# Foreign Bank Behavior During Financial Crises

Jonathon Adams-Kane, Julián A. Caballero
& Jamus Jerome Lim*

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**Abstract**

One of the persistent policy problems faced by governments contemplating financial liberalizations is the question of whether to allow foreign banks entry into the domestic economy. This question has become ever more urgent in recent times, due to rapid financial globalization, coupled with the credit contractions experienced as a result of the 2007/08 financial crisis. This paper examines the question of whether opening the financial sector to foreign participation is a good idea for developing countries, using a unique bank-level database of foreign ownership. In particular, we examine whether the credit supply of majority foreign-owned financial institutions differ systematically conditional on a crisis event in their home economies. We show that foreign banks that were exposed to crises in their home countries exhibit changes in lending patterns that are lower by between 13 and 42 percent than their non-crisis counterparts.

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*The World Bank, the Inter-American Development Bank, and the World Bank and Santa Cruz Institute for International Economics, respectively. Contact author: Jamus Lim, MSN MC2-204, 1818 H St NW, Washington, DC 20043, USA, Email: jlim@worldbank.org. Financial support from the KCPII Window 2 Grant TF095266 "Analyzing the Impact of the Financial Crisis on International Bank Lending to Developing Countries" was critical in supporting the data collection process. The paper has benefited from comments from Samuel Berlinski, Steve Kaplan, Andrew Powell, and Jasmine Xiao. The findings, interpretations, and conclusions expressed in this article are entirely those of the authors. They do not necessarily represent the views of the World Bank or the Inter-American Development Bank, their Executive Directors, or the countries they represent.

> A banker is a fellow who lends you his umbrella when the sun is shining, but wants it back the minute it begins to rain.
>
> Mark Twain
> American author and humorist (1835–1910)

# 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 there, 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 as a result of João's actions. Clearly, the concern that foreign banks may flee when their home countries experience difficult times is neither unwarranted nor unprecedented.

Indeed, over the long course of history, governments have often weighed potential liquidity and growth benefits of foreign bank presence against fears that such banks may prove unreliable sources of capital in times of crisis. 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 seeks to contribute to the literature on 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 activity of majority foreign-owned financial institutions that experienced a crisis in their home countries differ systematically in their lending behavior relative to foreign-owned institutions that did not, within the general setting of the global financial crisis of 2007/08.

Whether foreign-owned banks choose to scale back on their lending activity in such circumstances is far from obvious. Foreign subsidiaries experiencing a crisis in their home country may choose to repatriate capital to an ailing parent bank, but it is just as plausible that parent banks reallocate asset portfolios toward markets relatively less affected by the crisis. The issue of foreign bank lending 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 own-

ership dataset collected across countries and over time, and comprises 361 foreign-owned banks based in developing countries over the course of the recent 2007/08 global financial crisis and in the immediate pre- and post-crisis years (2006 and 2009). We define our crisis "treatment" as a financial or banking crisis (Laeven and Valencia, 2013) experienced in the home country of the foreign-owned bank. 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 in the crisis economy, *foreign subsidiaries* of these banks are unlikely to have contributed to the crisis there, so that the crisis event was an "import" from high-income countries. From the perspective of these banks, then, the financial crisis was essentially an exogenous event, just as it was for other foreign banks situated within the host economy, with the crucial difference being that the former group could subsequently be subject to potential constraints resulting from the home-country crisis—such as the need to repatriate profits to their parent banks—that foreign banks *not* facing similar shocks in their home economies would not experience.

We exploit this exogenous variation to identify the effect of a home-country crisis on foreign bank lending behavior 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, and exploring various dimensions of heterogeneity in the crisis effect among the foreign banks. These complementary methodologies thus allow us to both identify the causal effect of a crisis in a foreign bank's home country on the change in the bank's lending activity before and after the crisis, as well as provide some sense of whether certain bank- or country-specific characteristics may have contributed to the estimated avaerage 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 (Clarke *et al.*, 2003; de Haas and van Lelyveld, 2010; Martínez-Peria *et al.*,

2005; Wu *et al.*, 2011), this is not the case when the crisis originated from the foreign bank's home country. Thus, rather than expanding lending in an attempt to diversify away from the shock experienced in their home countries, such banks probably 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 also find 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, and that the crisis that faced foreign banks in Eastern Europe was especially severe.

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 a crisis.[1] For example, Peek and Rosengren (1997, 2000) document a reduction in lending by Japanese banks in the U.S. after the bust of the Japanese stock market in 1990; while Chava and Purnanandam (2011) and Schnabl (2012) present evidence of negative spillovers via foreign banks of the Russian crisis of 1998 to firms in the U.S. and Peru, respectively. Similarly, de Haas and van Lelyveld (2006) study how foreign banks in Central and Eastern Europe responded to banking crises there. In the setting of the crisis of 2007/08, Galindo *et al.* (2010) document negative spillovers of foreign banks in Latin America, Popov and Udell (2012) and Ongena *et al.* (2012) in Eastern Europe, Aiyar (2012) and Rose and Wieladek (2011) in the UK, and Cetorelli and Goldberg (2012b) in the USA. In contrast to the relatively narrow geographic focus of these papers, our country coverage includes 51 developing economies, across all regions of the world.

Relatively few papers have considered the specific issue of the influence of foreign bank ownership on credit across a wider range of countries (Cetorelli and Goldberg, 2011; Claessens *et al.*, 2001; Clarke *et al.*, 2006; Detragiache *et al.*, 2008; Van Rijckeghem and Weder di Mauro, 2003). However, in these papers, foreign bank presence in an economy is typically measured at an aggregate level, rather than the bank- and home-country-specific level we employ in this paper (which permits us to map banks to home-specific shocks).

To the extent that some papers have worked with bank-level data, their bases for compar-

[1] Other studies have explored the behavior of foreign banks in developing countries during normal times, or across the business cycle. For instance, Dages *et al.* (2000), Martínez-Peria *et al.* (2005), Clarke *et al.* (2005), and Galindo *et al.* (2005) study the influence of foreign bank presence on lending patterns in Latin America, while Gormley (2010) does the same for India. Detragiache and Gupta (2006) and Khwaja and Mian (2008) examine episodes of liquidity shortages in Malaysia and Pakistan, respectively.

ison 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. Similarly, de Haas and van Lelyveld (2013) and Claessens and van Horen (2013) are concerned with benchmarking lending by foreign subsidiaries of multinational banks against that of domestic banks. Since 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 the subset of foreign-owned banks operating in developing countries, since we believe that foreign banks from non-crisis home countries offer the purest control group for our treatment of interest.

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).[2] 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. However, the paper is primarily concerned with the effect of monetary shocks experienced 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 more general developing country focus. Finally, while the concerns of the final four papers do overlap with ours, the analyses mainly rely on datasets with somewhat weaker coverage (for example, on cross-border syndicated lending), and, most importantly, are not focused on addressing causal concerns in a systematic fashion, which we regard as our central contribution.

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 4then 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. A final section concludes with policy implications and avenues for future

[2]We should note that Micco *et al.* (2007) also approach the foreign ownership issue from a bank-level perspective. But the substantive focus of their paper is different, being concerned more about bank performance, rather than credit provision.

research.

## 2 Foreign and domestic bank lending in 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.[3] 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 reallocation to other subsidiary banks in the group that were relatively exposed to the shock (termed a "support effect" by de Haas and van Lelyveld (2010)). Similarly, when the parent encounters liquidity problems, the parent may pass these on by supplying less liquidity to the subsidiary via the group's internal capital market (Cetorelli and Goldberg, 2012a). This is analagous 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.

[3] Goldberg (2009) provides an overview of key international spillovers through banking, including some mechanisms of crisis transmission.

[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.

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 thus the net effect on its lending behavior, which is therefore ambiguous.[5] 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 these may have a political mandate to cushion the economy from shocks by lending countercyclically (Bertay *et al.*, 2012). In the context of asymmetric information, which is probably especially relevant in 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 tend to 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 vs. domestic firms.

All these differences between foreign and domestic banks also suggest that the two groups likely face different demand schedules for loans in a 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 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 this paper.

[5]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.

# 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 banking sector evolution in developing countries for the period of 1995–2010. This dataset, in turn, builds on a previous dataset compiled by Claessens *et al.* (2008), which includes data for the decade between 1995–2005 for a smaller set of developing countries (about two-thirds of the current coverage).[6]

The coverage is for 4,496 commercial banks, saving banks, cooperative banks and bank holding companies in 131 developing countries.[7] 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 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.[8]

Ultimate ownership was used when the main country of direct foreign ownership was a tax haven (classified by the OECD as a tax haven or as an OECD member country with a potentially harmful preferential tax regime), the owner(s) in the tax haven operated the firm purely as a holding company, and total foreign ownership was 50 percent or more; or when the bank was majority owned by a single owner which functions purely as a holding company, and which was fully owned by a third entity; or when a bank was transferred from its parent to another of the parent's subsidiaries for the purpose of being absorbed by that other subsidiary that year. 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.

We now turn to our definition of a banking crisis, which relies on the banking crisis

[6]The dataset has also been independently updated by Claessens and van Horen (2013), to the year 2009. We allude to some of the differences between this dataset and our own below, but the coverage is substantially similar.

[7]The observations are at legal entity level, and cover both branches and subsidiaries.

[8]Additional detail on the rules used to construct this variable is provided in the technical appendix.

database of Laeven and Valencia (2013). In this database, a banking crisis is defined as a *systemic banking crisis* when two conditions are met:[9] 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. Importantly, this definition does not include isolated banks in distress.

The starting year for a systemic banking crisis is identified by the two conditions just mentioned, along with the fulfillment of at least three out of the following six policy interventions (Laeven and Valencia, 2013, p. 4): extensive liquidity support; large bank restructuring costs; significant bank nationalizations; significant asset purchases; significant guarantees put in place; or deposit freezes and bank holidays.[10] 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." With the methodology just described, Laeven and Valencia (2013) identify 147 crises in 115 countries for the period 1973–2009. Of these crises, thirteen events are classified as borderline.

We combine these ownership and banking crisis variables to construct our crisis treatment effect, which is our main independent variable of interest. We define the *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 2007–2009, 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-hit countries.[12] The baseline definition yields a total of 17 systemic banking crises.

To enhance the quality of the data, we refine our working sample in several additional ways. First, we consider only host countries where there is at least one operating bank from

[9] A detailed description of the construction of this variable is provided in the technical appendix.

[10] When a country has faced financial distress but less than three of these measures have been used, the event is nevertheless classified as a crisis if either the country's banking system exhibits significant losses due to nonperforming loans or bank closures, or if fiscal restructuring costs of the banking sector exceed five percent of GDP.

[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 nonborderline systemic crises, such as Austria, Germany, Italy, Spain and UK. In robustness checks, we show that our baseline results hold after including all borderline crises.

[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.

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. 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 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 the 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. Additional bank-level variables—such as liquidity, reliance on wholesale funding, weakness as measured by loan loss provisions, and profitability—are treated as non-core bank covariates (either because they capture analogous concepts to the core variables, or suffer from weaker data availability).

The core set of 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); and a dummy for offshore financial center, as identified by the Bank of International Settlements. 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, along with summary statistics for the main variables of interest, are provided in the appendix.

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

### 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 between foreign banks with home countries that experienced a crisis relative to those with non-crisis home countries of ownership.

We begin by documenting the fact that domestic and foreign banks in developing countries exhibit systematically different lending behavior (*Fact 1*). For 2006, outstanding loans of an average domestic bank in developing countries amounted to $5 billion, while the average foreign bank had lent a third that amount, $1.7 billion (all measured in current U.S. dollars). The variation within each of the two groups is also substantial: the standard error of the mean for domestic banks in 2006 was $1.1 billion, compared to $295 million for foreign banks.[14]

These differences are statistically significant: the test statistic for differences in the means, assuming unequal variances in the unpaired data, is $t = 2.99\,(p = 0.001)$, while the variance ratio test yields $F = 27.50\,(p = 0.000)$, for 2006, and statistically significant differences hold for 2009 as well (we discuss theoretical reasons why this discrepancy between the two groups may exist in section 2).[15]

The second feature of our data is that 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 2*); in 2006, the mean for loans from the former group were $2.3 billion, versus $540 million for the latter.[16] 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.

The third element in the data that deserves mention is the fact that lending by both groups of foreign banks essentially followed the same trend up through the eve of the crisis (Figure 1): the 3-year change in (log) average lending between 2004–06 is statistically indistinguishable between crisis and non-crisis banks. Just as important, the trend in lending among just the

[14] Equivalent figures for the mean (standard error) for 2009 were $9.6 billion ($2.0 billion) and $2.4 billion ($380 million), respectively.

[15] These statistical differences persist even in the log-transformed data that we use for our econometric analysis; in 2006, for instance, the analogous two-group $t$ and $F$ tests are $t = 2.07\,(p = 0.019)$ and $F = 1.19\,(p = 0.03)$.

[16] A matrix summarizing the means, dispersions, and differences for crisis-treated and nontreated foreign banks is provided in the appendix.

*non*-crisis banks remains unchanged following the crisis[17] (*Fact 3*).

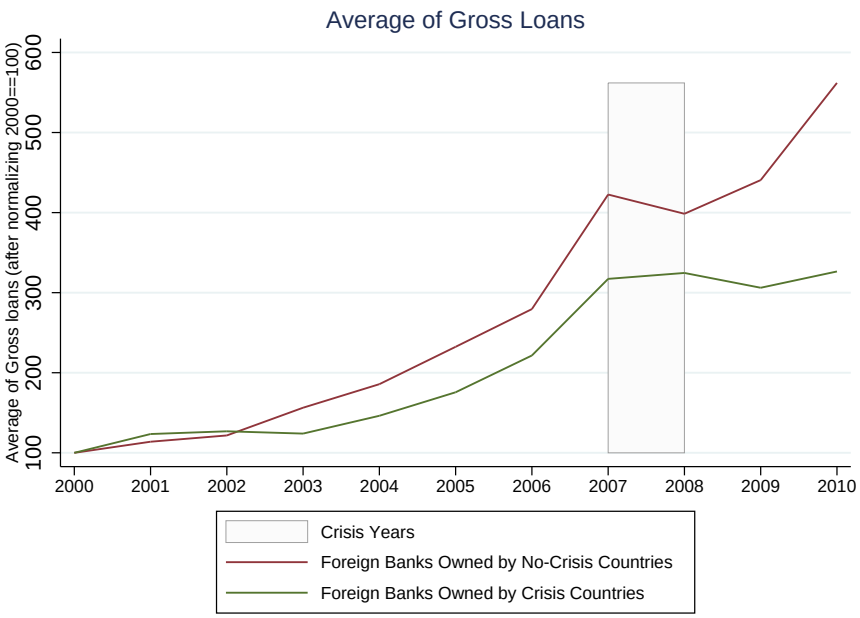


Figure 1: Trends in average gross loans, disaggregated by crisis treatment and nontreatment foreign banks, 2000–10. For comparison purposes, average loans for both groups are normalized to 100 for 2000. The crisis period is demarcated as 2007–08. Similar rising trends for both groups are evident until 2007, and the divergence in trends following 2008 is striking.

Taken together, the second and third 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 2*), while the coincident pre-crisis trends (and stable pre- and post-crisis trend for the control group) captured by *Fact 3* point to the fact that our two groups in question would likely have shared parallel trends in the absence of a crisis (the common support 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% ($972 million from $2.4 billion), whereas banks that did not experience crises increased their average post-crisis lending by 56% ($366 million from $651 million). Thus, the recovery in lending for noncrisis foreign banks was far sharper than that of crisis-stricken foreign banks; the difference in this

[17] The 3-year change in (log) average lending between 2004–06 and 2008–10 are statistically indistinguishable for non-crisis banks.

change in lending patterns between the two groups is large and statistically significant, and constitutes our stylized *Fact 4*.[18]

# 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 $j$ at time $t = \{2006, 2009\}$. Each foreign bank $i$ also has as an attribute its home country of ownership $k$, $crisis_k$ is an indicator variable that takes on the value of 1 when country $k$ experiences a systemic banking crisis in 2008 (the crisis treatment), and $post_t$ is an indicator variable that takes the value 1 if $t = 2009$ (the post-crisis period). $\epsilon$ 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 $\Delta$ 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. In principle, if the treatment is randomly assigned, estimates of this coefficient will be identified. Of course, this identification of the treatment effect also hinges crucially on our common support assumption (Imbens and Wooldridge, 2009). As shown in subsection 3.2, however, this assumption is fulfilled in our 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

[18] These results are also tabulated formally in the appendix.

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. By a similar token, 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 $\alpha_i$ captures a bank-specific effect, and $\alpha_j$ and $\alpha_k$ represent country effects for the *host* and *home* countries, respectively. Note that we have allowed for time-varying country effects ($\gamma_2$ and $\gamma_3$), but have constrained the coefficient on bank effects to be constant across the two periods.[19] The above specification can be rewritten

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

Since the bank fixed effect $\alpha_i$ is time-invariant, it drops out of the first-differenced specification.[20] To the extent that accounting for time-varying bank effects can further improve the efficiency of our estimate of $\delta'$ (and possibly account for omitted sources of bank-specific trends that may lead to violations of the common support assumption), 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 time-varying 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

[19] 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.

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

so. There is the possibility that introducing additional covariates may lead to a violation, rather than a strengthening, of our common support assumption.[21] In addition, introducing time-varying 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 against nontreated banks that share very similar observable characteristics—a matching difference-in-difference (matching DiD) specification—can further improve the quality of our estimate of $\delta$ (Abadie and Imbens, 2006).[22] 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 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 *unconfoundness*, which requires, conditional on covariates, that there be no unobservables associated with both the treatment and with the potential outcomes (Imbens and Wooldridge,

[21] This would result, for instance, if banks are relatively homogeneous within each group, so that adding additional covariates leads to a weakened likelihood that the groups maintain parallel trends.

[22] 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 matching 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).

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 that the crisis treatment satisfies the exclusion restriction. We make this case in three steps. First, we assert 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.

Second, only certain home—and, typically, high-income—countries underwent a financial crisis in 2008, 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 with regard to 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.

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.

# 5 Empirical Results

## 5.1 Baseline difference-in-differences

Our baseline results are reported 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.[23] 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 adequately capture heterogeneity in changing host country conditions. For instance, foreign banks generally extend loans in domestic currency, and since our dependent variable is measured in nominal U.S. dollars, the conversion into a common numeraire currency may introduce distortions into our estimate of the treatment effect. In theory, there is no reason why this should be a major concern. Consider, for example, the case of a host country facing high rates of inflation. Even if exchange rates do not efficiently correct for the inflation differential, foreign banks experiencing the crisis treatment are just as likely as non-treatment foreign banks, *ex ante*, to locate in countries with high or low inflation, and thus would introduce no bias to the residual.

Of course, systematic country-level differences need not be limited to inflation, but could include all manner of idiosyncratic country-level shocks (resource-rich countries experiencing a

[23] 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. Nevertheless, since the decision typically involves a tradeoff between robustness and efficiency, we report all three possible combinations of clustering in the baseline results.

positive terms-of-trade shock from rising commodity shocks, say). 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 the DiD strategy, as long as they do not also give rise to differences in *changes* in lending behavior 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 we obtain are unbiased (Imbens and Wooldridge, 2009).

Nevertheless, if we believe that efficiency is enhanced by allowing for time-varying country effects, it is straightforward to introduce these into the baseline setup as in equation (2). One immediate concern is the need to introduce changes to demand-side effects for lending, which can be done by adding host country fixed effects. Accordingly, column (B2) reports results when we allow for these. Doing so 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) thus reports the fullest articulation of equation (2), where we include time-varying 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.

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). To place this figure in some perspective, consider the change in lending the case that would obtain if a crisis-stricken foreign bank actually did *not* experience the crisis. If the 2006–09 change in lending for this bank was that of the average change in lending among crisis-stricken banks—of \$935 million—the bank would hypothetically have lent $\$\left[935/\left(1-0.42\right)\right] = \$1.6$ billion instead in the absence of the home-country crisis.

While this result accords well with the literature examining the influence of home crises on foreign bank behavior, it stands as a counterpoint to a larger literature that finds that foreign banks tend to be stability-enhancing in countries undergoing crises (Clarke *et al.*,

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 time varying. Cluster sizes are reported for home and host, respectively.

2003).[24]

Finally, it is worth considering what the time-varying 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]

[24] These disparate findings are easily reconciled: the larger literature has seldom examined the case where the financial crisis currently experienced in the developing country originates from abroad. de Haas and van Lelyveld (2013) conclude that multinational banks contribute to financial stability during local crisis episodes, but also increase the risk of importing instability from abroad.

[25] Admittedly, this interpretation would only be definitive if we are 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.

### 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, and there are reasonable objections to the indiscriminate inclusion of additional controls. The inclusion of covariates in our application is further complicated by the fact that such covariates may be correlated with the crisis treatment such that they violate the exogeneity assumption. Our resolution of this latter problem is to include our covariates as they are observed in the pre-crisis period ($\mathbf{B}_t = \mathbf{B}_{t+1}, t = 2006$).

Set against these potential disadvantages is the fact that including covariates allows us to capture the possibility that bank effects may vary over time, as they well could after a major shock such as a financial crisis. In Table 2, we incrementally introduce the four idiosyncratic bank controls included in the core set, along with two additional controls (wholesale funding and liquidity) (these are described in subsection 3.1). This core set is chosen to best capture important (observable) cross-bank heterogeneity that may potentially affect foreign bank lending behavior (further details of these measures are documented in the appendix).

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, even after we allow for the possibility of time-varying bank-specific effects. While the coefficient in the final specification, (C6), is not significant at conventional levels, this is likely due to the compromised sample size when data availability demands are greater due to the relatively larger number of covariates included. Overall, the results here point to post-crisis lending by crisis-stricken banks that is 26 to 57 percent lower than that of their noncrisis counterparts.

The point estimates here are also, on average, a hair larger than those in the baseline, accompanied by higher standard errors.[26] There are two potential explanations for these larger coefficients. The first possibility is that controlling for time-varying effects of these covariates yields more precise estimates of the average treatment effect, since including these additional observable controls may improve efficiency.

The second possibility is that the result may be biased due to the introduction of time-varying coefficients on covariates into the DiD design Lechner (2010). This may be a problem

[26] In the interests of space, the reported standard errors 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 | -0.296 |
| | (0.14)* | (0.26)** | (0.24)** | (0.27)* | (0.22)* | (0.25) |
| | | | *Core bank-specific characteristics* | | | |
| Size | -0.110 | 0.028 | 0.029 | 0.017 | 0.008 | 0.009 |
| | (0.11) | (0.09) | (0.09) | (0.08) | (0.09) | (0.08) |
| 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 | 0.256 |
| | | | | (0.01) | (0.04)*** | (0.04)*** |
| | | | *Additional bank-specific characteristics* | | | |
| Wholesale | | | | | 0.001 | 0.002 |
| | | | | | (0.00) | (0.00) |
| Liquidity | | | | | | 0.003 |
| | | | | | | (0.00) |
| 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 | 0.668 |
| 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 time varying. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

for all the estimates in Table 2, although the very comparable coefficients (relative to Table 1) would suggest that any such bias is small. Nevertheless, a more powerful way to control for 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 (columns (M1)–(M3)),[27] and with additional bank-specific covariates added (columns (M4)–(M6)). Since there is no agreement on an optimal number of matches that should be chosen (Imbens and Wooldridge, 2009), we present results for one, two, and four matches, for each of the two cases.[28]

Table 3: Matching difference-in-difference regressions for bank lending, with bank- and country-level controls, 2006 and 2009†

| | **M1** | **M2** | **M3** | **M4** | **M5** | **M6** |
|---|---|---|---|---|---|---|
| Crisis effect | -0.497 | -0.367 | -0.496 | -0.071 | -0.277 | -0.381 |
| | (0.13)*** | (0.13)*** | (0.11)*** | (0.12) | (0.11)*** | (0.11)*** |
| Core host covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Core home covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Core bank covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Additional bank controls | No | No | No | Yes | Yes | Yes |
| Estimation | Matching | Matching | Matching | Matching | Matching | Matching |
| Matches | 1 | 2 | 4 | 1 | 2 | 4 |
| N (banks) | 340 | 340 | 340 | 322 | 322 | 322 |

† 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. Additional bank covariates are wholesale and liquidity. All bank-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

[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] 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 mean-squared error (Abadie and Imbens, 2011). We include two matches as an intermediate case, noting that the difference between coefficient estimates for three and four matches is small. 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. These additional results are available on request.

-0.50. In the fullest articulation of the baseline model with four matches—shown in column (M6)—foreign banks exposed to a financial crisis in their home countries have changes in lending that are 38 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 noncrisis banks are pooled together—even if it were the case that they differed along a significant number of dimensions—and identification of the average crisis treatment effect relies on a more-or-less random distribution of additional characteristics across the sample. Such pooling may fail to accurately gauge the true extent of the crisis effect, to the extent that bank characteristics are correlated both with the treatment and with the error term. In contrast, the matching estimator forces the comparison to occur either with an otherwise similar (at least along observable dimensions) bank, or against a synthetic equivalent. To the extent that such matching on observables is indeed appropriate (and does not introduce any selection bias), 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.[29] These are reported in the six columns on the left panel of Table 4.

We first supplement the core set of bank covariates with two alternative bank-level controls, bank weakness and profitability.[30] These are given in columns (R1) and (R2) which build on, respectively, the DiD specification with the core bank covariates—extending specification (C4)—and the matching DiD equivalent, which extends specification (M3).

Next, we allow for the possibility of time-varying effects that operate at the country-pair

[29] Alternative permutations, such as using matching with core and additional bank controls, did not qualitatively alter our findings, and are available on request.

[30] 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 do 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 | -0.320 | -0.413 | -0.521 | -0.375 | -0.684 | -0.478 | -0.530 | -1.014 | -0.437 | -0.990 |
| | (0.19)$^{*}$ | (0.10)$^{***}$ | (0.00)$^{***}$ | (0.16)$^{**}$ | (0.14)$^{***}$ | (0.15)$^{***}$ | (0.19)$^{***}$ | (0.14)$^{***}$ | (0.18)$^{**}$ | (0.14)$^{***}$ |
| 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 | No | Yes | Yes | No | No | No | No | No | Yes |
| Bank | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Noncore covariates? | | | | | | | | | | |
| Additional country-specific | No | No | No | No | Yes | Yes | Yes | No | No | No |
| Additional bank-specific | No | No | No | No | No | No | No | No | Yes | Yes |
| Alternative bank-specific | Yes | Yes | No | No | No | Yes | No | No | No | No |
| Adj. $R^2$ | 0.670 | | 0.800 | 0.880 | | | 0.527 | | 0.561 | |
| 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) | 340 | 321 | 361 | 361 | 247 | 234 | 361 | 347 | 361 | 347 |

[†] 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 time varying. Core bank and country covariates are listed in the appendix. Additional bank covariates are wholesale and liquidity, alternative bank covariates are weakness and profitability, 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-level covariates enter with their values set in the pre-crisis period ($t = 2006$). Cluster sizes are reported for home and host, respectively.

level (as opposed to independently at the country level). More specifically, we replace the home and host country fixed effects $\alpha'_j$ and $\alpha'_k$ in (2) with a fixed effect $\alpha'_{jk}$ 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 from even our simple augmented model (2), we show the DiD results obtained both without and with core bank covariates in columns (R3) and (R4) (analogous to specifications (B4) and (C4), respectively).

Third, we exploit the additional flexibility for including covariates offered by the matching DiD estimator by adding additional country- and bank-level covariates (although with the same caveat as before that doing so may lead to significant sample size reductions, which justifies our decision not to use them in the baseline estimates). These additional covariates relate to country-level characteristics (regarding the quality of the financial system and the openness of the economy) and 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.[31] 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 additional 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, except in the two matching DiD estimates when two-year averages are considered (namely, specifications (R8) and (R10)). The coefficient estimates are bound by $[-0.32, -1.01]$, and all retain their statistical significance at the 10 percent level or lower. We suspect that the somewhat higher point estimates obtained in (R8) and (R10) may be because the matching algorithm is inflated by the relatively higher 2010 lending data among noncrisis countries,

[31] 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.

resulting in artificially strong synthetic comparators, and thereby possibly overestimating the crisis treatment effect.

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 reliably estimated. This lends confidence that even the most parsimonious DiD specification, given by equation (1), is probably sufficient for our analysis. 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 noncrisis-related trends in the two groups, or to other, distinct noncrisis shocks that occurred between 2006 and 2009 which were correlated with the crisis treatment.

Table 5: Falsification tests for difference-in-difference regressions for bank lending, 2003, 2006, and 2009[†]

| | **F1** | **F2** | **F3** | **F4** | **F5** | **F6** |
|---|---|---|---|---|---|---|
| | *t=2002, t+1=2005* | | *treatment=trade* | | *treatment=fiscal* | |
| Treatment effect | 0.077 | -0.389 | 0.889 | 0.435 | 0.517 | 0.675 |
| | (0.32) | (0.29) | (0.33)*** | (0.74) | (0.23)** | (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 | 0.516 | 0.490 | 0.558 | 0.490 | 0.558 |
| Clusters (countries) | 49, 42 | 49, 42 | 66, 51 | 66, 51 | 66, 51 | 66, 51 |
| Estimation | OLS | OLS | OLS | OLS | OLS | OLS |
| N (banks) | 265 | 264 | 361 | 361 | 316 | 316 |

[†] 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 time varying. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

Our first test alters the pre- and post-crisis dates to an earlier period; we choose 2002 and 2005 as alternative years.[32] This falsification test is designed to rule out the possibility that trends in 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 (C4), 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.[33]

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).[34] Of 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 underwent 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 (perhaps because such exporters are also Spanish-owned) vis-à-vis other banks in Mexico, then these banks will face a greater decrease in loan demand during the crisis than other banks. Thus, their lending will fall more than other banks' lending in Mexico. If this 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).

[32] We pick these two years to maximize data coverage, as data availability for most bank-level controls is quite limited for years prior to 2002.

[33] 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 three specifications for two other subsamples: first, by repeating the exercise for this smaller subsample for 2006–09, and second, by re-estimating 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). Our crisis effect actually remains significant in the reduced subsample from the first step, and the placebo remains insignificant in the second step. Taken together, these additional tests indicate that the results are not due to sample variations.

[34] 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 are distinct: a real side shock affects other economies via exports, while a financial sector one via capital 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.

To rule out this channel, the falsification test requires the construction of a new treatment variable that captures the effect of a trade collapse, replaces our crisis variable with this dummy, and relies 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 without and with core 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).

The coefficients in this case are *positive*, and in one of the two cases, statistically significant. Not only does 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 diminished its estimated effect.

The final falsification exercise that we implement turns to the possibility that fiscal stimuli, introduced as a result of the financial crisis, may have supported growth in economies receiving such a positive real shock, which subsequently allowed bank parents in these countries to systematically expand the lending activities of their developing country subsidiaries (possibly crowding out lending by banks with home economies that did not undergo stimulus). In this case, we can no longer claim that the treatment is capturing a financial crisis effect, but rather that the treatment was due to a beggar-thy-neighbor spillover from fiscal policy expansion.

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, and the positive and significant coefficients point to a bias against our estimated crisis effect operating via fiscal policy expansions.

# 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 pools all control banks in the comparison group, despite that domestic and foreign banks likely differ along important dimensions related to changes in lending outcomes during the crisis period (for example, facing different loan demand schedules).

However, much of the empirical literature (as cited in the introduction) *is* interested in distinctions between domestic and foreign bank behavior, and it is useful to expand our consideration of banks 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 with domestic banks 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 6 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.[35]

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 suggests that the post-crisis recovery in lending for foreign non-crisis banks, as compared with domestic banks, may have been larger. This also suggests

[35]The algorithm performs exact matching, or as exact as possible, on the country of operation. Because for each crisis-stricken bank there may not exist $M$ members among the noncrisis group, 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 96% when using one matching noncrisis bank to 82% when matching on four banks.

Table 6: Matching difference-in-difference regressions using exact matching for host country with expanded sample, 2006 and 2009[†]

| | **D1** | **D2** | **D3** | **D4** | **D5** | **D6** |
|---|---|---|---|---|---|---|
| Crisis effect | -0.229 (0.08)*** | -0.364 (0.07)*** | -0.334 (0.07)*** | -0.161 (0.08)** | -0.160 (0.07)** | -0.210 (0.07)*** |
| Core bank covariates | Yes | Yes | Yes | Yes | Yes | Yes |
| Noncore covariates? | | | | | | |
| Additional bank-specific | No | No | No | Yes | Yes | Yes |
| Exact host matching | Yes | Yes | Yes | Yes | Yes | Yes |
| Exact matches (%) | 95.7 | 94.5 | 87.0 | 92.8 | 91.8 | 81.8 |
| Estimation | Matching | Matching | Matching | Matching | Matching | Matching |
| Matches | 1 | 2 | 4 | 1 | 2 | 4 |
| N (banks) | 1,099 | 1,099 | 1,099 | 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. Additional bank covariates are wholesale and liquidity. All bank-level covariates enter with their values set in the pre-crisis period ($t = 2006$).

that total lending in the host countries may well have been lower in the absence of these non-crisis stricken foreign 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 noncrisis foreign banks (Claessens and van Horen, 2013; de Haas and van Lelyveld, 2013). 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 *noncrisis* countries with that of domestic banks. We assign treatment to the first group, and run similar DiD and matching DiD regressions as before.[36] The results in Table 7 verify that foreign banks from noncrisis countries *increased* their lending relatively more than domestic banks: the coefficients on the foreign non-crisis term are uniformly positive, and in a number of specifications, statistically significant. This result also helps us reconcile our results with the claim in the literature that, *on aggregate*, foreign banks may increase their

[36] The same caveats we remarked on before about 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 the our basic DiD model).

lending following a crisis (Clarke *et al.*, 2003; de Haas and van Lelyveld, 2010; Martínez-Peria *et al.*, 2005; Wu *et al.*, 2011).

### 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 investigating further 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 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:

$$\Delta l_{ijk} = \beta''' + \tilde{\delta}_0 crisis_k + \sigma state_i + \tilde{\delta}_1 \left(crisis_k \cdot state_i\right) + \alpha_j''' + \alpha_k''' + \varepsilon_{ijk}''', \tag{5}$$

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 assist in our iterpretation of $\tilde{\delta}_1$, our specification builds on the relatively parsimonous augmented difference-in-difference specification (2) with time-varying country fixed effects.

These results are reported in Table 8. The left panel reports the triple interaction on six developing-country geographical regions (as defined by the World Bank), while the right panel reports 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 the finding 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 which 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

Table 7: DiD and matching DiD regressions comparing noncrisis foreign banks ($treat = noncrisis$) with domestic banks using exact matching for host country, 2006 and 2009[†]

| | **N1** | **N2** | **N3** | **N4** | **N5** | **N6** | **N7** | **N8** |
|---|---|---|---|---|---|---|---|---|
| Crisis effect | 0.251 | 0.166 | 0.140 | 0.184 | 2.158 | 1.299 | 3.383 | 2.997 |
| | (0.12)** | (0.22) | (0.20) | (0.27) | (0.09)*** | (0.08)*** | (0.14)*** | (0.08)*** |
| Fixed effects/core covariates | | | | | | | | |
| Home | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Host | No | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bank | No | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Noncore covariates? | | | | | | | | |
| Additional bank-specific | No | No | No | Yes | No | No | No | Yes |
| Exact host matching | - | - | - | - | Yes | Yes | Yes | Yes |
| Exact matches (%) | - | - | - | - | 95.4 | 87.6 | 73.7 | 69.5 |
| Adj. $R^2$ | 0.012 | 0.395 | 0.405 | 0.452 | - | - | - | - |
| Clusters (countries) | 74, 51 | 74, 51 | 74, 51 | 74, 51 | - | - | - | - |
| Estimation | OLS | OLS | OLS | OLS | Matching | Matching | Matching | Matching |
| Matches | - | - | - | - | 1 | 2 | 4 | 4 |
| N (banks) | 891 | 891 | 891 | 827 | 891 | 891 | 891 | 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 time varying. Core bank and country covariates are listed in the appendix. Additional bank covariates are wholesale and liquidity. All bank-level covariates enter with their values set in the pre-crisis period ($t = 2006$).

Table 8: 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 time varying. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

negative.[37]

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.[38]

# 8 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 policy makers 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 questionable that the unique circumstances of the 2007/08 crisis 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, and support domestic liquidity formation during crises accordingly.

[37] 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, which are reported in the appendix, generally hold up to this selective exclusion, indicating that the crisis effect is not due to the lending behavior of foreign banks from any one region.

[38] 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.

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# Technical 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,[39] and total foreign ownership is 50% 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.[40]
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. Despite that the last owner of the bank in 2008 was Albanian, 2008 ownership is recorded as Italian.

### 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 and when at least three out of the following six policy interventions have been used (Laeven and Valencia, 2013, p. 229):

[39] 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.

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

- Deposit freezes and bank holidays;
- Significant bank nationalizations (treasury or central bank asset purchases exceeding 5% of GDP);
- Large bank restructuring gross costs (at least 3% of GDP);
- Extensive liquidity support (exceeds 5% of deposits and liabilities to nonresidents);
- Significant guarantees in place (exceeding 5% of GDP); or
- Significant asset purchases (at least 5% 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% or bank closures of at least 20% of banking system assets;
2. Fiscal restructuring costs of the banking sector exceed 5% 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%. By this definition, this treatment includes 66 treated banks, with 295 nontreated 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%) or relaxed (e.g. the 70th percentile, which implies a fall of 3.9%) 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 ($15 billion and $ 200 billion, Jamaica and Romania, respectively). An economy is coded as having experienced a stimulus treatment if the stimulus amount exceeded 2.5% of GDP. By this definition, this treatment includes 226 treated banks, with 135 nontreated 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% of GDP), although the treatment in this latter case comprises 162 treated banks and 199 nontreated ones.

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 |
| | *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[§] |
| | *Additional and alternative bank, and additional country covariates* | |
| Liquidity | Liquid assets/Total Assets (%) | Bankscope |
| Wholesale | Net loans as a percentage of customer funding (%) | Bankscope |
| 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.

## A.2 Data sample

Table A.2: Baseline sample of home countries by crisis and noncrisis 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 | | | |

## A.3 Additional tables

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. Table A.7 reports baseline difference-in-difference regressions (corresponding to specification (B4)) on subsamples that either exclude or include a given region.[41]

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.

[41] 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.5: Summary statistics for main variables of interest[†]

| **Variable** | **N** | **Mean** | **Std. Dev.** | **Min** | **Max** |
|---|---|---|---|---|---|
| | | *Crisis banks* | | | |
| Total loans, 2006 | 221 | 2,344.02 | 7,007.38 | 0.97 | 53,633.43 |
| Total loans, 2009 | 221 | 3,279.40 | 8,963.77 | 0.03 | 68,622.90 |
| Size | 221 | 6.78 | 1.76 | 0.75 | 11.17 |
| Solvency | 221 | 1,404.57 | 1,166.55 | 104.00 | 9,394.00 |
| Income-to-loan | 221 | 0.39 | 3.51 | -0.24 | 44.05 |
| Interest margin | 221 | 580.95 | 354.66 | -921.00 | 2,240.00 |
| GDP per capita | 221 | 26,494.67 | 8,865.31 | 2,166.32 | 53,628.23 |
| Lagged GDP growth | 221 | 2.56 | 1.13 | 0.68 | 10.60 |
| Inflation | 221 | 2.41 | 0.95 | 1.06 | 8.59 |
| Current account balance | 221 | -2.08 | 6.53 | -22.68 | 13.78 |
| | | *Noncrisis banks* | | | |
| Total loans, 2006 | 143 | 539.55 | 1,358.54 | 0.10 | 7,736.05 |
| Total loans, 2009 | 143 | 910.83 | 1,945.48 | 1.97 | 12,925.52 |
| Size | 143 | 5.57 | 1.63 | -0.01 | 9.88 |
| Solvency | 143 | 1,895.05 | 1,609.80 | 237.00 | 8,031.00 |
| Income-to-loan | 143 | 0.07 | 0.37 | -0.57 | 4.12 |
| Interest margin | 143 | 606.87 | 491.09 | -208.00 | 4,085.00 |
| GDP per capita | 143 | 10,668.40 | 12,149.55 | 257.40 | 39,965.86 |
| Lagged GDP growth | 143 | 5.51 | 3.29 | 0.93 | 26.40 |
| Inflation | 139 | 4.95 | 3.76 | 0.24 | 14.45 |
| Current account balance | 142 | 2.99 | 10.40 | -15.39 | 39.25 |

[†] Notes: All variables are for 2006, unless otherwise stated.

Table A.6: 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 | 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. Point estimates and heteroskedasticity and intragroup correlation-robust standard errors (reported in parentheses) with two-way clustering. 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.7: 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). 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 time varying. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively. Subsample are defined by the exclusion of all banks within a given region, or the inclusion of only banks within a given region.