JONATHON ADAMS-KANE
JULIÁN A. CABALLERO
JAMUS JEROME LIM

Foreign Bank Behavior during Financial Crises

This paper studies whether lending by foreign banks is affected by financial crises. We pair a bank-level data set of foreign ownership with information on banking crises and examine whether the credit supply of majority foreign-owned banks that underwent home-country crises differ systematically from those of other foreign banks. In contrast to the literature, our broad global coverage allows us to exploit variations *between* foreign banks; this enables us to identify an average treatment effect directly attributable to crises. Our baseline results show that banks exposed to home-country crises between 2007–08 exhibit changes in lending patterns that are lower by between 13% and 42% than their noncrisis counterparts. This finding is robust to potential alternative explanations, and also holds, though less strongly, for the 1997/98 Asian crisis.

JEL codes: F34, G01, G21
Keywords: foreign bank ownership, financial crisis, bank lending.

On April 25, 1821, then prince regent Dom João VI set sail from Brazil to Portugal in an attempt to deal with a revolution that was underway in the latter, carrying with him a large part of the deposits of the Banco do Brasil, the

The data collection process was financed by the World Bank KCPII Window 2 Grant TF095266 "Analyzing the Impact of the Financial Crisis on International Bank Lending to Developing Countries." Foreign bank ownership data used in this paper are available at: http://siteresources.worldbank.org/INTKNOWLEDGEFORCHANGE/Resources/491519-1386966650966/PRWP6590.zip. The paper has benefited from comments from the editor, two anonymous referees, Samuel Berlinski, Steve Kaplan, Andrew Powell, and Jasmine Xiao. Yueqing Jia and Mauricio Pinzón helped collecting and processing the data. 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.

Jonathon Adams-Kane *is an Economist at the International Finance and Macroeconomics Research Department, the Milken Institute* (*E-mail*: jadams-kane@milkeninstitute.org). Julián A. Caballero *is a Research Economist at the Research Department, the Inter-American Development Bank* (*E-mail*: julianc@iadb.org). Jamus Jerome Lim *is a Senior Economist at the Development Prospects Group Department, the World Bank, and a research affiliate at the Center for Analytical Finance* (*E-mail*: jlim@worldbank.org).

Received December 15, 2015; and accepted in revised form June 27, 2016.

Journal of Money, Credit and Banking, Vol. 49, No. 2–3 (March-April 2017)

colony's major financial institution. The bank, which was already in crisis as a result of its close ties with the Portuguese Crown, was left bankrupt. Dom João's actions two centuries ago are but one reminder that foreign banks' financing may flee when their home countries experience difficult times.

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

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

Whether foreign-owned banks choose to scale back on their lending activity under such circumstances is not immediately obvious. A foreign subsidiary experiencing a crisis in its home country may face a contraction in the banking group's internal capital market or may need to repatriate capital to an ailing parent bank. But it is also entirely possible that the parent bank, in a bid to enhance the group's balance sheet, reallocates its asset portfolio toward markets relatively less affected by the crisis. The issue of how foreign bank lending changes during financial crises is thus, ultimately, an empirical question.

Our empirical exploration seeks to answer this question by relying on a quasi-experimental difference-in-difference (DiD) approach. Our baseline sample draws on a unique bank ownership data set collected across countries and over time, and comprises 361 foreign-owned banks based in 51 developing countries during the recent 2007/08 global financial crisis, and in the immediate pre- and postcrisis years. We define our crisis "treatment" as a banking crisis (Laeven and Valencia, 2013) experienced in the home country of the foreign-owned bank.[1] Central to our identification strategy is the fact that while financial crises experienced in the home economy may have been closely tied to the performance of banks based there, *foreign subsidiaries* of these banks are unlikely to have contributed to the crisis itself. From the perspective of these subsidiaries, then, the crisis event was essentially exogenous, just as it was for other foreign banks in the host economy. The crucial difference lies in how the former group may subsequently be subject to constraints directly resulting from the home-country crisis, which the latter group would not face.

We exploit this exogenous variation to identify the effect of a home-country crisis on foreign bank lending in our baseline DiD specification. We further refine our

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

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.[2] 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 noncrisis mechanisms may be driving the results, examining potential channels of transmission for the crisis effect, and exploring various dimensions of heterogeneity in the crisis effect among the foreign banks. Our baseline strategy of comparing foreign banks that experienced home-country crises to foreign banks that did not, together with a thorough set of robustness checks, enables us to rigorously identify the average treatment effect. This careful identification of a causal effect of home-country crises on lending by foreign banks is a key contribution of our paper.

Our main result is that foreign banks owned by countries experiencing crises do, in fact, experience a postcrisis change in their lending that is relatively smaller—by between 13 and 42 percentage points in our baseline—compared to noncrisis 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, Martínez-Peria, Powell, and Vladovka-Hollar 2005, de Haas and van Lelyveld 2010), this is not the case when the crisis originated from the foreign bank's home country. In this case, rather than expanding lending in an attempt to diversify away from the shock experienced at home, such banks either repatriate capital to shore up the liquidity of their parents, or endure contractions in liquidity from their parents. While this point has been made previously by others (Peek and Rosengren 1997, de Haas and van Horen 2012a, Giannetti and Laeven 2012b), the general nature of this result has never been established, owing to the absence of a crisis that was experienced across a variety of treatment countries at the global level.

Our expanded coverage also offers us enough degrees of freedom to explore heterogeneity *among* foreign banks. We find new evidence suggesting that lending by noncrisis-stricken foreign banks may have helped offset reductions in postcrisis lending by crisis-stricken foreign banks and domestic banks; consequently, overall credit in a host economy need not fall during a crisis. We also verify that the crisis experienced by foreign banks in Eastern Europe was especially severe (Claessens et al. 2010). Finally, although our evidence in favor of a negative crisis effect is somewhat weaker for the Asian crisis, our findings for this alternative case broadly corroborate those in our baseline.

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

2. The caveat here is that we rely on observables to ensure a "good enough" match, which need not be the case, since matching is seldom perfect. We thank an anonymous referee for prompting us to clarify this point.

the U.S. after the bust of the Japanese stock market in 1990, while Schnabl (2012) presents evidence of negative spillovers via foreign banks of the Russian crisis of 1998 to firms in Peru. Aiyar (2012) considers UK banking activity during the crisis of 2007/08, and Cetorelli and Goldberg (2012b) do the same for U.S. banks.[3] What is common among these papers is that they have been limited to a single country and/or crisis episode. The upshot is that it has been difficult to confidently distinguish idiosyncratic results from insights of a more general nature. In contrast, our study—which examines two distinct crisis episodes and includes economies spread across all regions of the world—offers the opportunity to draw broader implications regarding the causal effects of crises on foreign bank lending.

A number of papers have considered the influence of foreign bank ownership on credit across a wider range of countries (Martínez-Peria, Powell, and Vladovka-Hollar 2005, Cetorelli and Goldberg 2011). However, in these papers, foreign bank presence in an economy is typically measured at an aggregate level, rather than the bank level we employ.

To the extent that some papers have worked with bank-level data, their bases for comparison have been different: de Haas and van Lelyveld (2010), for example, restrict their analysis to only subsidiaries of the 45 largest multinational banks, while Galindo, Izquierdo, and Rojas-Suarez (2013) focus on Latin American host countries, and de Haas and van Lelyveld (2006) and Popov and Udell (2012) focus on Eastern European countries. By a similar token, Cull and Martínez-Peria (2013), Claessens and van Horen (2014), and de Haas and van Lelyveld (2014) are concerned with benchmarking lending by foreign subsidiaries of multinational banks against that of domestic banks; while Ongena, Peydró Alcalde, and Van Horen (2015) compare the lending of foreign-owned and internationally borrowing domestic banks with that of locally funded domestic ones. Because we are interested in the effects of a crisis in the home country on lending activity by foreign banks, our study necessarily restricts itself to only foreign-owned banks, since we believe that doing so helps us isolate our treatment effect against the most appropriate control group. While this approach means that we do not consider certain heterogeneities among banks—such as distinguishing between bank branches and subsidiaries, or a fuller universe of banks that includes smaller institutions—our mapping of foreign banks to their home countries allows us to establish a convincing causal narrative involving home-specific shocks.

The papers that are closest in approach to this paper are three sets of papers, by Peek and Rosengren (1997, 2000), de Haas and van Horen (2012a, 2012b), and Giannetti and Laeven (2012b, 2012a). The seminal papers by Peek and Rosengren (1997, 2000) study how shocks experienced in foreign banks' home countries affect lending in host economies, but the papers are essentially case studies of a pair of major developed economies (Japanese banks in the United States), as opposed to our

3. Other studies have explored the behavior of foreign banks in developing countries during normal times or across the business cycle (Claessens, Demirgüç-Kunt, and Huizinga 2001, Clarke et al. 2005, Khwaja and Mian 2008, Gormley 2010), or their reaction to monetary shocks in the host economy (Wu, Luca, and Jeon 2011).

broader developing country focus. While the concerns of the papers by de Haas and van Horen and Giannetti and Laeven do overlap with ours in considering foreign ownership at the bank level across emerging economies—and whether such banks respond differentially to exogenous shocks—the analyses mainly rely on data sets with a limited scope: on cross-border syndicated lending only, rather than overall lending.[4] Methodologically, our paper also differs considerably from these papers, as we depart from identification strategies based on fixed effects models pioneered by Khwaja and Mian (2008) and rely instead on a research design that identifies causal effects based on a DiD approach paired with a matching estimator.[5] To our knowledge, there is no other paper that adopts our approach to addressing endogeneity concerns in identifying causal effects, while rivaling the multiregion, multiple-event scope of our analysis, which helps alleviate external validity concerns typical of more narrowly defined case studies.

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 data set and its main stylized features of banks during the financial crisis of 2007/08 (Section 2). Section 3 then outlines our econometric setup, along with a discussion of identification issues. Our baseline results are reported in Section 4, and robustness checks in Section 5. In Section 6, we explore heterogeneity among banks, for foreign relative to domestic banks, and between foreign banks. Section 7 extends our basic framework to the 1997/98 Asian crisis. A final section concludes with policy implications and avenues for future research.

1. BANK LENDING DURING A FINANCIAL CRISIS

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

There is no single, well-established theory of how foreign banks' characteristics or lending decisions can be expected to differ systematically from those of domestically owned banks, nor of how a crisis in a foreign bank's home country can be expected to influence its lending. However, some mechanisms by which shocks are transmitted through international banking have been discussed in the literature. One important consideration is that subsidiary banks—whether they are part of a multinational

4. Syndicated loans, for example, are extended only to large corporate entities; consequently, the findings pertaining to lending may not apply to small business or household lending. That said, the nature of their data allows these authors to better control for the demand side using borrower fixed effects. Nevertheless, while the detailed loan-level data in these papers are able to show convincingly that banks transmitted funding shocks across borders during the 2007/08 crisis, it remains unclear to what extent this reduction in lending can be directly attributable to the crisis *in the home country* (rather than generally difficult crisis conditions), which is our concern here.

5. A recently released paper by De Haas et al. (2015) applies very similar estimation strategies (DiD and propensity score matching) to our exercise here. However, the coverage is limited to Europe, and the substantive scope of the paper is on the role of the Vienna Initiative, rather than effects of, and channels of transmission for, home-country shocks.

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 with regard to the transmission of a shock within the group, as emphasized by Morgan, Rime, and Strahan (2004).[6]

On the one hand, when a foreign-owned bank is hit by a crisis in its parent's home country, the shock in the home country may be cushioned by repatriation of capital from the bank to its parent, or by capital reallocations from other subsidiary banks within the group that were relatively less exposed to the shock (this has been termed the "flight home effect" by Giannetti and Laeven 2012b and "support effect" by de Haas and van Lelyveld 2010). Similarly, when the parent faces liquidity problems, the parent may pass these on by supplying less liquidity to the subsidiary via the group's internal capital market (Cetorelli and Goldberg 2012a).[7] This is analogous to a wealth or income effect.

On the other hand, when the parent bank faces a less favorable risk-return trade-off 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. This "diversification effect" differs from the "flight to quality" between borrowers possessing different levels of creditworthiness, which has been emphasized more in the literature (de Haas and van Horen 2012b). It is, instead, a distinct channel that works in direct opposition to the support effect. By design, our paper is concerned with understanding the *net effect* of flight home versus diversification, rather than contrasting the former to flight-to-quality considerations (as has been done in the literature).

Which of these effects dominates determines the net effect of a crisis in the home country on a foreign bank's access to liquidity and/or its solvency position, and thus the net effect on its lending behavior is ambiguous.[8] The answer to this question is ultimately empirical.

One strategy for addressing this question is to compare the lending behavior of foreign banks that face a crisis in their home countries to the lending behavior of domestic banks, and infer whether there are any systematic differences between postcrisis lending activity in the two groups. But this could be problematic, since

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

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

8. 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.

foreign banks likely differ from domestic banks in systematic ways in terms of their precrisis characteristics, which could also make a difference in terms of how a crisis affects their lending.

For example, some domestic banks are state owned, and such banks may have a political mandate to cushion the economy from shocks by lending countercyclically (Brei and Schclarek 2015). In the context of asymmetric information, which is especially relevant for developing countries, foreign banks may also face greater costs of acquiring information about borrowers, potentially leading to "cherry picking" the most attractive, or largest, clients (Dell'Ariccia and Marquez 2004, Detragiache, Tressel, and Gupta 2008). Domestic and foreign banks may also differ in size (as shown in Subsection 2.2), capital structure, sources of funding, pursuit of longer client relationships versus "transaction-by-transaction" lending, and degree of lending to foreign versus domestic firms (see, e.g., Clarke et al. 2003).

All these differences between foreign and domestic banks also suggest that the two groups likely face different demand schedules for loans in any given host economy. Thus, there is little reason to expect that lending by domestic and foreign banks in developing countries would have shared similar trends 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 *noncrisis* home countries of ownership—rather than domestic banks. This is the crux of the empirical strategy that we adopt in the paper.

2. FOREIGN BANKS AND THE 2007/08 FINANCIAL CRISIS

2.1 Data Source and Description

The data set used in this paper is based on an extensive data collection effort on the evolution of bank ownership in developing countries for the period of 1995–2010. This data set, in turn, builds on a data set compiled by Claessens et al. (2008), which spans 1995–2005 for a smaller set of developing countries (about two thirds of the current coverage).[9]

The full coverage is for 4,496 commercial banks, saving banks, cooperative banks, and bank holding companies in 131 developing countries. The information sources used to build the data set include *Bankscope* (the primary source),[10] supplemented by individual banks' websites and annual reports, banking regulation agencies' publications and announcements, parent companies' reports, and news articles.

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

10. Observations are at the legal entity level. Since the distinction between branches and subsidiaries may vary between countries (Cerutti, Dell'Ariccia, and Martínez Peria 2007), we refrain from drawing this distinction. Moreover, in certain developing countries (such as Argentina, Colombia, and Peru), both branches and subsidiaries are subject to similar regulatory requirements. Thus, while we recognize the limitations of *Bankscope* data insofar as branch-subsidiary taxonomies are concerned, there is little reason to believe that such definitional differences can (or should) be systematically addressed. Indeed, we would argue that imposing the distinction may risk introducing bias due to measurement error.

A bank is defined as foreign owned if 50% 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.[11] If the majority of shares of a bank are held by foreigners but no single nationality accounts for a majority, then the foreign country with the highest share is considered the nationality of ownership.

Our definition of a banking crisis relies on the database of Laeven and Valencia (2013), in which a crisis is identified as a *systemic banking crisis* when two conditions are met:[12] first, there are significant signs of financial distress in the banking system (as indicated by significant bank runs, losses in the banking system, and/or bank liquidations); and second, significant banking policy intervention measures were undertaken in response to losses in the banking system. Because the quantitative thresholds used in this definition of systemic banking crises are *ad hoc*, events that almost meet these thresholds are classified as "borderline." By these criteria, Laeven and Valencia identify 147 crises in 115 countries for the period 1973–2009. Of these crises, 13 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 2007–08, and zero otherwise. The baseline definition includes all systemic crises identified by Laeven and Valencia (2013) for this period.[13] To avoid confounding home- and host-country crises, we also exclude from the pool of host countries all crisis-hit countries (which effectively controls for host-country crises by design).[14] The baseline definition yields a total of 17 systemic banking crises.

To ensure a high-quality working sample, we refine the data in several ways. First, we consider only host countries where there is at least one operating bank from a crisis-stricken country and at least one foreign bank from a noncrisis country, so that a comparison can be made between these two groups (which is necessary for our research design). Second, we drop from the sample all host countries that have less than five operating banks (after excluding the cases mentioned before), so that our results are not driven by unrepresentative outliers. Finally, we exclude foreign banks that change their main country of ownership between 2006 and 2009, so that the country effect is well defined. The resulting sample comprises 361 foreign banks

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

12. A detailed description of the construction of this variable is provided in the technical appendix.

13. 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. We also include Nigeria, which is the only country that was coded as experiencing the start of a crisis in 2009, because many analysts trace the genesis of the crisis to 2008. Our baseline results are robust to relaxing either of these assumptions, and are available on request.

14. Including the few host countries (Nigeria and Ukraine) that did experience host-country crises does not qualitatively affect our results. These additional results are reported in the online appendix.

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.[15]

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

The core set of (host and home) country-level variables consists of (lagged) real GDP growth, real GDP per capita, consumer price inflation, and the current account balance from the World Bank's *World Development Indicators* (WDI). Additional country-level covariates used in our robustness checks include trade openness and financial exports from the WDI; and the aggregate capital to assets ratio and ratio of banks' nonperforming loans to total gross loans from the World Bank's *Financial Development and Structure* database (Beck, Demirgüç-Kunt, and Levine 2000). Additional details on the definitions and sources of all variables are in the technical appendix, and Table OA.2 of the online appendix provides summary statistics for the main variables of interest.

2.2 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 banks with home countries that experienced a crisis relative to those with noncrisis home countries of ownership.

First, foreign banks with home countries experiencing a crisis do differ, on average, in their amounts of outstanding loans as compared to noncrisis foreign banks (*Fact 1*); in 2006, the mean for loans from the former group was $2.4 billion, versus $651 million for the latter.[16] Foreign banks exposed to the crisis in their home countries represent 58% of the sample of foreign banks—which provides some assurance that any estimated treatment effects are unlikely to be driven by outliers or small-sample problems.

Next, a crucial feature of the data is that lending by both groups of foreign banks essentially followed the same trend up through the eve of the crisis (Figure 1): The

15. Tables A1 and A2 in the technical appendix provide additional information on the sample, organized by home and host countries.

16. A matrix summarizing the means, dispersions, and differences for crisis-treated and nontreated foreign banks is provided in Table OA.1 of the online appendix.

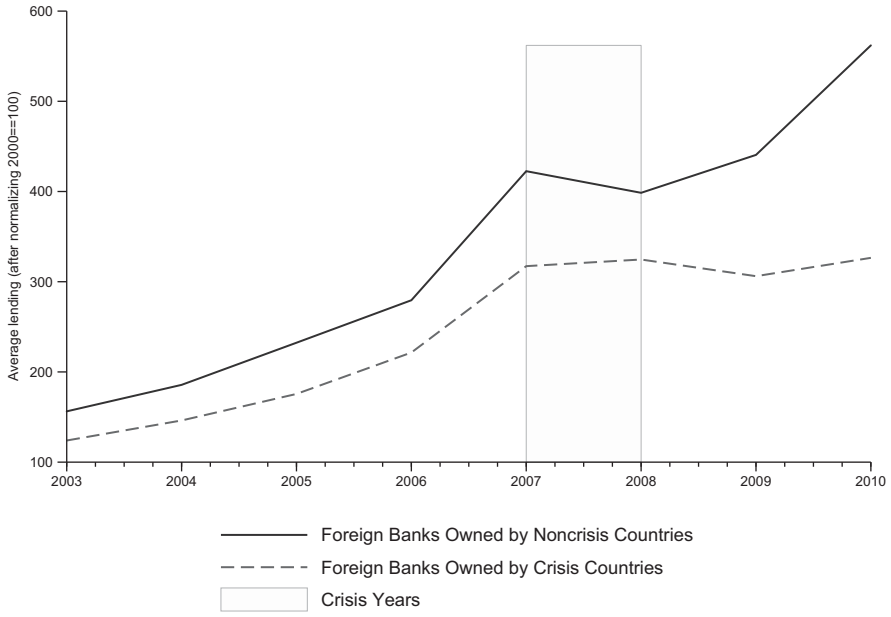

FIG. 1. Trends in Average Lending.

NOTE: This figure presents trends in average lending disaggregated by crisis treatment and nontreatment foreign banks for the period 2003–10. For comparison purposes, the average for each group is normalized to 100 for 2000 (these trends are substantially unaltered by the normalization; see Figure OA.1 of the online appendix). 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.

3-year change in (log) average lending between 2004–06 is statistically indistinguishable between crisis and noncrisis banks (*Fact 2*).[17]

Taken together, these stylized facts argue strongly in favor of our DiD approach: the methodology allows for initial differences to exist between the two groups of interest (*Fact 1*), while the coincident precrisis trends (*Fact 2*) suggest that the two groups in question would likely have shared parallel trends in the absence of a crisis (the common trend assumption).

Finally, as a preview of the results to follow, it is useful to compare the extent to which lending for each group changed between 2006 and 2009. For foreign banks that experienced home country crises, postcrisis average lending increased by 41% (by $972 million, from $2.4 billion), whereas banks that did not experience crises increased their average postcrisis lending by 56% (by $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 change in

17. Just as important, despite a small break in 2007–08, the trend in *non*crisis banks' loans remained unchanged following the crisis. The 3-year change in (log) average lending between 2004–06 and 2008–10 is statistically indistinguishable for noncrisis banks.

lending patterns between the two groups is large and statistically significant, constitutes our stylized *Fact 3*, and motivates the analysis that follows.[18]

3. ECONOMETRIC METHODOLOGY

3.1 A Difference-in-Difference Design

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

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

where the dependent variable $l_{ijk,t}$ is (the logarithm of) total lending of bank $i = 1, \ldots, I$ in host country $j \in J$ at time $t = \{2006, 2009\}$. Each foreign bank i also has as an attribute its home country of ownership $k \in K$. $crisis_k$ is an indicator variable that takes on the value of 1 when country k experiences a systemic banking crisis in 2007/08 (the crisis treatment). $post_t$ is an indicator variable that takes the value 1 if $t = 2009$ (the postcrisis period). ϵ is an idiosyncratic error term, which, depending on the specification, may be clustered along host j and/or home k.

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

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

where the operator Δ denotes the change between two periods.[19] The coefficient of interest, $\tilde{\delta} = \delta$, is the average treatment effect on the treated (ATET) and captures the difference in the average change in lending for crisis vis-à-vis noncrisis banks. Identification of this treatment effect hinges crucially on a common trends assumption between crisis and noncrisis banks (Imbens and Wooldridge 2009). As shown in Subsection 2.2, this assumption is fulfilled in our baseline sample.

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

18. These results are also tabulated formally in Table OA.1 of the online appendix.

19. Since $l_{ijk,t}$ is measured in logs, the left-hand side of (1) is essentially the percentage change in lending pre- and postcrisis.

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

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

where α_i captures a bank-specific effect, and α_j and α_k represent country effects for the *host* and *home* countries, respectively.[20,21] Note that we have allowed for period-specific country effects (γ_2 and γ_3)—which accounts for country-specific idiosyncrasies, such as crisis-related policy interventions—but have constrained the coefficient on bank effects to be constant across the two periods.[22] The above specification can be rewritten as

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

Since the bank fixed effect α_i is time-invariant, it drops out of the first-differenced specification.[23] To the extent that accounting for period-specific bank effects can further improve the efficiency of our estimate of δ', we may wish to explicitly introduce additional bank-specific controls into (2). More specifically, we can estimate

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

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

Although including additional controls in (2) and (3) does mean that time-varying factors at the bank as well as the country level are accounted for, there are two problems with doing so. There is the possibility that introducing additional covariates may lead to a violation, rather than a strengthening, of our common trends assumption.[24] In addition, introducing period-specific coefficients for observable vectors of characteristics may also violate the exogeneity assumption and hence result in biased estimates (Lechner 2010). Consequently, $\tilde{\delta}'$ and $\tilde{\delta}''$ may capture only a relatively

20. Note that we are able to embed home fixed effects without introducing a collinearity problem (relative to the crisis treatment) because even though both operate with the country as the main ontological unit, the two are distinct at the *period-country* level.

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

22. 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.

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

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

crude estimate of the average crisis 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 DiD specification—can further improve the quality of our estimate of δ (Abadie and Imbens 2006).[25] Let

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

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

where $\mathfrak{I}_M(i)$ 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 DiD estimator of Abadie and Imbens generates our coefficient of interest given by

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

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 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.

3.2 Identification of the Crisis Treatment

The estimation described in Subsection 3.1 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 2.2, 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 DiD approach allows initial differences to exist between the two groups in question.

25. Note that in contrast to propensity score matching DiD, the matching estimator of Abadie and Imbens (2006) is not effected to determine *selection* into the crisis treatment; rather, the algorithm ensures comparability of treated and untreated banks. We compute the Abadie and Imbens 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).

To further establish identification, it is necessary that the crisis treatment satisfies the exogeneity assumption. We make this case in four steps. First, we observe that, by and large, developing country-based subsidiary banks of foreign multinationals are dwarfed by the size of their home country banking systems. Consequently, the likelihood that they influenced their respective home-country crises is extremely small.[26]

Second, only certain home—typically, high-income—countries underwent a systemic banking crisis in 2007/08, and consequently, only a subset of the foreign-owned banking subsidiaries in our sample were exposed to the crisis treatment. It is this exogenous variation in home-country experiences that we exploit to identify the effect of a home-country crisis on foreign bank behavior in our baseline DiD specification.

Third, while it is theoretically possible that other noncrisis factors may threaten the quasi-random assignment of the treatment, this does not appear to be a problem in practice. For example, Ongena, Popov, and Udell (2013) have shown that regulatory changes spurred greater bank risk-taking abroad, which would mean that precrisis regulatory differences, rather than the crisis, would have been responsible for the observed treatment. However, while there is evidence that foreign banks in our treatment countries did lend more (in volume terms) precrisis, the stylized facts we show in Subection 2.2 demonstrate that, *even if this were the case*, this distinction did not affect the precrisis *trend* in lending between the two groups, and is therefore inconsequential for our identification strategy. An analogous argument can be made for many other factors that operated in the precrisis period.[27]

The final issue with regard to identification is that of 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 other potential channels may be responsible for the observed treatment effect. The most plausible of these is a trade channel: if foreign banks tend to specialize in lending to firms that export to their home countries, then a collapse in demand for goods and services in crisis countries could cause a collapse in loan demand faced by foreign banks from crisis countries. We test this channel by replacing our crisis treatment with a trade contraction treatment and comparing the results; as shown in Section 5, the estimated treatment effect does not hold up to this replacement, so the estimated treatment effect cannot be explained by this alternative channel. We also consider a fiscal stimulus channel, which many countries introduced in the aftermath of the crisis. In our view, many other candidate channels are almost certainly capturing variants of the crisis effect. For instance, one could argue that risk aversion among treated

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

27. In any case, since we rely on a discrete treatment period, other potential precrisis confounding treatments would have to come into effect both simultaneously and concurrently during this period, which is difficult to see for changes that are implemented in a more incremental fashion, such as regulatory reform.

TABLE 1

BASELINE DIFFERENCE-IN-DIFFERENCE REGRESSIONS FOR BANK LENDING, 2006 AND 2009

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

NOTE: 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; and (3) with two-way clustering. A constant term was included in the regressions, but not reported. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Fixed effects for home and host are period-specific. Cluster sizes are reported for home and host, respectively.

banks may have changed systematically during the crisis period (relative to untreated banks), or that crisis economies may have engaged in more aggressive monetary expansion to ease credit conditions (such as unconventional monetary policy via quantitative easing). In such cases, these changes are *directly related to the crisis*. Consequently, such outcomes do not compromise identification, although it would have implications for the channels of transmission.

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.[28]

4. EMPIRICAL RESULTS

4.1 Baseline Difference-in-Differences

Our baseline results are shown in Table 1. In the first column (B1), we report the baseline DiD specification in (1) with no fixed effects. Below the estimated coefficient we report robust standard errors clustered either by home country, host

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

country, or both.[29] 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 noncrisis-stricken countries. As discussed in the Introduction and Section 1, while this result strikes us as reasonably intuitive, the alternative outcome (of increased lending) is an *a priori* theoretical possibility.

One objection to this simple benchmark is that pooling all foreign banks, regardless of host country, may fail to account for heterogeneity in changing host country conditions. For example, since foreign banks generally extend loans in domestic currency, an appreciation of the currency could cause an increase in our dependent variable (loans measured in U.S. dollars). However, there is no reason to necessarily expect such host country effects to distort our estimate of the treatment effect. Provided that foreign banks experiencing the crisis treatment are just as likely as nontreatment foreign banks, *ex ante*, to locate in those host countries (in the example, those with appreciating currencies), the host country effect would introduce no bias to the residual.[30]

Moreover, any host- or home-country-specific factors that give rise to initial differences between the lending of the treatment and nontreatment groups of banks are controlled for by the DiD strategy, as long as they do not also give rise to differences in *changes* in the lending behavior of the two groups over the crisis period. Any time-invariant bank-specific effects are also captured by even this simple specification. In sum, as long as the identifying assumptions described in Subsections 3.1 and 3.2 hold, we are assured that the estimates in column (B1) are unbiased (Imbens and Wooldridge 2009).

Even so, if we believe that efficiency is enhanced by allowing for period-specific country effects, it is straightforward to introduce these into the baseline setup as in equation (2). One concern is to control for demand-side effects on changes in lending, which can be done by adding host country fixed effects. Accordingly, column (B2) reports results when we allow for these. Doing so (unsurprisingly) substantially improves the fit of the model. Column (B3) controls for only home fixed effects, and column (B4) reports the fullest articulation of equation (2), where we include period-specific fixed effects for both home and host economies. The estimated crisis effect in this case is even stronger than in (B1), and is significant at the 5% level or better, regardless of our choice of error clustering.

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

30. Perhaps more importantly, if the currency composition of loans is indeed correlated with the crisis treatment (as opposed to uncorrelated; in which case the effect would be captured by the host- and home-country fixed effects), this would likely bias our estimated treatment effect *downward*, working against our results.

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% (relative to foreign banks whose home countries did not experience a crisis). This means that for a crisis-stricken foreign bank with an average change in lending of $935 million—the actual average 2006–09 change for crisis-stricken banks—the bank would hypothetically have lent $ [935/(1 − 0.42)] = $ 1.6 billion instead in the absence of its home-country crisis. This is a nontrivial difference.

Finally, it is worth considering what the period-specific country fixed effects mean for our estimated coefficients. Columns (B2)–(B4) essentially allow home and host country fixed effects to take on different slopes in the postcrisis 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).[31]

4.2 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. Furthermore, there are reasonable objections to the indiscriminate inclusion of additional controls. Set against these potential disadvantages is the fact that including covariates in (3) allows us to capture the possibility that bank effects may be period-specific, as they well could after a major shock such as a financial crisis. Our resolution of this latter problem is to include our covariates as they are observed in the precrisis period.

In Table 2, we incrementally introduce the six idiosyncratic bank-specific measures that we define as our core set of bank controls (these are described in Subsection 2.1, with further details provided in the technical appendix). This core set is chosen to best capture important (observable) cross-bank heterogeneity that may potentially affect foreign bank lending behavior.

The main message from this set of results is that, compared to the bare-bones specifications in Table 1, the magnitude and significance of the crisis effect generally holds. Overall, the results here point to postcrisis lending by crisis-stricken banks that is 26% to 57% lower than that of their noncrisis counterparts. The point estimates here are also, on average, a hair larger than those in the baseline, but accompanied by higher standard errors.[32] 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.

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

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

TABLE 2

DIFFERENCE-IN-DIFFERENCES REGRESSIONS FOR BANK LENDING, WITH CORE AND ADDITIONAL BANK-LEVEL COVARIATES, 2006 AND 2009

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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. All bank-level covariates enter with their values set in the precrisis period ($t = 2006$). Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

4.3 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[33] and core bank covariates.[34] Since there is no agreement on an optimal number of matches that should be chosen (Imbens and Wooldridge 2009), we present results for one through four matches in columns (M1)–(M4).[35]

33. 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 DiD, there is a case against overfitting of covariates.

34. In this baseline, we eschew exact matching (which we relax in Section 6 when domestic banks are included). With significant cross-country differences in the levels of the main variables and a nontrivial number of covariates, exact matching offers little improvement over the bias-corrected estimates, at the cost of complicating our interpretation of the matched entity (the percentage of exact matches falls as low as 47%). In any case, imposing exact matching yields qualitatively very similar results, as shown in Table OA.3 of the online appendix.

35. The choice of one match is entirely reasonable—we wish to compare only banks existing in the data, rather than synthetic comparators—and the choice of four matches has been shown to perform well in terms of minimizing mean-squared error (Abadie and Imbens 2011). We also considered higher numbers of matches. In general, these decreased the magnitude of the estimated coefficient, but even for the (extreme) case of 20 matches, the coefficient remained statistically significant (see Table OA.3 of the online appendix).

TABLE 3

MATCHING DIFFERENCE-IN-DIFFERENCE REGRESSIONS FOR BANK LENDING, WITH BANK- AND COUNTRY-LEVEL CONTROLS, 2006 AND 2009

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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank- and country-level covariates enter with their values set in the precrisis period ($t = 2006$).

The qualitative findings remain largely unchanged. The matching DiD estimates are in the same ballpark as the simple DiD regressions and those obtained when bank covariates are included, and the (statistically significant) crisis effect coefficients range from −0.28 to −0.32. In the fullest articulation of the baseline model with four matches—shown in column (M4)—foreign banks exposed to a financial crisis in their home countries have changes in lending that are 30% 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, conditional on the quality of the matches, matching DiD may offer a more refined estimate of the crisis effect. In the simplest DiD implementation with no additional controls, all crisis and noncrisis banks are pooled together and identification of the average crisis effect relies on the more-or-less random distribution of other characteristics across the sample. But such pooling may fail to accurately gauge the true extent of the crisis effect if banks' observable characteristics are correlated with both the treatment and error term, and including covariates may not completely resolve this.[36] 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 matching on observables does not introduce any selection bias—and with a fairly comprehensive set of core bank controls, there is little reason to think it would—the estimate renders a better apples-to-apples comparison. Therefore, the fact that this set of estimates is consistent with our baseline DiD results lends greater strength to our claim of a genuinely causal crisis effect.

36. If the covariate distributions differ substantially between the crisis and noncrisis groups, then the estimates depend on extrapolation out of sample, and become much more sensitive to parallel trends assumption.

5. ROBUSTNESS CHECKS

5.1 Additional Controls

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

We first replace two variables in the core set of bank covariates with alternative measures: bank weakness, and profitability.[37] Specifically, we substitute interest margin with profitability, and liquidity with weakness. These results are given in columns (R1) and (R2) that build on, respectively, the DiD specification with bank covariates—a variation of specification (C6)—and the matching DiD equivalent, which is a variation of specification (M4).

Next, we allow for the possibility of period-specific effects that operate at the country-pair level (as opposed to independently at the country level). More specifically, we replace the home and host country fixed effects α'_j and α'_k in (2) with a fixed effect α'_{jk} for each unique home-host dyad. This approach will absorb greater unobservable heterogeneity insofar as pairwise effects—such as those arising from economic closeness at the bilateral level (de Haas and van Horen 2012b)—are relevant to lending behavior. Since this approach to capturing fixed effects is fundamentally distinct from even our simple augmented model (2), we show first the DiD results obtained without bank covariates altogether in column (R3), and then with all core bank covariates in (R4) (analogous to specifications (B4) and (C6), respectively).

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

In the right panel, we examine the robustness of our results to an alternative measure of our pre- and postcrisis periods: rather than utilizing data from two individual years (2006 and 2009), we average observations from 2005 and 2006 for the precrisis period, and 2009 and 2010 for the postcrisis period.[38] Here, for reasons of space, we report only the DiD and matching DiD estimates when controlling for only country-level fixed effects/core covariates (columns (R7) and (R8), respectively), and with core and alternative bank covariates (columns (R9) and (R10)).

37. 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).

38. We perform the period averaging to avoid serial correlation problems that may arise from DiD treatments that span multiple time periods (Bertrand, Duflo, and Mullainathan 2004). Data limitations prevent us from using longer averages.

TABLE 4

ROBUSTNESS OF DID AND MATCHING DID REGRESSIONS FOR BANK LENDING, WITH ALTERNATIVE AND ADDITIONAL BANK- AND COUNTRY-LEVEL CONTROLS, 2006 AND 2009

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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Fixed effects for home, host, and pair are period-specific. Core bank and country covariates are listed in the Appendix. The alternative set of covariates includes *profitability* in lieu of interest margin and *weakness* in lieu of liquidity. Additional country covariates are related to the banking system (bank capital, bank nonperforming loans) and economic openness (trade openness, financial services exports), and are listed in the Appendix. All bank- and country-level covariates enter with their values set in the precrisis period ($t = 2006$). Cluster sizes are reported for home and host, respectively.

As evident from Table 4, our baseline results by and large survive this array of robustness checks. There is little variation in the magnitude of the estimated crisis treatment effect, although the range is broader: coefficient estimates are bound by [−0.30, −0.63], and most retain their statistical significance at the 5% level or lower.

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

5.2 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 that were correlated with the crisis treatment.

Our first test alters the pre- and postcrisis dates to an earlier period; we choose 2002 and 2005 as alternative years.[39] 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 (C6), respectively). The insignificant estimated coefficients indicate that the baseline estimations of the treatment effect in the DiD model are indeed capturing an effect unique to the period between 2006 and 2009.[40]

Our second falsification exercise considers the other major nonfinancial crisis-related event that occurred in the intervening period: the great trade collapse of 2008/09 (Baldwin 2009).[41] Of course, financial crises and other economic crises are

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

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

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

TABLE 5

FALSIFICATION TESTS FOR DIFFERENCE-IN-DIFFERENCE REGRESSIONS FOR BANK LENDING

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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. All bank-level covariates enter with their values set in the precrisis period ($t = 2006$). Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

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

To rule out this channel, the falsification test requires the construction of a new treatment variable that captures the effect of a trade collapse, a replacement of the crisis treatment with this dummy, and new estimates that rely on differences between the two sets of treatments to identify the trade contraction effect. The second panel of Table 5 shows the results of this second set of falsification exercises. In columns (F3) and (F4), we again estimate the augmented DiD specifications both without and with 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 (i.e., the 50th percentile of all decreases in home-country trade flows; full details for the construction of this treatment are provided in the technical appendix).[42]

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

42. Our qualitative findings do not depend on this choice of cutoff; Table OA.4 reports results with cutoffs at the 30th and 70th percentiles.

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, even if foreign banks with crises at home tended to face demand shocks associated with trade collapses, this effect appears to have operated in the opposite direction as the crisis treatment, and would have diminished its estimated effect.[43] This striking result is consistent with the interstate banking model of Morgan, Rime, and Strahan (2004), in which a shock to borrowers' collateral at home causes a holding company to send capital to its subsidiary banks out of state, while a home shock to bank capital causes it to pull capital from its subsidiaries.

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

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

5.3 Potential Channels of Transmission for the Crisis Effect

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

Consequently, our strategy here is to restrict our analysis to home countries that *did* experience crises, and to explore whether *changes* in bank-level liquidity or solvency are correlated with changes in bank lending. Both liquidity (Bernanke 1983) and

43. This falsification test does not completely rule out the possibility that part of the effects are driven by reduced loan demand from firms operating in the host country but with headquarters in a crisis country. Nonetheless, our results are robust to the extent that this reduced demand does not stem from a country-level collapse in trade, which we regard as the primary channel for loan demand contraction that may confound our identification strategy.

solvency (Bernanke and Gertler 1989, Freixas and Jorge 2008) channels have been routinely identified as key channels for monetary transmission (in general) as well as shocks that can affect credit provision (in particular). These channels are therefore prime candidates for the transmission of the home-country banking crisis.[44]

Adding changes to liquidity and solvency (rather than initial levels, as in our baseline) introduces new technical considerations. First, since both are measured as ratios to total assets, normalizing the dependent variable with a common denominator enables a cleaner comparison. In addition, this normalization allows us to circumvent an indeterminacy problem that could arise with changes in the solvency measure.[45] We accordingly replace Δl_{ijk} with $\Delta \frac{l_{ijk}}{a_{ijk}}$, where a_{ijk} are the total assets of bank i hosted in country j with home country k. Specifically, we regress

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

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

It is also important to exercise caution in the interpretation of the coefficients. An increase in the ratio of liquid assets between 2006 and 2009 may reflect an improvement of a bank's already-strong liquidity position, but it is also consistent with a liquidity-starved bank that needed to significantly shore up its liquidity following the crisis. Our interest is therefore less in the *sign* of the coefficients, but their statistical significance and relative magnitudes (setting up a "horse race" between the two). Table 6 presents standardized coefficients for all specifications that we consider. For economy, we report only the DiD specifications without (odd-numbered columns) and with (even-numbered columns) country fixed effects and two-way clustering, although other variations are qualitatively similar and support our main conclusion drawn here.[46]

The results make a fairly convincing case that in the recent crisis, bank-level access to liquidity, rather than concerns over solvency, was the key transmission channel for banks that scaled back on lending due to home country crises. The coefficients for changes in liquidity are consistently statistically significant, and twice the magnitude of those for solvency. Importantly, while potential endogeneity in the channels variables militates against making any causal claims, we can nevertheless infer that changes in liquidity are more consistently associated with changes in lending, and hence are a more likely transmission channel for the crisis effect.

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

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

46. A number of these additional results are reported in Table OA.5 of the online appendix.

TABLE 6

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

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

Note: The dependent and channel variables are in 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; and (3) with two-way clustering. A constant term was included in the regressions, but not reported. * indicates significance at 10% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Fixed effects for home and host are period-specific. Cluster sizes are reported for home and host, respectively.

5.4 Differences in Treatment of Subsidiaries and Branches

In this subsection, we explore whether our baseline results are robust to possible differences between branches and subsidiaries. As noted in footnote 10, the distinction is not standardized across countries, which makes any straightforward comparison potentially misleading. With this caveat in mind, we explore the differences along two dimensions.

First, we attempt to explore the differences between branches and subsidiaries at the entity level. For this purpose, we are limited to the branch coverage defined by *Bankscope*. Unfortunately, the coverage of branches in *Bankscope* is quite limited,[47] and only five entities in our baseline sample of 361 foreign banks were classified as branches: two with crisis-stricken home countries, and three with noncrisis home countries. We rerun our baseline DiD specification after dropping these five branches. Table OA.6 in the online appendix reports the results, which show that the baseline results remain qualitatively unchanged when identifying the crisis effect in a sample composed solely of subsidiaries (which is unsurprising given the small number of branches in the sample).

Second, we examine the distinction from the perspective of differences in country-level treatment of branches relative to subsidiaries. To this end, we construct an indicator variable that classifies whether the two entities face differential regulatory

47. This limitation has already been emphasized in the literature; see, e.g., Claessens and van Horen (2015). We also confirmed this limited coverage via private correspondence with a *Bankscope* representative.

treatment in the host country, based on both survey question data from the Bank Regulation and Supervision Survey carried out by the World Bank (Čihák et al. 2012) and a regulatory index drawn from Cerutti, Dell'Ariccia, and Martínez Peria (2007, p. 1677) (details are in the technical appendix). Table OA.7 in the online appendix reports the results when we split the sample between countries that impose this regulatory distinction, versus those that do not. It turns out that the economies that impose a branch/subsidiary distinction appear to be somewhat more central in driving our results, although qualitatively similar estimates of the crisis effect (though not always statistically significant) are found among countries that do not make this distinction. These suggest that differences in the regulatory treatment of branches vis-à-vis subsidiaries may have affected postcrisis bank lending patterns, but any effect appears to be relatively small. The small number of branches in our sample precludes us from exploring the question of whether branches' and subsidiaries' responses to home-country crises are conditioned differently by the regulatory environment, so we leave this open as an avenue for future research.

5.5 Other Robustness Checks

Controlling for host-country crises and aggregate lending. Our baseline results are based on a working sample that does not contain host countries that experienced a crisis. This in itself eliminates bias from host-country crisis effects, and means that our baseline results are (by construction) identified for cases where a crisis occurred *only* in the home country. In this subsection, we explore whether our results are also robust to the inclusion of the two host countries that experienced a crisis according to the crisis definition of Laeven and Valencia (2013): Nigeria and Ukraine, which together bring into the sample 16 banks. We do this in an incremental fashion. In Table AO.8 of the online appendix, we simply include these observations into the sample and reestimate our baseline specification (1). However, this specification does not control specifically for *host* crisis effects. We therefore introduce an additional host crisis indicator; results are in Table AO.9. Finally, we allow for differential effects between home and host crises by including, in the model in levels, a triple interaction between the home-crisis, host-crisis, and post indicators. These results are reported in Table AO.10. In all three cases, the results for the home-crisis effect hold in these alternative specifications, which reassures us that our baseline is robust to the effects of crises in the host country.

We also run our baseline DiD model including a control for aggregate domestic lending (proxied by domestic bank credit as percentage of GDP). The advantage of this control is that it is available for all host countries in our sample, and would capture changes in aggregate lending between the pre- and postcrisis periods in host countries. We include this variable as a stand-alone control interacted with the post dummy and also interacted with the $HomeCrisis \times Post$ interaction. The results of these specifications, reported in Table OA.11, suggest that the baseline results for

the home-crisis effect are robust to controlling for changes in aggregate bank lending in the host economies. The triple interaction yields inconclusive results whether foreign banks retrenched more from countries that simultaneously saw a slowdown in aggregate lending.

Ownership changes following the crisis. As we noted in Section 2.1, in order to have a well-defined crisis effect, we exclude from the baseline sample banks that changed their main country of ownership between 2006 and 2009. However, it may be informative to know if our baseline results are robust to the inclusion of foreign banks that changed ownership classification between 2006 and 2009. This can occur either because they were acquired by foreigners (a change from domestic to foreign), or because their main country of foreign ownership changed. Our baseline sample excluded 74 banks because of either of these two cases. As a check, Table AO.12 of the online appendix replicates our baseline results after including all banks that were classified as foreign as of 2009. Our baseline results are qualitatively unchanged even when we account for postcrisis ownership changes.[48]

Alternative dependent variable. Our choice of a bank's total outstanding loans (net of loan loss reserves) is motivated by the question of whether lending behavior by crisis-stricken foreign banks differs from that of their noncrisis counterparts. That said, it is of potential interest whether our baseline results are robust to alternative measures of the dependent variable. Tables OA.13 and OA.14 of the online appendix report estimates of our baseline model using two alternative dependent variables: the annual growth rate of loans and the ratio of loans to assets, respectively. While these dependent variables answer somewhat different questions, both of these tables nevertheless show that the postcrisis period was characterized by negative effects in other measures of lending by foreign banks if they were headquartered in crisis-stricken countries (relative to those from noncrisis countries).[49]

48. The bulk of the new banks in this sample are from the same home countries in our baseline sample. Of the 74 new banks, 65 changed ownership to countries already in our baseline sample, either from domestic to foreign (40) or from other foreign countries (25). These results also show 81 home clusters because there are a total of 15 new home countries in this sample compared to the baseline. All of these new home countries are noncrisis countries (five countries gained banks as of 2009, and 10 lost banks and do not appear as home countries as of 2009). Additional unreported regressions also indicate that the baseline results are also robust to the addition of only the 30 banks that changed ownership from other foreign countries.

49. Table AO.13, in which the growth rate (rather than levels) of bank lending is the dependent variable, explores whether credit provision accelerated or decelerated in the postcrisis period. Scaling the dependent variable by assets, as done in Table AO.14, alters the interpretation of what a change in the dependent variable implies. For example, changes in the ratio of loans to assets could result from either an increase in lending (the numerator) or a decrease that is more than offset by a decline in asset valuations (in the denominator). Thus, even if one believes that assets (or other bank-specific factors) are important conditioning variables for lending, we believe that these are best entered into the specification in an unrestricted fashion—by including them as controls as we do in Table 2—rather than as a scaling factor in the denominator.

TABLE 7

MATCHING DIFFERENCE-IN-DIFFERENCE REGRESSIONS USING EXACT MATCHING FOR HOST COUNTRY WITH SAMPLE EXPANDED TO INCLUDE DOMESTIC BANKS, 2006 AND 2009

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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank-level covariates enter with their values set in the precrisis period ($t = 2006$).

6. HETEROGENEITY IN THE CRISIS EFFECT

6.1 Comparing Foreign Banks to Domestic Banks

In Subsection 2.2, we made the case for why noncrisis foreign banks are the most appropriate comparison group for estimating the effect of the crisis treatment: introducing domestic banks into the working sample would be inappropriate from an econometric perspective when using the DiD estimator, since it would pool all control banks in the comparison group despite the fact that domestic and foreign banks differ along important dimensions related to changes in lending outcomes (e.g., facing different loan demand schedules).

However, much of the empirical literature (cited in the Introduction) *is* interested in distinctions between domestic and foreign bank behavior, and it is informative to expand our analysis to include domestic banks. While the use of the DiD estimator remains circumspect for the purposes of obtaining a causal estimate of the crisis effect when domestic banks are included in the sample, the matching estimator offers an ideal solution to this problem: since matching uses the closest possible match(es) for each treated bank, we are assured that the control is, in fact, an appropriate counterfactual. A further advantage of this empirical strategy is that it allows us to expand to a larger pool of controls from which matches are chosen. We should emphasize, however, that the results we present here should not be used to render general statements about aggregate credit provision (since our comparisons of each group's lending behavior are ultimately made relative to each other).

Table 7 presents the results of the matching estimator using this expanded sample, analogous to Table 3, which now allows both domestic banks and noncrisis foreign banks to be used to construct controls. In these regressions, we match treated banks only with nontreated 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.[50]

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 postcrisis recovery in lending for foreign noncrisis banks, as compared with domestic banks, may have been faster on average. This also suggests that total lending in the host countries may well have been lower in the absence of these noncrisis stricken foreign banks, whose immediate postcrisis lending exceeded that of domestic banks. This is important, as most studies so far compare domestic and foreign banks, or alternative sources of funding, with no distinction between crisis and noncrisis foreign banks (Claessens and van Horen 2014, de Haas and van Lelyveld 2014). Some of these studies are then led to conclude that foreign banks may have reduced their lending vis-à-vis domestic banks. Our findings, in contrast, point to important differences *among* foreign banks. Indeed, nothing in our findings rules out the possibility that postcrisis credit provision by foreign banks, *on aggregate*, may actually be higher.[51]

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.[52] The results in Table 8 verify that foreign banks from noncrisis countries *increased* their lending relatively more than domestic banks: the coefficients on the foreign noncrisis term are positive, and in a number of specifications, statistically significant.[53]

6.2 Comparing Distinct Features among Foreign Banks

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

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

51. This is actually a fairly common result in the empirical literature; see, e.g., Clarke et al. (2003), Martínez-Peria, Powell, and Vladovka-Hollar (2005), de Haas and van Lelyveld (2010), and Wu, Luca, and Jeon (2011).

52. 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 our basic DiD model).

53. This result is graphically illustrated in Figure OA.2 of the online appendix.

TABLE 8

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

Note: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Cluster sizes are reported for home and host, respectively. Fixed effects for home, host, and pair are period-specific. Core bank and country covariates are listed in the Appendix. All bank- and country-level covariates enter with their values set in the precrisis period ($t = 2006$).

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

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

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

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

Note that the insignificant coefficient on the uninteracted crisis term for Eastern Europe and Central Asia is no real cause for concern. The total effect of the crisis has to be inferred from the sum of both the uninteracted and the interaction term, and if

TABLE 9

DIFFERENCE-IN-DIFFERENCE REGRESSIONS FOR BANK LENDING WITH ADDITIONAL INTERACTIONS, 2006 AND 2009

	Regions						*Ownership*	
	S1	S2	S3	S4	S5	S6	S7	S8
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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Regions correspond to World Bank regions (EAP = East Asia and Pacific; ECA = Eastern Europe and Central Asia; LAC = Latin America and Caribbean; MNA = Middle East and North Africa; SAS = South Asia; SSA = sub-Saharan Africa). Ownership is either publicly listed (pub. list.) or government-owned (govt.), the other group being privately held banks. Fixed effects for home and host are period-specific. A constant term was included in the regressions, but not reported. Cluster sizes are reported for home and host, respectively.

we treat statistically insignificant coefficients as equal to zero, the total effect for all cases remains significantly negative.[54]

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.[55]

54. Another possible interpretation of the insignificant independent crisis effect in the ECA specification is that only ECA banks are responsible for our results; that is, the crisis effect would not be significant if ECA banks, which were especially hard hit by the crisis, were not included in our sample. To rule out this possibility (as well as the possibility that selected regional subsamples may be giving rise to our overall crisis effect), we ran regressions using our baseline specification that systematically excluded one region at a time from the sample. The results, reported in Table OA.15 of the online appendix, generally hold up to this selective exclusion, indicating that the crisis effect is not due to the lending behavior of foreign banks in any one region.

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

TABLE 10

DIFFERENCE-IN-DIFFERENCE AND MATCHING DIFFERENCE-IN-DIFFERENCE REGRESSIONS FOR ASIAN FINANCIAL CRISIS, 1996 AND 1999

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

NOTE: 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% level, ** indicates significance at 5% level, and *** indicates significance at 1% level. Covariates used for matching are the core country and bank controls listed in the Appendix. All bank- and country-level covariates enter with their values set in the precrisis period ($t = 2006$).
[a]Bank core covariates include only size and solvency.

7. COMPARISON TO THE 1997/98 ASIAN FINANCIAL CRISIS

In this section, we expand the scope of our baseline by considering an alternative case study, that of the 1997/98 Asian financial crisis. Although this crisis was not as global as the 2007/08 crisis, the Asian crisis—which was precipitated by the failed defense of the Thai baht in mid-1997—ultimately spread across East Asian emerging markets and, by 1998, had induced crises in other developing economies: Russia, and parts of Eastern Europe and Latin America. The scope of the crisis enables us to replicate our analytical framework outlined in Section 3.[56]

We proceed by redefining our crisis treatment as unity when its main country of ownership experienced a banking crisis in the years 1997–98, and zero otherwise.[57] We then repeat our exercise for the various specifications in Section 4.

One major caveat is that, owing to data limitations, our sample size is drastically reduced: our baseline comprises just 32 banks, operating in only six economies. Treated banks comprise just over a fifth of the sample, and many home countries

56. Importantly, we find no compelling reason to reject our key identification assumptions. For the common trends assumption, while data limitations prevent us from computing precrisis trends—data for the Asian crisis subsample begin only in 1996—there is no evidence of convergence in average lending between the two groups between 1996 and 1997 (see Figure OA.3 of the online appendix). Although this fact alone is not *sufficient* to verify parallel trends, its presence is certainly *necessary* if the assumption were to hold. For the exogeneity assumption (that treated subsidiaries did not induce their home-country crises), the subsidiaries of Asian crisis-stricken banks in our sample were all small relative to their home banking systems.

57. In contrast to our baseline, where the crisis treatment overlaps almost perfectly between the coding scheme of Laeven and Valencia (2013) and Reinhart and Rogoff (2009), there is greater variation in the dating of crises in the Asian case. Two countries, in particular, have crisis start dates in the latter data set that begin in 1992 instead of either 1997 or 1998 (China and Japan). We code these as noncrisis countries.

are represented by only one bank. With these caveats in mind, Table 10 reports a selection of these results, corresponding to specifications (B1), (B4), (C2), and (M2)–(M4).[58] The results are broadly in line with our baseline findings. Coefficients for the crisis effect are negative, and across many specifications, statistically significant. The magnitudes, however, are far less stable, likely owing to the small sample size. Overall, it appears that foreign banks with ownership based in countries that experienced the 1997/98 crisis also scaled back lending relatively more than those with ownership based in noncrisis economies. Given the caveats already raised, however, we view the results here as mainly providing corroborative, rather than definitive, evidence in support of our central claim.

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% 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 reasonably confident that this effect is causal.

We recognize, however, that our approach and database carry limitations. It would be desirable to have lender–borrower matched data for a large number of countries, which would enable us to explore borrower heterogeneity to a deeper extent, but data limitations render this infeasible. Similarly, we do not have data on banks' internal capital markets or cross-border lending, which would allow us to explore important issues such as flight-home and flight-to-quality effects. Nevertheless, our ability to offer very general evidence of foreign bank behavior during crises—which overcomes external validity concerns endemic to single-country case studies—strikes us as a reasonable trade-off.

Importantly, we caution against the use of our results to justify closure of emerging economies' financial markets to foreign banks; after all, foreign banks probably carry a host of additional benefits in terms of financial stability and enhanced competition (Clarke et al. 2003), and our results—premised on DiD analysis—apply to a very specific situation (when foreign countries are experiencing crises of their own) and subset of the banking landscape (to foreign banks, in particular). Our results do, however, suggest that domestic monetary authorities should be aware of the potential for greater credit contraction by foreign banks under certain circumstances, and—if warranted by real-time data—support domestic liquidity formation during such crises accordingly.

58. Data limitations imply that including all bank covariates for the final four specifications would have resulted in an unacceptably small sample size. Estimates for additional specifications are reported in Table OA.16 of the online appendix.

APPENDIX A: TECHNICAL APPENDIX

A.1 Data Sources and Definitions

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,[59] 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.[60]
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 that 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.

Classification of banking and financial crises. Systemic banking crises are taken

59. 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.

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

TABLE A1

BASELINE SAMPLE OF HOME COUNTRIES BY CRISIS AND NONCRISIS STATUS, WITH CORRESPONDING NUMBER OF BANKS

Country	Banks	Country	Banks	Country	Banks
Crisis countries[a]					
(17 countries; 208 banks)					
Austria	10	Ireland	1	Portugal[b]	7
Belgium	3	Italy	6	Slovenia[b]	1
Denmark	1	Latvia	1	Spain	16
France[b]	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[c]	6
Australia	2	Hong Kong[c]	2	Peru	2
Azerbaijan	1	Hungary	3	Russia	9
Bahrain[c]	6	India	9	Saudi Arabia	1
Botswana	2	Indonesia	1	Singapore[c]	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[c]	2	UAE	4
Ecuador	1	Libya	4	Uruguay	3
Egypt	1	Liechtenstein	1	Uzbekistan	1
Estonia	1	Malaysia	1	Venezuela	1
Finland	1	Mauritius[c]	1		
Guatemala	1	Mexico	1		

NOTE: [a] As defined by Laeven and Valencia (2013).
[b] Borderline banking crisis.
[c] Offshore financial center.

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

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

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

- deposit freezes and bank holidays;
- significant bank nationalizations (treasury or central bank asset purchases exceeding 5% of GDP);

TABLE A2

BASELINE SAMPLE OF HOST COUNTRIES, AND CORRESPONDING NUMBER OF FOREIGN AND DOMESTIC BANKS

Host Countries (51 countries; 361 foreign banks; 738 domestic banks)					
Country	Foreign	Domestic	Country	Foreign	Domestic
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			

- 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.

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

TABLE A3

SOURCES AND DEFINITIONS FOR MAIN VARIABLES OF INTEREST

Variable	Definition	Source
Variable of interest		
Loans	Stock of gross loans,[a] less reserves for impaired loans (NPLs)	Bankscope
Crisis	1 if the home country experienced a systemic banking crisis; 0 otherwise[b]	Authors/Laeven and Valencia (2013)
Core bank-level covariates		
Size	Stock of total earning assets	Bankscope
Solvency	Ratio of equity to total assets (%)	Bankscope
Income to loan ratio	Net current income/Total loans (%)	Bankscope
Interest margin	Interest income on assets less expense paid on liabilities/Total assets (%)	Bankscope
Wholesale	Net loans as a percentage of customer funding (%)	Bankscope
Liquidity	Liquid assets/Total Assets (%)	Bankscope
Core country-level covariates		
GDP growth	Real GDP growth, lagged one year	WDI[c]
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[d]
Alternative bank and additional country covariates		
Profitability	Return on average equity (%)	Bankscope
Weakness	Ratio of loan loss provisions to net interest revenue (%)	Bankscope
Trade openness	Imports plus exports (% of GDP)	WDI
Financial exports	Insurance and financial services (% of service exports, BoP)	WDI
Bank capital	Bank capital to assets ratio (%)	Beck, Demirgüç-Kunt, and Levine (2000)
Nonperforming loans	Ratio of banks' nonperforming loans to total gross loans (%)	Beck, Demirgüç-Kunt, and Levine (2000)

NOTE: [a]Gross loans include residential mortgage, other mortgage, other consumer/retail, corporate and commercial, and other loans.
[b]The construction of this variable is described in detail in the text.
[c]WDI = World Development Indicators.
[d]BIS = Bank of International Settlements.

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 tables of the online appendix).

The *fiscal stimulus* treatment is based on the data set 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% of GDP at the high end ($400 billion and $2.1 trillion, Saudi Arabia and China, respectively) to 0.07% and 0.04% 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.

Construction of Regulation Indicator. The indicator for differential *regulatory treatment* of branches versus subsidiaries in host countries was constructed from two main sources: regulators' answers to survey questions in the Bank Regulation and Supervision Survey (BRSS) carried out by the World Bank (Čihák et al. 2012), and a host-country index drawn directly from Cerutti, Dell'Ariccia, and Martínez Peria (2007). Our dummy variable equals one if according to the BRSS, the country prohibits branches, has extra capital requirements on branches, or prohibits fully owned subsidiaries; or if according to the index constructed by Cerutti, Dell'Ariccia, and Martínez Peria (for a smaller number of host countries) the country's regulators treat branches and subsidiaries differently. By using these two sources in combination, the dummy variable covers 44 of the 51 host countries in our baseline working sample. Coverage was then extended to the remaining seven countries based on researching their banking regulations individually. In total, the dummy equals one for 22 out of our 51 host countries.

LITERATURE CITED

Abadie, Alberto, David M. Drukker, Jane L. Herr, and Guido W. Imbens. (2004) "Implementing Matching Estimators for Average Treatment Effects in Stata." *Stata Journal*, 4, 290–311.

Abadie, Alberto, and Guido W. Imbens. (2006) "Large Sample Properties of Matching Estimators for Average Treatment Effects." *Econometrica*, 74, 235–67.

Abadie, Alberto, and Guido W. Imbens. (2011) "Bias-Corrected Matching Estimators for Average Treatment Effects." *Journal of Business & Economic Statistics*, 29, 1–11.

Aiyar, Shekhar. (2012) "From Financial Crisis to Great Recession: The Role of Globalized Banks." *American Economic Review*, 102, 225–30.

Baldwin, Richard E. (editor). (2009) *The Great Trade Collapse: Causes, Consequences and Prospects*. London, England: Centre for Economic Policy Research Press.

Beck, Thorsten, Aslı Demirgüç-Kunt, and Ross Levine. (2000) "A New Database on the Structure and Development of the Financial Sector." *World Bank Economic Review*, 14, 597–605.

Bernanke, Ben S. (1983) "Nonmonetary Effects of the Financial Crisis in Propagation of the Great Depression." *American Economic Review*, 73, 257–76.

Bernanke, Ben S., and Mark L. Gertler. (1989) "Agency Costs, Net Worth, and Business Fluctuations." *American Economic Review*, 79, 14–31.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. (2004) "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, 119, 249–75.

Brei, Michael, and Alfredo Schclarek. (2015) "A Theoretical Model of Bank Lending: Does Ownership Matter in Times of Crises?" *Journal of Banking and Finance*, 50, 298–307.

Cameron, Colin, Jonah B. Gelbach, and Douglas L. Miller. (2011) "Robust Inference with Multiway Clustering." *Journal of Business & Economic Statistics*, 29, 238–49.

Cerutti, Eugenio M., Giovanni Dell'Ariccia, and María Soledad Martínez Peria. (2007) "How Banks Go Abroad: Branches or Subsidiaries?" *Journal of Banking and Finance*, 31, 1669–92.

Cetorelli, Nicola, and Linda S. Goldberg. (2011) "Global Banks and International Shock Transmission: Evidence from the Crisis." *IMF Economic Review*, 59, 41–76.

Cetorelli, Nicola, and Linda S. Goldberg. (2012a) "Banking Globalization and Monetary Transmission." *Journal of Finance*, 67, 1811–43.

Cetorelli, Nicola, and Linda S. Goldberg. (2012b) "Follow the Money: Quantifying Domestic Effects of Foreign Bank Shocks in the Great Recession." *American Economic Review*, 102, 213–8.

Cetorelli, Nicola, and Linda S. Goldberg. (2012c) "Liquidity Management of U.S. Global Banks: Internal Capital Markets in the Great Recession." *Journal of International Economics*, 88, 299–311.

Čihák, Martin, Aslı Demirgüç-Kunt, Maria Soledad Martínez Peria, and Amin Mohseni-Cheraghlou. (2012) "Bank Regulation and Supervision around the World: A Crisis Update." Policy Research Working Paper 6286, The World Bank, Washington, DC.

Claessens, Stijn A.M.F., and Neeltje van Horen. (2015) "The Impact of the Global Financial Crisis on Banking Globalization." *IMF Economic Review*, 63, 868–918.

Claessens, Stijn A.M.F., Giovanni Dell'Ariccia, Deniz Igan, and Luc A. Laeven. (2010) "Cross-Country Experiences and Policy Implications from the Global Financial Crisis." *Economic Policy*, 25, 267–93.

Claessens, Stijn A.M.F., Aslı Demirgüç-Kunt, and Harry P. Huizinga. (2001) "How Does Foreign Entry Affect Domestic Banking Markets?" *Journal of Banking and Finance*, 25, 891–911.

Claessens, Stijn A.M.F., and Neeltje van Horen. (2014) "Foreign Banks: Trends and Impact." *Journal of Money, Credit and Banking*, 46, 295–326.

Claessens, Stijn A.M.F., Neeltje van Horen, Tugba Gurcanlar, and Joaquin Mercado. (2008) *Foreign Bank Presence in Developing Countries 1995–2006: Data and Trends*. Mimeograph: University of Amsterdam.

Clarke, George R.G., Robert J. Cull, Maria Soledad Martínez-Peria, and Susana M. Sánchez. (2003) "Foreign Bank Entry: Experience, Implications for Developing Economies, and Agenda for Further Research." *World Bank Research Observer*, 18, 25–59.

Clarke, George R.G., Robert J. Cull, Maria Soledad Martínez-Peria, and Susana M. Sánchez. (2005) "Bank Lending to Small Businesses in Latin America: Does Bank Origin Matter?" *Journal of Money, Credit and Banking*, 37, 83–118.

Cull, Robert J., and Maria Soledad Martínez-Peria. (2013) "Bank Ownership and Lending Patterns during the 2008–2009 Financial Crisis: Evidence from Latin America and Eastern Europe." *Journal of Banking and Finance*, 37, 4861–78.

de Haas, Ralph, Yevgeniya Korniyenko, Alexander Pivovarsky, and Teodora Tsankova. (2015) "Taming the Herd? Foreign Banks, the Vienna Initiative and Crisis Transmission." *Journal of Financial Intermediation*, 24, 325–55.

de Haas, Ralph, and Neeltje van Horen. (2012a) "International Shock Transmission after the Lehman Brothers Collapse: Evidence from Syndicated Lending." *American Economic Review*, 102, 231–7.

de Haas, Ralph, and Neeltje van Horen. (2012b) "Running for the Exit? International Bank Lending During a Financial Crisis." *Review of Financial Studies*, 26, 244–85.

de Haas, Robert, and Iman van Lelyveld. (2006) "Foreign Banks and Credit Stability in Central and Eastern Europe. A Panel Data Analysis." *Journal of Banking and Finance*, 30, 1927–52.

de Haas, Robert, and Iman van Lelyveld. (2010) "Internal Capital Markets and Lending by Multinational Bank Subsidiaries." *Journal of Financial Intermediation*, 19, 1–25.

de Haas, Robert, and Iman van Lelyveld. (2014) "Multinational Banks and the Global Financial Crisis: Weathering the Perfect Storm?" *Journal of Money, Credit and Banking*, 46, 333–64.

Dell'Ariccia, Giovanni, and Robert Marquez. (2004) "Information and Bank Credit Allocation." *Journal of Financial Economics*, 72, 185–214.

Detragiache, Enrica., Thierry Tressel, and Poonam Gupta. (2008) "Foreign Banks in Poor Countries: Theory and Evidence. *Journal of Finance*, 63, 2123–60.

Donald, Stephen G., and Kevin Lang. (2007) "Inference with Difference-in-Differences and Other Panel Data." *Review of Economics and Statistics*, 89, 221–33.

Freixas, Xavier, and José M.P. Jorge. (2008) "The Role of Interbank Markets in Monetary Policy: A Model with Rationing." *Journal of Money, Credit and Banking*, 40, 1151–76.

Galindo, José M.P., Alejandro Izquierdo, and Liliana Rojas-Suarez. (2013) "Financial Integration and Foreign Banks in Latin America: How Do They Impact the Transmission of External Financial Shocks?" *International Finance Review*, 14, 305–39.

Giannetti, Mariassunta, and Luc A.H. Laeven. (2012a) "The Flight Home Effect: Evidence from the Syndicated Loan Market During Financial Crises." *Journal of Financial Economics*, 104, 23–44.

Giannetti, Mariassunta, and Luc A.H. Laeven. (2012b) "Flight Home, Flight Abroad, and International Credit Cycles." *American Economic Review*, 102, 219–24.

Goldberg, Linda S. (2009) "Understanding Banking Sector Globalization." *IMF Staff Papers*, 56, 171–97.

Gormley, Todd A. (2010) "The Impact of Foreign Bank Entry in Emerging Markets: Evidence from India." *Journal of Financial Intermediation*, 19, 26–51.

Grail Research. (2009) "Global Financial Crisis Bailout/Stimulus Tracker." Technical report, Grail Research, Cambridge, MA.

Imbens, Guido W., and Jeffrey M. Wooldridge. (2009) "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature*, 47, 5–86.

Khwaja, Asim Ijaz, and Atif R. Mian. (2008) "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review*, 98, 1413–42.

Laeven, Luc A.H., and Fabián Valencia. (2013) "Systemic Banking Crises Database." *IMF Economic Review*, 61, 225–70.

Lechner, Michael. (2010) "The Estimation of Causal Effects by Difference-in-Difference Methods." *Foundations and Trends in Econometrics*, 4, 165–224.

Martínez-Peria, Maria Soledad, Andrew Powell, and Ivanna Vladovka-Hollar. (2005) "Banking on Foreigners: The Behavior of International Bank Claims on Latin America, 1985–2000." *IMF Staff Papers*, 52, 430–61.

Morgan, Donald, Bertrand Rime, and Philip E. Strahan. (2004) "Bank Integration and State Business Cycles." *Quarterly Journal of Economics*, 119, 1555–84.

Ongena, Steven, José-Luis Peydró Alcalde, and Neeltje van Horen. (2015) "Shocks Abroad, Pain at Home? Bank-Firm Level Evidence on Financial Contagion During the 2007–2009 Crisis." *IMF Economic Review*, 63, 698–750.

Ongena, Steven, Alexander A. Popov, and Gregory F. Udell. (2013) "When the Cat's away the Mice Will Play: Does Regulation at Home Affect Bank Risk-Taking Abroad?" *Journal of Financial Economics*, 108, 727–50.

Peek, Joe, and Eric S. Rosengren. (1997) "The International Transmission of Financial Shocks: The Case of Japan." *American Economic Review*, 87, 495–505.

Peek, Joe, and Eric S. Rosengren. (2000) "Collateral Damage: Effects of the Japanese Bank Crisis on Real Activity in the United States." *American Economic Review*, 90, 30–44.

Popov, Alexander, and Gregory F. Udell. (2012) "Cross-Border Banking, Credit Access, and the Financial Crisis." *Journal of International Economics*, 87, 147–61.

Reinhart, Carmen M., and Kenneth S. Rogoff. (2009) *This Time Is Different: Eight Centuries of Financial Folly*. Princeton, NJ: Princeton University Press.

Schnabl, Philipp. (2012) "The International Transmission of Bank Liquidity Shocks: Evidence from an Emerging Market." *Journal of Finance*, 67, 897–932.

Wu, Ji, Alina C. Luca, and Bang Nam Jeon. (2011) "Foreign Bank Penetration and the Lending Channel in Emerging Economies: Evidence from Bank-Level Panel Data." *Journal of International Money and Finance*, 30, 1128–56.

SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's website.