-	-	-	78.5	72.5	67.5
Adj. R^2	0.012	0.420	0.446	-	-	-
Clusters (countries)	74, 51	74, 51	74, 51	-	-	-
Estimation	OLS	OLS	OLS	Matching	Matching	Matching
Matches	-	-	-	2	3	4
N (banks)	827	827	827	827	827	827

The dependent variable is in log differenced form. Matching estimates computed with Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity (all specifications) and intragroup correlation (OLS only)-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Cluster sizes are reported for home and host, respectively. Fixed effects for home, host, and pair are period-specific. Core bank and country covariates are listed in the Appendix. All bank and country-level covariates enter with their values set in the pre-crisis period ($t = 2006$).

several dimensions, and in this subsection we report results for two dimensions considered: geographical region, and ownership structure. Our empirical strategy in this regard is straightforward; we add an interaction term to our difference-in-difference setup:

$$\Delta l_{ijk} = \beta'''' + \tilde{\delta}_0 crisis_k + \sigma state_i + \tilde{\delta}_1 \left(crisis_k \cdot state_i \right) + \alpha_j'''' + \alpha_k'''' + \varepsilon_{ijk}'''', \tag{6}$$

where $state$ distinguishes various dimensions along which a given bank i can differ from another. $\tilde{\delta}_1$ is now our coefficient of interest. To avoid overfitting the model and to assist in our interpretation of $\tilde{\delta}_1$, our specification builds on the relatively parsimonious augmented difference-in-difference specification (2) with period-specific country fixed effects.

These results are reported in Table 9. The left panel includes the triple interaction on six developing-country geographical regions (as defined by the World Bank), while the right panel includes ownership in terms of whether the banks were publicly listed, and whether they were government-owned.

The first observation we make about these results is that the only significant $\tilde{\delta}_1$ estimate applies to Eastern Europe, and this point estimate is extremely large. This is consistent with findings in the literature that the region was especially hard-hit by the crisis (Claessens et al., 2010), and suggests that the crisis, as experienced in Eastern Europe, was such that foreign banks there that faced home-country crises tended to contract their lending more than those in the rest of the developing world, on average.

Note that the insignificant coefficient on the uninteracted crisis term for Eastern Europe and Central Asia is no real cause for concern. The total effect of the crisis has to be inferred from the sum of both the uninteracted and the interaction term, and if we treat statistically insignificant coefficients as equal to zero, the total effect for all cases remains significantly negative.[42]

Second, there also appears to be no significant influence of ownership structure in terms of crisis treatment: both publicly listed and government-owned banks in crisis treatment economies had loan outcomes indistinguishable from privately-held banks. Thus ownership structure does not appear to be a significant source of variation in our data.[43]

[42] Another possible interpretation of the insignificant independent crisis effect in the ECA specification is that only ECA banks are responsible for our results; that is, the crisis effect would not be significant if ECA banks, which were especially hard hit by the crisis, were not included in our sample. To rule out this possibility (as well as the possibility that selected regional subsamples may be giving rise to our overall crisis effect), we ran regressions using our baseline specification that systematically excluded one region at a time from the sample. The results, reported in Table A.9 of the Appendix, generally hold up to this selective exclusion, indicating that the crisis effect is not due to the lending behavior of foreign banks in any one region.

[43] We would, however, caution against excessive inference in this regard. As is the case for certain regions (notably EAP and SAS), the number of banks that are publicly-listed or government-owned is fairly small—70 and 24, respectively—which would serve to limit statistical power.

Table 9. Difference-in-Difference Regressions for Bank Lending with Additional Interactions, 2006 and 2009

	S1	S2	S3	S4	S5	S6	S7	S8
	Regions						*Ownership*	
Crisis effect	-0.425	0.010	-0.447	-0.621	-0.467	-0.515	-0.544	-0.355
	(0.08)***	(0.37)	(0.07)***	(0.08)**	(0.07)**	(0.07)***	(0.28)*	(0.21)*
Crisis × EAP	-0.304							
	(0.50)							
Crisis × ECA		-1.560						
		(0.61)**						
Crisis × LAC			0.551					
			(0.63)					
Crisis × MNA				0.549				
				(0.43)				
Crisis × SAS					0.301			
					(0.42)			
Crisis × SSA						0.562		
						(0.43)		
Crisis × Pub. List.							0.273	
							(0.38)	
Crisis × Govt.								-0.552
								(0.55)
Fixed effects								
Home	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Host	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.491	0.505	0.493	0.492	0.491	0.492	0.491	0.4948
Clusters (countries)	66, 51	66, 51	66, 51	66, 51	66, 51	66, 51	66, 51	66, 51
Estimation	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
N (banks)	361	361	361	361	361	361	361	1,021

The dependent variable is in log differenced form. Heteroskedasticity and intragroup correlation-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Regions correspond to World Bank regions (EAP = East Asia and Pacific; ECA = Eastern Europe and Central Asia; LAC = Latin America and Caribbean; MNA = Middle East and North Africa; SAS = South Asia; SSA = Sub-Saharan Africa). Ownership is either publicly-listed (pub. list.) or government-owned (govt.), the other group being privately-held banks. Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

8 Comparison to the 1997-98 Asian Financial Crisis

In this section we expand the scope of our baseline by considering an alternative case study, that of the 1997-98 Asian financial crisis. Although this crisis was not as global as the 2007-08 crisis, the Asian crisis—which was precipitated by the failed defense of the Thai baht in mid-1997—ultimately spread across East Asian emerging markets and, by 1998, had induced crises in other developing economies: Russia, and parts of Eastern Europe and Latin America. The scope of the crisis enables us to replicate our analytical framework outlined in Section 4.[44]

Table 10. Difference-in-Difference and Matching Difference-in-Difference Regressions for Asian Financial Crisis, 1996 and 1999

	A1	A2	A3	A4	A5	A6
Crisis effect	-0.216	-0.604	-1.479	-1.034	-0.957	-0.628
	(0.34)	(0.00)***	(1.81)	(0.32)***	(0.33)***	(0.32)**
Fixed effects/core covariates						
Home	No	Yes	Yes	Yes	Yes	Yes
Host	No	Yes	Yes	Yes	Yes	Yes
Bank†	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.007	0.774	0.896			
Clusters (countries)	24, 6	24, 6	24, 6	-	-	-
Estimation	OLS	OLS	OLS	Matching	Matching	Matching
Matches	-	-	-	2	3	4
N (banks)	32	32	32	31	31	31

The dependent variable is in log differenced form. Matching estimates computed with Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity (all specifications) and intragroup correlation (OLS only)-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank and country-level covariates enter with their values set in the pre-crisis period ($t =$ 2006). †Bank core covariates include only size and solvency.

We proceed by redefining our crisis treatment as unity when its main country of ownership experienced a banking crisis in the years 1997–1998, and zero otherwise.[45] We then repeat our exercise for the various specifications in Section 5.

[44] Importantly, we find no compelling reason to reject our key identification assumptions. For the common trends assumption, while data limitations prevent us from computing pre-crisis trends—data for the Asian crisis subsample begin only in 1996—there is no evidence of convergence in average lending between the two groups between 1996–97 (see Figure A.2 of the Appendix). Although this fact alone is not *sufficient* to verify parallel trends, its presence is certainly *necessary* if the assumption were to hold. For the exogeneity assumption (that treated subsidiaries did not induce their home-country crises), the subsidiaries of Asian crisis-stricken banks in our sample were all small relative to their home banking systems.

[45] In contrast to our baseline, where the crisis treatment overlaps almost perfectly between the coding scheme of Laeven and Valencia (2013) and Reinhart and Rogoff (2009), there is greater variation in the dating of crises in the

One major caveat is that, owing to data limitations, our sample size is drastically reduced: our baseline comprises just 32 banks, operating in only six economies. Treated banks comprise just over a fifth of the sample, and many home countries are represented by only one bank. With these caveats in mind, Table 10 reports a selection of these results, corresponding to specifications (B1), (B4), (C2), and (M2)–(M4).[46] The results are broadly in line with our baseline findings. Coefficients for the crisis effect are negative, and across many specifications, statistically significant. The magnitudes, however, are far less stable, likely owing to the small sample size. Overall, it appears that foreign banks with ownership based in countries that experienced the 1997-98 crisis also scaled back lending relatively more than those with ownership based in non-crisis economies. Given the caveats already raised, however, we view the results here as mainly providing corroborative, rather than definitive, evidence in support of our central claims.

9 Conclusion

In this paper, we examined the question of whether foreign banks whose home countries were hit by the 2007-08 financial crisis altered their lending behavior as a result of the shock. We find strong and consistent evidence that they do indeed scale back on their lending: in our baseline, by between 13 and 42 percent relative to foreign banks that did not experience such a crisis in their home countries. This result holds up to a battery of robustness checks, which include a range of controls for covariates, and falsification tests for alternative hypotheses. Consequently, we are confident that this effect is causal.

To infer from our results that developing country policymakers should close their financial markets to foreign banks would be to carry our results too far; after all, foreign banks probably carry a host of additional benefits in terms of financial stability and enhanced competition (Clarke et al., 2003). Our results apply to a very specific situation—when foreign countries are experiencing crises of their own—and it is unclear whether shutting our foreign banks altogether would necessarily outweigh the other benefits that generally accrue from maintaining a liberalized financial sector. Our results do, however, suggest that domestic monetary authorities should be aware of the potential for greater credit contraction by foreign banks under certain circumstances, and support domestic liquidity formation during such crises accordingly.

Asian case. Two countries, in particular, have crisis start dates in the latter dataset that begin in 1992 instead of either 1997 or 1998 (China and Japan). We code these as non-crisis countries.

[46] Data limitations imply that including all bank covariates for the final four specifications would have resulted in an unacceptably small sample size. Estimates for additional specifications are reported in Table A.10 of the Appendix.

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10 Appendix

A.1 Data Sources and Definitions

A.1.1 Rules for Ownership Determination in the Ownership Database

Ultimate ownership was used if:

1. The main country of foreign ownership is a tax haven, the owner(s) in the tax haven is a holding company runs the firm as a holding company and not as an operational firm,[47] and total foreign ownership is 50 percent or more;
   - If the main foreign owner(s) is a holding company located in a country classified by the OECD as a tax haven or classified by the OECD as an OECD member country with a potentially harmful preferential tax regime; whenever the direct owner is a holding company resident in one of these countries, we assume that the arrangement exists for tax purposes and uses direct ownership, except in cases when there is evidence that the owner is not merely a holding company but an operational firm in its own right;
2. Majority ownership by a holding company, which functions purely as a holding company, and which is fully owned by a third firm;
   - When a bank is majority owned by a holding company, and that holding company is not itself an operational bank and is deemed to exist purely for the purpose of ownership (according to the best judgment of the authors); and that holding company is fully owned by a parent firm; then the nationality of the holding company's parent is used.[48]
3. Transfer of a bank from its parent to another of the parent's subsidiaries for the purpose of being absorbed by that other subsidiary;
   - Ownership was transferred from a parent company to another subsidiary for the purpose of absorption by that other subsidiary. The nationality of the parent is applied to that year (the final year of the bank's existence), since the bank is in effect still directly owned by that parent at the time when it becomes part of another bank which is owned by that parent, and effectively loses its autonomy more or less at the time of transfer to the domestic sibling.
   - For example, Banca Italo Albanese (Albania) is owned by Intesa (Italy) for several years, until March 2008, when the bank is acquired by ABA, another Albanian subsidiary of Intesa, from the parent company, for the purpose of absorbing it. Banco Italo Albanese is immediately absorbed by ABA, and ceases to exist as a bank. Even though the last owner of the bank in 2008 was Albanian, 2008 ownership is recorded as Italian.

[47] In practice, it is occasionally difficult to definitively ascertain whether a given firm operates as a pure holding company or not, and so holding company status was established with reference to relevant public documentation.

[48] In most cases when this rule is applied, the ultimate owner is a large global bank with a familiar name (HSBC, Citibank/Citigroup, etc.)

A.1.2 Classification of Banking and Financial Crises

Systemic banking crises are taken from Laeven and Valencia (2013). In this dataset, a banking crisis is defined as a *systemic banking crisis* when two conditions are met:

1. Significant signs of financial distress in the banking system (as indicated by significant bank runs, losses in the banking system, and bank liquidations); and
2. Significant banking policy intervention measures in response to losses in the banking system. The definition does not include isolated banks in distress.

The year in which a systemic banking crisis *starts* is identified by the two conditions just mentioned; policy intervention is identified as significant when at least three out of the following six policy interventions have been used (Laeven and Valencia, 2013, p. 229):

- Deposit freezes and bank holidays;
- Significant bank nationalizations (treasury or central bank asset purchases exceeding 5 percent of GDP);
- Large bank restructuring gross costs (at least 3 percent of GDP);
- Extensive liquidity support (exceeds 5 percent of deposits and liabilities to nonresidents);
- Significant guarantees in place (exceeding 5 percent of GDP); or
- Significant asset purchases (at least 5 percent of GDP).

When a country has faced financial distress but fewer than three of these measures have been used, the event is classified as a crisis if one of the following two conditions has been met:

1. Country's banking system exhibits significant losses resulting in a share of nonperforming loans above 20 percent or bank closures of at least 20 percent of banking system assets;
2. Fiscal restructuring costs of the banking sector exceed 5 percent of GDP.

A.1.3 Construction of Trade Collapse and Fiscal Stimulus Treatments

The treatment variable for *trade collapse* was constructed by first compiling total trade (the sum of imports and exports) for a given economy, and computing the percentage change in total trade flows between 2006 and 2009. *Only* the economies that experienced a net decline in trade flows between the two periods were then sorted, and the threshold for what constituted a trade collapse was then defined as contractions that fell below the median (the 50th percentile) of this group. This is equivalent to a percentage decrease of total trade of -3.9 percent. By this definition, this treatment includes 66 treated banks, with 295 non-treated banks. Comparable results to that reported in the text were obtained when more stringent (e.g., the 30th percentile, or a fall of 13.9 percent) or relaxed (e.g., the 70th percentile, which implies a fall of 3.9 percent) definitions of a trade collapse were employed (these are reported in the optional tables in the Appendix).

The *fiscal stimulus* treatment is based on the dataset by Grail Research (2009), which compiles, *inter alia*, the total announced bailout amounts in U.S. dollar terms. These were then normalized by 2008 GDP from the World Development Indicators. Stimulus amounts ranged from 86 and 47 percent of GDP at the high end ($400 billion and $2.1 trillion, Saudi Arabia and China, respectively) to 0.07 and 0.04 percent of GDP at the low end ($11 billion and $85 billion, Jamaica and Romania, respectively). An economy is coded as having experienced a stimulus treatment if the stimulus amount exceeded 2.5 percent of GDP. By this definition, this treatment includes 226 treated banks, with 135 non-treated banks. Comparable results to that reported in the text were obtained when a more stringent definition of a fiscal stimulus was employed (e.g., a stimulus of 5 percent of GDP), although the treatment in this latter case comprises 162 treated banks and 199 non-treated ones.

A.2 Additional Tables

Tables A.2 and A.3 report the data sample for banks in the home and host countries, respectively. In Table A.4, we report a comparison of means (and accompanying standard errors) between crisis treatment and nontreatment foreign banks, along with the difference in the two groups, for the years 2006 and 2009. Table A.5 provides summary statistics for the main variables in the effective sample. The first three columns of Table A.6 replicates (M2)–(M4) of Table 3, but with exact matching imposed, while the next three columns reports Abadie and Imbens (2011) bias-corrected Mahalanobis matching for 10, 15, and 20 matches. Table A.7 repeats the falsification treatment in specifications (F3) and (F4) of Table 5 using the 30th and 70th percentile as cutoffs (instead of the 50th as reported in the text). Table A.8 reports additional OLS regressions for the channel variables omitted in Table 6 of the text. Table A.9 reports baseline difference-in-difference regressions—corresponding to specification (B4)—on subsamples that either exclude or include a given region.[49] Table 10 reports additional results pertaining to the Asian crisis: replicating regressions for (B2)–(B3) in Table 1, (C1) in Table 2, and (M2)–(M4) with exact matching.

[49] Naturally, small sample sizes substantially reduce our ability to draw inferences from subsamples that only include banks within a region, but these are reported for completeness.

Table A.1. Sources and Definitions for Main Variables of Interest

Variable	Definition	Source
	Variable of interest	
Loans	Stock of gross loans† less reserves for impaired loans/NPLs	Bankscope
Crisis	1 if the home country experienced a systemic banking crisis; 0 otherwise‡	Authors/Laeven and Valencia (2013)
	Core bank level covariates	
Size	Stock of total earning assets	Bankscope
Solvency	Ratio of equity to total assets (%)	Bankscope
Income to loan ratio	Net current income/Total loans (%)	Bankscope
Interest margin	Interest income on assets less expense paid on liabilities/Total assets (%)	Bankscope
Wholesale	Net loans as a percentage of customer funding (%)	Bankscope
Liquidity	Liquid assets/Total Assets (%)	Bankscope
	Core country level covariates	
GDP growth	Real GDP growth, lagged one year	WDI*
GDP per capita	GDP per capita (constant 2000 USD)	WDI
Inflation	Inflation, consumer prices (annual %)	WDI
Current account balance	Current account balance (% of GDP)	WDI
Offshore	Dummy for home country classified as offshore financial center	BIS§
	Alternative bank and additional country covariates	
Profitability	Return on average equity (%)	Bankscope
Weakness	Ratio of loan loss provisions to net interest revenue (%)	Bankscope
Trade openness	Imports plus exports (% of GDP)	WDI
Financial exports	Insurance and financial services (% of service exports, BoP)	WDI
Bank capital	Bank capital to assets ratio (%)	Beck et al. (2000)
Nonperforming loans	Ratio of banks' nonperforming loans to total gross loans (%)	Beck et al. (2000)

†Gross loans include residential mortgage, other mortgage, other consumer/retail, corporate and commercial, and other loans. ‡The construction of this variable is described in detail in the text. * WDI = World Development Indicators. §BIS = Bank of International Settlements.

Table A.2. Baseline Sample of Home Countries by Crisis and Non-Crisis Status, with Corresponding Number of Banks

Country	Banks	Country	Banks	Country	Banks
		*Crisis countries** (17 countries; 208 banks)			
Austria	10	Ireland	1	Portugal†	7
Belgium	3	Italy	6	Slovenia†	1
Denmark	1	Latvia	1	Spain	16
France†	28	Luxembourg	3	United Kingdom	46
Germany	13	Netherlands	18	United States	38
Greece	14	Nigeria	2		
		Noncrisis countries (49 countries; 153 banks)			
Argentina	4	Honduras	1	Panama‡	6
Australia	2	Hong Kong‡	2	Peru	2
Azerbaijan	1	Hungary	3	Russia	9
Bahrain‡	6	India	9	Saudi Arabia	1
Botswana	2	Indonesia	1	Singapore‡	6
Brazil	9	Israel	4	South Africa	9
Canada	8	Japan	10	Sweden	1
China	1	Jordan	1	Switzerland	4
Colombia	4	Kazakhstan	1	Thailand	1
Costa Rica	2	Kenya	4	Togo	5
Croatia	1	Korea, Rep.	2	Turkey	5
Dominican Rep.	2	Lebanon‡	2	UAE	4
Ecuador	1	Libya	4	Uruguay	3
Egypt	1	Liechtenstein	1	Uzbekistan	1
Estonia	1	Malaysia	1	Venezuela	1
Finland	1	Mauritius‡	1		
Guatemala	1	Mexico	1		

* As defined by Laeven and Valencia (2013). †Borderline banking crisis. ‡Offshore financial center.

Table A.3. Baseline Sample of Host Countries, and Corresponding Number of Foreign and Domestic Banks

Country	Foreign	Domestic	Country	Foreign	Domestic
Host Countries (51 countries; 361 foreign banks; 738 domestic banks)					
Algeria	5	3	Kenya	5	15
Angola	4	4	Lebanon	3	20
Argentina	15	41	Lithuania	5	3
Armenia	6	2	Macedonia	2	3
Belarus	4	4	Malaysia	11	22
Bolivia	4	6	Mauritius	6	3
Bosnia & Herz.	8	5	Mexico	14	19
Botswana	3	5	Moldova	2	7
Brazil	26	52	Nepal	2	10
Bulgaria	7	7	Pakistan	7	11
Cameroon	5	1	Panama	17	9
China	5	58	Paraguay	7	3
Colombia	5	6	Peru	6	5
Congo, Dem. Rep.	4	1	Romania	15	3
Costa Rica	5	34	Russia	23	168
Côte d'Ivoire	4	1	Senegal	5	1
Dominican Rep.	2	27	Sierra Leone	2	3
Ecuador	2	13	South Africa	7	19
Egypt	9	10	Tanzania	11	4
El Salvador	4	2	Tunisia	5	8
Georgia	4	2	Turkey	10	11
Guatemala	3	10	Uganda	9	1
Honduras	3	7	Uruguay	13	3
India	6	48	Venezuela	3	11
Indonesia	16	18	Zambia	6	1
Kazakhstan	6	8			

Table A.4. Student's T-Tests for Bank Lending, 2006 and 2009

	2006	2009	*Difference*
Crisis treatment	5.83	6.36	0.52
	(0.14)	(0.18)	(0.20)**
Nontreatment	4.63	5.51	0.88
	(0.17)	(0.26)	(0.27)***
Difference	1.20	0.85	-0.36
	(0.30)***	(0.26)***	(0.14)**

Means are for bank lending in log form. Standard errors are reported in parentheses and are estimated by linear regression with clustering by host country. Differences are calculated as that between 2009 (treatment) and 2006 (nontreatment). * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

Table A.5. Summary Statistics for Main Variables of Interest

Variable	**N**	**Mean**	**Std. Dev.**	**Min**	**Max**
		Crisis banks			
Total loans, 2006	208	2,390.9	7,202.4	1.0	53,633.4
Total loans, 2009	208	3,362.6	9,211.8	0.0	68,622.9
Size	208	6.8	1.8	1.5	11.2
Solvency	208	1,449.3	1,254.5	104.0	9,394.0
Income-to-loan	208	0.4	3.6	-0.2	44.0
Interest margin	208	579.0	355.8	-921.0	2,240.0
GDP per capita	208	36,078.5	9,768.3	831.8	83,575.9
Lagged GDP growth	208	2.5	1.1	0.7	10.6
Inflation	208	2.5	1.0	1.2	8.2
Current account balance	208	-2.2	6.6	-22.7	25.1
		Noncrisis banks			
Total loans, 2006	153	651.1	1,495.4	0.0	7,736.1
Total loans, 2009	153	1,017.0	2,049.0	2.0	12,925.5
Size	153	5.7	1.8	0.0	9.9
Solvency	153	1,820.8	1,578.0	-1257.0	8,031.0
Income-to-loan	153	-3.4	43.0	-532.0	4.1
Interest margin	153	590.4	485.1	-114.0	4,085.0
GDP per capita	153	14,435.9	16,624.1	387.1	113,469.8
Lagged GDP growth	153	5.6	3.1	1.0	26.4
Inflation	145	4.8	3.8	0.2	14.5
Current account balance	147	3.6	10.4	-15.4	39.2

All variables are for 2006, unless otherwise stated.

Table A.6. Matching Difference-in-Difference Regressions Using Exact Matching for 2–4 Matches, and Mahalanobis Matching for 10–20 Matches, 2006 and 2009

	A.M1	**A.M2**	**A.M3**	**A.M4**	**A.M5**	**A.M6**
Crisis effect	-0.136	-0.145	-0.183	-0.396	-0.394	-0.386
	(0.08)*	(0.08)*	(0.08)**	(0.10)***	(0.10)***	(0.09)***
Core host covariates				Yes	Yes	Yes
Core home covariates				Yes	Yes	Yes
Core bank covariates	Yes	Yes	Yes	Yes	Yes	Yes
Exact host matching	Yes	Yes	Yes	No	No	No
Exact matches (%)	58.5	51.4	46.0	-	-	-
Estimation	Matching	Matching	Matching	Matching	Matching	Matching
Matches	2	3	4	10	15	20
N (banks)	343	343	343	328	328	328

The dependent variable is in log differenced form. Point estimates computed from matching DiD, with replacement, based on the Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity-robust standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank and country-level covariates enter with their values set in the pre-crisis period ($t = 2006$).

Table A.7. Falsification Tests for Linear Difference-in-Difference Regressions for Bank Lending with Alternative Definitions of the Trade Treatment, 2006 and 2009

	A.F1	**A.F2**	**A.F3**	**A.F4**
	$treat = trade_{30p}$		$treat = trade_{70p}$	
Crisis effect	0.025	-0.071	0.889	0.435
	(0.17)	(0.27)	(0.33)***	(0.74)
Fixed effects/core covariates				
Home	Yes	Yes	Yes	Yes
Host	Yes	Yes	Yes	Yes
Bank	No	Yes	No	Yes
Adj. R^2	0.490	0.558	0.490	0.558
Clusters (countries)	66, 51	49, 42	66, 51	66, 51
Estimation	OLS	OLS	OLS	OLS
N (banks)	361	361	361	361

The dependent variable is in log differenced form. Heteroskedasticity and intra-group correlation-robust standard errors with two-way clustering are reported in parentheses. All bank-level covariates enter with their values set in the pre-crisis period ($t = 2006$). A constant term was included in the regressions, but not reported. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

Table A.8. Additional OLS Regressions for Change in Bank Loan-to-Asset Ratio with Changes in Channel Variables, 2006 and 2009

	A.X1	A.X2	A.X3	A.X4	A.X5	A.X6
Δ Liquidity	-0.533	-0.594			-0.547	-0.607
	(0.06)***	(0.07)***			(0.07)***	(0.08)***
	(0.07)***	(0.09)***			(0.07)***	(0.10)***
	(0.06)***	(0.08)***			(0.07)***	(0.09)***
Δ Solvency			-0.242	-0.273	-0.303	-0.308
			(0.22)	(0.22)	(0.20)	(0.19)
			(0.16)	(0.19)	(0.17)*	(0.22)
			(0.21)	(0.22)	(0.21)	(0.21)
Fixed effects						
Home	Yes	No	Yes	No	Yes	No
Host	No	Yes	No	Yes	No	Yes
Adj. R^2	0.420	0.665	0.039	0.363	0.451	0.679
Clusters (countries)	17, 51	17, 51	17, 51	17, 51	17, 51	17, 51
N (banks)	194	194	208	208	194	194

The dependent and channel variables are in differenced form. Heteroskedasticity and intra-group correlation-robust standard errors are reported in parentheses; the rows correspond to standard errors: (1) clustered by home country; (2) clustered by host country; (3) with two-way clustering. A constant term was included in the regressions, but not reported. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Fixed effects for home and host are period-specific. Cluster sizes are reported for home and host, respectively.

Table A.9. Baseline Difference-in-Difference Regressions for Bank Lending, 2006 and 2009, Regional Subsamples

	A.S1	**A.S2**	**A.S3**	**A.S4**	**A.S5**	**A.S6**
Crisis effect	-0.435	-0.386	-0.352	-0.468	-0.411	-0.574
	(0.17)***	(0.22)**	(0.27)	(0.20)**	(0.16)***	(0.20)***
Fixed effects						
Home	Yes	Yes	Yes	Yes	Yes	Yes
Host	Yes	Yes	Yes	Yes	Yes	Yes
Subsample						
Excluding	EAP	ECA	LAC	MNA	SAS	SSA
Adj. R^2	0.499	0.516	0.531	0.493	0.531	0.410
Clusters (countries)	59, 48	51, 39	51, 35	64, 47	65, 48	59, 38
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
N (banks)	329	269	232	339	346	290
	A.S7	**A.S8**	**A.S9**	**A.S10**	**A.S11**	**A.S12**
Crisis effect	-1.482	-0.296	-0.630	-0.104	-2.735	-0.373
	(1.76)	(0.35)	(0.32)**	(0.00)***	(0.00)***	(0.45)
Fixed effects						
Home	Yes	Yes	Yes	Yes	Yes	Yes
Host	Yes	Yes	Yes	Yes	Yes	Yes
Subsample						
Including	EAP	ECA	LAC	MNA	SAS	SSA
Adj. R^2	0.508	0.561	0.404	0.644	0.374	0.792
Clusters (countries)	16, 3	27, 12	27, 16	11, 4	9, 3	16, 13
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
N (banks)	32	92	129	22	15	71

The dependent variable is in log differenced form. Heteroskedasticity and intragroup correlation-robust standard errors with two-way clustering reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Regions correspond to World Bank regions (EAP = East Asia and Pacific; ECA = Eastern Europe and Central Asia; LAC = Latin America and Caribbean; MNA = Middle East and North Africa; SAS = South Asia; SSA = Sub-Saharan Africa). Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively. Subsamples are defined by the exclusion of all banks within a given region, or the inclusion of only banks within a given region.

Table A.10. Additional Difference-in-Difference and Matching Difference-in-Difference Regressions for Asian Financial Crisis, 1996 and 1999

	A.A1	**A.A2**	**A.A3**	**A.A4**	**A.A5**	**A.A6**
Crisis effect	-0.755	-0.604	-0.196	-1.557	-0.674	-0.662
	(0.00)***	(0.30)	(3.24)	(0.36)**	(0.31)**	(0.35)
Fixed effects/core covariates						
Home	No	Yes	Yes	Yes	Yes	Yes
Host	Yes	No	Yes	Yes	Yes	Yes
Bank†	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.007	0.774	0.849			
Clusters (countries)	24, 6	24, 6	24, 6	-	-	-
Estimation	OLS	OLS	OLS	Matching	Matching	Matching
Matches	-	-	-	2	3	4
N (banks)	32	32	32	31	31	31

The dependent variable is in log differenced form. Matching estimates computed with Mahalanobis metric and are Abadie and Imbens (2011) bias-corrected. Heteroskedasticity (all specifications) and intragroup correlation (OLS only)-robust standard errors with two-way clustering reported in parentheses. Heteroskedasticity-robust standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank- and country-level covariates enter with their values set in the pre-crisis period ($t = 2006$). †Bank core covariates include only size and solvency.

A.3 Additional Figures

In Figure A.1, we replicate Figure 1, but without normalization for the year 2000, thus presenting trends in average gross loans disaggregated by crisis treatment and non-treatment foreign banks for the period 2003–2010 and denominated in millions of U.S. dollars (the crisis period is demarcated as 2007–2008). In Figure A.2, we replicate Figure 1, but for the Asian crisis, presenting trends in average gross loans for the period 1996–2000 denominated in millions of U.S. dollars (the crisis period is demarcated as 1997–1998). Although the short time period prior to the crisis makes definitive inference challenging, there is insufficient evidence to reject the possibility that the two groups experienced parallel trends (note that the figures were produced using *only* banks with data for all years between 2003–2010 and 1996–2000, and so is a slightly smaller subset of the data we use for our analysis).

Figure A.1. Trends in Average Gross Loans, Millions of U.S. Dollars, 2003-2010

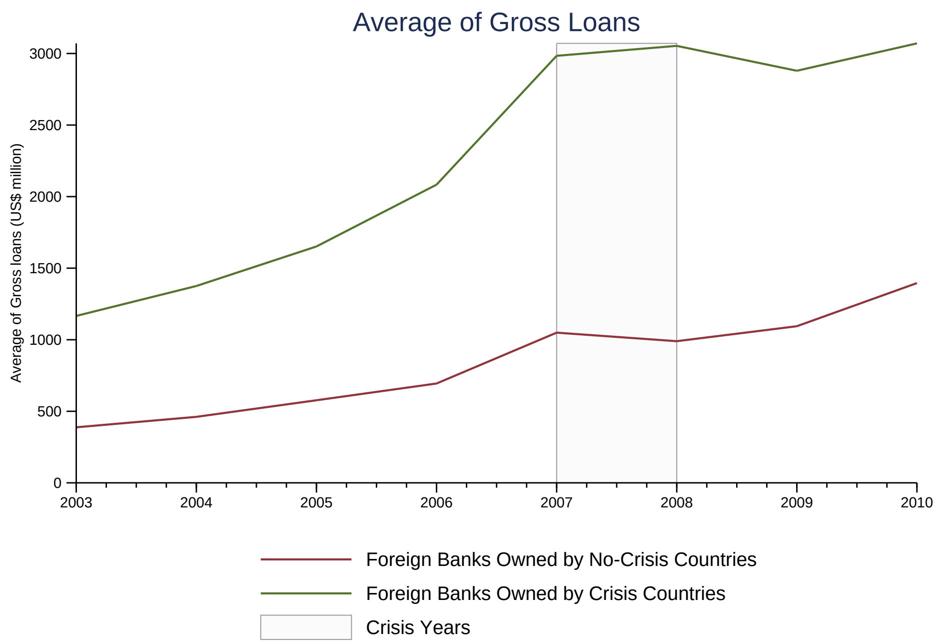

Figure A.2. Trends in Average Gross Loans, Millions of U.S. Dollars, 1996-2000