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ABSTRACT

Using data from British and American banks, we provide empirical evidence that government intervention affects banking globalization along three dimensions: depth, breadth and persistence. We examine depth by studying whether a bank's preference for domestic, as opposed to external, lending (funding) changes when it is subjected to a large public intervention, such as bank nationalization. Our results suggest that, following nationalization, non-British banks allocate their lending away from the UK and increase their external funding. Second, we find that nationalized banks from the same country tend to have portfolios of foreign assets that are spread across countries in a way that is far more similar than either private banks from the same country or nationalized banks from different countries, consistent with an impact on the breadth of globalization. Third, we study the Troubled Asset Relief Program (TARP) to examine the persistence of the effect of large government interventions. We find weak evidence that upon entry into the TARP, foreign lending declines but domestic does not. This effect is observable at the aggregate level, and seems to disappear upon TARP exit. Collectively, this evidence suggests that large government interventions affect the depth and breadth of banking globalization, but may not persist after public interventions are unwound.

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1. Introduction

International financial intermediation has changed significantly since the 2007-2009 global financial crisis: portfolio flows have taken up the slack left by the collapse in bank lending.[1] Several reasons have been proposed for this development, including a) a rise in bank regulation, b) weakness in loan demand, and c) political interference in banking as a result of government intervention.[2] In this paper, we explore the last explanation.

We hypothesize that public intervention can affect the amount (*depth)*, cross-country allocation (*breadth)*, and *persistence* of banks' external activities. We employ three different empirical approaches. First, we use the same bank-level balance sheet data from the Bank of England that Rose and Wieladek (2014) used to analyze the effect of bank nationalization on *assets*, but we examine the depth effects of large public interventions on the composition of domestic and foreign bank *liabilities*. Second, we assess to what extent bank nationalization leads to the convergence (*breadth)* of banks' cross-border portfolios when the affected banks are from the same country. Finally, we study whether these effects persist or disappear following the unwinding of government interventions. Given the significant number of foreign and domestic banks with substantial lending to/funding from a large and diverse number of countries, the UK banking system provides an ideal empirical setup to test the first two hypotheses. To our knowledge, the only large recent public capital injection that has been unwound on a wide scale is the American Troubled Asset Relief Program (TARP). Accordingly, we test the *persistence* hypothesis using American data.

Government intervention may affect the *depth* of banking globalization. On the asset side of a bank's balance sheet, a disproportionate reduction in external (as opposed to domestic) lending following nationalization constitutes *prima facie* evidence of a negative impact on banking globalization, referred to as "financial protectionism" by Rose and Wieladek (2014). In a crisis, nationalized banks are

also perceived to be the safest home for deposits, as they are owned and backed by the government. Unsurprisingly, Berger and Roman (2015) find that the TARP gave participating institutions a competitive advantage in raising deposits, mainly because those banks were perceived to be safer. Similarly, following the introduction of TARP, Acharya and Mora (2015) document a "flight to safety" effect in that previously liquidity-constrained banks experienced an increase in deposits. This suggests that banks which received government aid may have been faced with an excess supply of deposits. That is, government intervention may induce banks to accept more domestic, as opposed to external, deposits. Our analysis of British data clearly indicates that government interventions affect banking globalization on both the asset and liability sides.

Large banks, such as those in our sample, usually lend (and borrow) in many different foreign countries. The mix of these assets (*breadth)* across countries differs by bank for a variety of reasons, often having to do with the particular regional or industrial expertise of the bank. For example, Standard Chartered is a large UK bank whose lending is primarily focused on Asia; Santander, a Spanish bank with substantial operations in Latin America, is now the third largest mortgage lender in the UK. For any given pair of banks, the similarity in exposure to different countries (which we refer to as 'the asset mix') can be measured quantitatively; we use the general measure of cosine similarity. We study similarity of the asset mix across pairs of banks, after abstracting from dyadic fixed effects and common trends. In particular, we ask if the asset mix converges *when a pair of banks from the same country is nationalized*. If the public authorities from a certain country who take charge of a bank upon nationalization simply shrink the size of the balance sheet of a bank, there is no reason to expect the asset mix for a pair of nationalized banks from the same country to begin to converge. Similarly, if good and bad assets are randomly distributed across countries, banks will not begin to look more similar upon nationalization. Even if bad assets are concentrated in a particular country, there would be no reason why the reduction in lending should be different for nationalized (as opposed to private sector) banks.

But if the authorities impose their political preferences on nationalized banks, leading to a reduction in lending to a particular set of countries, then one might expect bank nationalization to result in cross-country asset portfolios that *diverge* if the nationalized banks are from different countries but *converge* if nationalized banks are from the same country. This is, in fact, exactly what we find in the data.

Finally, we examine the *persistence* of the depth effect. Unlike banks in other countries that received public support during the global financial crisis, most American banks have now repaid the funds they received through TARP. This makes the TARP an ideal intervention to study, since bias towards foreign lending could either persist or disappear following TARP exit. To allow us to assess whether banks' lending preferences were indeed affected by their presence in TARP, we compare the growth of foreign as opposed to domestic lending, upon TARP entry and exit. We find some evidence that banks seemed to discriminate against foreign lending after entry into TARP, but there is also evidence (albeit weaker) that this was reversed upon TARP exit. This is consistent with the idea that the effects of large public interventions (such as partial government bank ownership) dissipate after the intervention has ended. These effects are small, though not trivial. A counterfactual exercise suggests that aggregate US foreign lending would have been 3.3% higher in the absence of TARP. This suggests that once public interventions are unwound globally, growth rates in cross-border bank lending may return to those observed before the crisis. Although, all our TARP results on persistence are economically small and statistically marginal (in marked contrast to our British results on depth and breadth); they are consistent with temporary financial protectionism, and none are significantly inconsistent with the notion.

Our paper contributes to a growing literature that analyzes the impact of large public interventions on banks' financial activity. Most previous work examines the impact on *domestic* bank lending, resilience and deposits. For example, Giannetti and Laeven (2012) find that the collapse of the

global syndicated lending market can be partially explained by lenders rebalancing their portfolios in favor of domestic borrowers. Veronesi and Zingales (2010) study the costs and benefits of US banks' recapitalization following the first phase of the US TARP while Cornett, Li, and Tehranian (2013), Ivashina and Scharfstein (2010), and Duchin and Sosyura (2014) examine the effect on the type and quality of domestic lending by American banks following the receipt of TARP funds. In contrast, we examine the extent to which banks discriminate against foreign customers, whether borrowers or depositors, following large interventions such as nationalization or a public capital injection.

Our findings also contribute to the literature on international cross-border flows following the financial crisis which documents that internationally-active banks reduced their cross-border lending during the financial crisis in favor of domestic borrowers (Cerutti and Claessens, 2014; Cerutti, Claessens and Ratnovski, 2014, De Haas and Van Horen, 2012; Giannetti and Laeven, 2012). Our results suggest that large government interventions play a role in affecting the depth and breadth of external bank lending (banking globalization), but we also find some evidence, albeit weak, that these effects do not persist once government interventions are reversed.

2. Examining the Depth Effect for British Bank Assets and Liabilities

In this section, we provide evidence for the depth effect of bank assets and liabilities. Given the presence of a large number of domestic and foreign banks that engage in significant external lending, the UK banking system is an ideal place to test this hypothesis. In a similar vein, Rose and Wieladek (2014) examined the asset side of the depth effect and estimated the effects of large public interventions on the "loan mix", defined as the ratio of foreign asset to total assets. We now re-estimate the model proposed in their paper with an updated data set below. More importantly, we also estimate the effect of large public interventions on the analogous "liability mix", which is defined as the

ratio of foreign liabilities to total liabilities. (For the sake of completeness, we also use the difference between the two to construct net foreign exposure.) To account for the many reasons loan and liabilities mixes vary across time and banks, we include comprehensive sets of bank- and time-specific fixed effects.

Data and Methodology

Our investigation uses quarterly data from the Bank of England, covering all banks doing business in the UK. London's status as a major international financial center means that the UK's banking system has considerable diversity, which enables us to examine the impact of government interventions on banking globalization for British and foreign banks. While the data are confidential, the scope and coverage of these data mean that this data set is better suited to test our hypothesis of interest than any other dataset, to the best if our knowledge. Our data set includes complete balance sheet data for every institution in the UK banking sector; in particular, we rely on series from the Bank of England's BT, AL, CC and CL forms (details are relegated to the data appendix).[3] Our data set also includes hand-collected series of large public banking interventions, in particular: 1) nationalizations, 2) injections of public capital, and 3) unusual access to loans, guarantees, or liquidity. We focus on bank nationalizations in our empirical work, since this is a clear-cut, discrete, one-time event; it is also the most obvious public intervention likely to result in detectable impact. Our panel of data covers 334 banks between 1999Q1 and 2011Q4. Our data set has three disadvantages: a) the data set is available at the level of individual banks, precluding analysis of bilateral relationships between individual banks and borrowers; b) the data set covers only the part of the banking group which is UK-resident; and c) the data set is confidential. Further details on the data set are contained in Rose and Wieladek (2014). The only new variable of substance compared to Rose and Wieladek (2014) is the pair of regressands that involve liabilities and net exposures. In our dataset, total liabilities are defined as the sum of all bank

funding, including capital. External liabilities are defined as total funding due to non-residents.[4] We use the ratio of the latter with respect to the former as the dependent variable in our regression model.

We analyze our data set using the following simple panel data model:

$$For_{i,t}/(Dom_{i,t}+For_{i,t}) = \alpha_i + \beta_t + \gamma_{UK} Nat_{UK,i,t} + \gamma_{FOR} Nat_{FOR,i,t} + \theta_{UK} LL_{UK,i,t} + \theta_{FOR} LL_{FOR,i,t}$$

$$+ \zeta_{UK} Cap_{UK,i,t} + \zeta_{FOR} Cap_{FOR,i,t} + \varepsilon_{i,t,} \qquad (1)$$

where:

- $For_{i,t}$ is lending to (or borrowing from) foreign residents by bank *i* at time *t*;
- $Dom_{i,t}$ is lending to (or borrowing from) domestic (British) residents by bank *i* at time *t*;
- α_i is a set of bank-specific fixed effects;
- β_t is an analogous set of time fixed effects;
- $Nat_{UK,i,t}$ is a dummy variable that takes a value of one when British bank *i* is nationalized at or before time *t*, and zero otherwise;
- $Nat_{FOR,i,t}$ is a dummy variable that takes a value of one when a foreign bank *i* is nationalized at or before time t and zero otherwise;
- $LL_{UK,i,t}$, $LL_{FOR,i,t}$, $Cap_{UK,i,t}$ and $Cap_{FOR,i,t}$ are analogues for banks that receive unusual access to liquidity or loan guarantees (*LL*) or are the recipients of public capital injections (*Cap*);
- $\varepsilon_{i,t}$ is a well-behaved disturbance term; and
- γ_{UK}, $\gamma_{FOR,}$ θ_{UK}, θ_{FOR}, ζ_{UK}, and ζ_{FOR} are coefficients.

The coefficients of greatest interest to us are γ_{UK} and γ_{FOR}, which measure the *permanent* effect of bank nationalization on the mix of foreign to total borrowing (or lending). We note that our regressands are unaffected by proportionate changes in *total* lending (or borrowing). Our approach therefore picks up

changes in the *composition*, rather than the *quantity*, of lending. We estimate our model with least squares; the presence of comprehensive sets of both bank- and time-specific fixed effects means that our model can be interpreted as a difference-in-differences estimator, comparing the foreign mixes of assets (and liabilities) for nationalized and private banks, before and after nationalization. Foreign banking activity may be intrinsically more "footloose" than domestic; the question we ask is whether there is a systematic difference between banks that were nationalized and those that remained private.

Results

Our default results are tabulated in Table 1. Each column presents key coefficient estimates from (1) with a different regressand (tabulated in the top row). Perhaps the effects of greatest interest to us are those in the top row, representing estimates of γ, the permanent effect of foreign bank nationalization on the ratio of foreign to total assets, liabilities and net exposures. Consider the top-left coefficient; this indicates that foreign bank nationalization raises the mix of foreign to total assets by around 15%. This is an economically significant amount, comparable to those of Rose and Wieladek (2014), and is consistent with the hypothesis that government intervention has an adverse impact on banking globalization; external lending is cut back more than domestic lending. The effect is statistically significant; the robust t-ratio is 2.35, inconsistent with the null hypothesis of no effect at the 2% significance level. It is striking that the effect of nationalization of a *British* bank is insignificant in both the economic and statistical senses. Indeed, the only other statistically significant effect on the asset mix is the much smaller effect of a capital injection into a British bank; this effect is economically smaller; it also turns out to be more fragile. All these results are consistent with those presented in Rose and Wieladek (2014).

What of the liabilities side? Here we find similar, and again striking, results. When a foreign bank is nationalized, it increases the fraction of its foreign liabilities by around 14%, almost exactly the

same as the increase in foreign assets; this is an effect that is both statistically and economically significant. Not only do nationalized foreign banks tilt their lending practices away from the UK; they also tilt their borrowing away, and to a similar degree. These results are not mechanically implied by those on the asset side. While, in an accounting sense, total assets need to equal total liabilities plus shareholders' equity, there can be stark differences in the composition because assets in one country can be financed with liabilities from another. In the presence of time effects, our results on the liabilities side can be interpreted as reflecting a bank's demand for UK versus foreign deposits. And in times of uncertainty, nationalized banks provide the safest home for deposits essentially by definition. A rise in foreign nationalized bank preference for foreign, as opposed to British, deposits is therefore consistent with the idea that government interventions may have an adverse impact on banking globalization.[5] Since the mix of foreign assets and liabilities increases similarly upon nationalization, it is little surprise that there is no significant effect of nationalization on the net foreign exposure of the nationalized banks. We also find an economically smaller (but statistically significant) effect of foreign capital injections on the ratio of foreign to total liabilities.

In Table 2, we show that our results of γ_{UK} and γ_{FOR} are insensitive to a large number of perturbations to the exact estimation technique. We record our default nationalization estimates in the top row to ease comparison. We then tabulate four types of robustness checks to account for sensitivity with respect to: a) the precise measurement of the dependent variable; b) the sample; c) the addition of controls; and d) the estimation technique.

The first set of (three) checks accounts for possible mismeasurement in our regressands. First, we replace with unity, all observations where the ratio of foreign to total asset/liabilities exceeds one. Second, we winsorize the series at the 1%- and 99%-levels. Finally, we drop all observations that lie outside the (1%, 99%) range of our data. None of these checks alters our conclusions.

Our next set of (six) tests split the data set in different ways to see if our results are particularly dependent on a certain set of observations. First, we drop all observations where the residual lies more than 3 (2.5) standard errors from zero. We then successively drop early observations (from the first two years of our sample), and late observations (the last two years). We also drop small banks (those whose size, defined as total assets, lies below the 10th percentile), and large banks (those whose size lies above the 90th percentile). Our results also seem insensitive to the exact sample. If the late observations are removed, our liability results remain economically large but (unsurprisingly) become indistinguishable from zero; dropping the large banks also renders the British asset results significant (although the relevant sample of nationalizations also becomes small). Even taking into account these minor issues, our results are essentially insensitive to reasonable changes in the exact sample.

Our third set of tests adds (four) controls to our regressions. In particular, we include: a) the ratio of domestic retail to total liabilities, to control for differences in funding models across different institutions; b) liquidity (measured as cash, market loans, bills, short-term paper and reverse repos); c) size (measured as the natural log of total assets), and d) capitalization (measured as the ratio of capital, defined as capital and other funds to total assets).[6] In a separate line, we also add a) profitability (measured by operating profits as a ratio of total assets), as well as b) the return on assets; we do this separately since these data are not available for all banks so that including them reduces the sample size considerably.[7] Adding these controls reduces the size of the effects, but the coefficients on foreign nationalization remain economically and statistically significant, in contrast to those on British nationalization.

Our final set of (six) robustness checks varies the estimation technique. First, we weight our observations by bank size; this reduces the asset estimates to insignificance but increases the precision of our British liability effect to the point where it is statistically significant (if fragile). Next, we replace

our bank-specific fixed-effects with random effects; then we drop them altogether (a dubious exercise, in our minds). For symmetry, we also drop time effects. Our default standard errors are robust and clustered by bank; we provide conventional standard errors in another line. Our final check is to estimate the equation with Tobit instead of least squares.[8]

Generally, our results are resilient to a wide battery of sensitivity checks. In particular, we find that the effect of foreign nationalization on the ratios of both foreign to total assets and foreign to total liabilities is economically large (around fifteen percent), statistically significant, and similar. In the presence of time effects, this second set of results can be interpreted as a reflection of banks' demand for deposits. The fact that nationalized banks, which are likely to be faced with an excess supply of deposit as a result of their perceived safety, discriminate against foreign depositors is new and additional evidence that government interventions have an impact on banking globalization. However, we find no reliable evidence that nationalization of *British* banks has any substantial effect on these ratios. Our findings are also consistent with prior studies that document that foreign banks reduce their lending compared to domestic banks unless they are funded locally (Claessens and Van Horen, 2013; Cull and Martinez Peria, 2013).

3. Examining the Breadth Effect for British Assets and Liabilities

Banks' foreign assets are often spread across many countries. We have already established that in the presence of large public interventions – such as bank nationalizations – the composition of a bank's balance sheet (both on the asset and liability side) tilts away from British to foreign activity, at least for non-British banks. We now ask a related question: do portfolios of nationalized banks become more alike? In particular, we ask whether a given bank's cross-country portfolio mix converges (to resemble the portfolio of other nationalized banks) or diverges in the wake of nationalization. This

would suggest the officials who take charge of nationalized banks make them divest in a similar fashion. We interpret convergence as evidence of government interventions that limits the *breadth* of banking globalization.[9] The fact that banks resident in the UK (both domestic and foreign) lend to an average of 53 countries makes the British banking system particularly suitable to test this hypothesis.

Similarity across Banks of Cross-Country Portfolios

The spread of foreign assets across countries differs by bank for a variety of different reasons, often having to do with the particular regional or industrial expertise of the bank. For example, HSBC is a UK-owned bank that has substantial operations in Asia, while Yorkshire Bank is a subsidiary of National Australia Bank that specializes in UK mortgage lending, with little overseas activity. A bank's cross-country mix of foreign assets (hereafter a bank's "cross-country portfolio mix") also changes over time, as circumstances, opportunities, and bank preferences change. We examine the similarity of the mixes for a pair of banks at a point in time. In particular, we examine asset cosine similarity for a pair of banks, measured as:

$$COSA_{i,j,t} \equiv [\Sigma_k \, Assets(k)_{i,t} \cdot Assets(k)_{j,t}] / \{[\Sigma_k (Assets(k)_{i,t})^2]^{.5}\} \{[\Sigma_k (Assets(k)_{j,t})^2]^{.5}\} \qquad (2)$$

where $Assets(k)_{i,t}$ denotes the assets that bank *i* holds in country *k* at time *t*, taken from the CC1 form collected by the Bank of England. The cosine similarity of assets for a pair of banks *i* and *j* at time *t* is the inner (dot) product of their assets across countries, normalized by their asset holdings. Cosine similarities vary in principle between -1 (meaning exactly opposite) to +1 (meaning identical). Since all our terms are positive, in our data set, they vary between 0 (unrelated cross-country portfolios) and 1 (identical cross-country portfolios).

Our data set contains foreign asset holdings for an individual bank in a given quarter for a large number of foreign countries (indexed by k). The data are taken from lines 16-249 in CC1 form.

However, the vast majority of these countries are unimportant loan destinations, and our data set registers a missing value if foreign assets, for a given bank in a given country, are less than half a million British pounds. As a result, there are many missing observations, at times every other time-period, for countries that only have a small presence in the foreign portfolio of the banks that we study. Accordingly, we limit our scope to the 40 most important overseas destinations for foreign loans.[10] As our default, we require that the pair of banks (*i* and *j*) share observations from a minimum of 30 countries (that is, at least 30 *k*'s from the 40 possible) in which they have foreign assets to compute the cosine similarity; we check the sensitivity to this threshold below.

Cosine similarity of foreign assets for a randomly chosen pair of banks might not be expected to change much upon nationalization of one of the banks, at least if the events which led to nationalization were independent of the cross-country mix of exposures. However, if, say, losses associated with the global financial crisis were concentrated in sub-prime mortgages issued only in the United States, then it seems reasonable to expect banks to reduce asset exposure to the American market, so that cosine similarities might increase. Of course, such behavior might be expected of *all* banks, not just nationalized banks; we account for this feature by including a comprehensive set of time effects. Since we also include comprehensive dyadic (bank-pair specific) fixed effects, our coefficient estimates will only reflect idiosyncratic within-dyad time-series variation.

What if *both* banks in the pair are nationalized? (We have 354 such observations in our sample.) The idea that government intervention may have an adverse impact on the breadth of banking globalization leads us to two testable hypotheses. We expect the similarity of a pair of nationalized banks' cross-country portfolio mixes to *diverge* if the nationalized banks are from different countries, but *converge* if they are from the same country. It is easiest to consider this with a simple example. Suppose that the authorities from country *x* encourage divestment from countries *a*, *b*, and *c* and

investment in countries *d*, *e*, and *f*, while the authorities from country *y* have the opposite preferences, since national interests differ. If authorities imposed their geographical lending preferences on nationalized banks, then we would expect the cross-country portfolio mixes to diverge when banks from countries *x* and *y* are nationalized; their cosine similarities should fall. But if both nationalized banks are from the same country (we have 55 such observations), we would expect the opposite; their cross-country portfolios should converge and cosine similarities rise.

Accordingly, we estimate the following regression model:

$$COSA_{i,j,t} = \alpha_{i,j} + \beta_t + \gamma OneNat_{i,j,t} + \psi BothNat_{i,j,t} + \phi SCBothNat_{i,j,t} + v_{i,j,t} \quad (3)$$

where

- $COSA_{i,j,t}$ is the cosine similarity of foreign assets (across countries) for a pair of banks *i* and *j* at time *t* (a histogram can be found in Figure 1),
- $\alpha_{i,j}$ is a comprehensive set of dyadic bank pair-specific fixed effects;
- β_t is an analogous set of time fixed effects;
- $OneNat_{i,j,t}$ is a dummy variable which is one if either bank *i* or bank *j* (but not both) is nationalized at or before time *t*;
- $BothNat_{i,j,t}$ is a dummy variable which is one if both bank *i* and bank *j* are nationalized at or before time *t*;
- $SCBothNat_{i,j,t}$ is a dummy variable which is one if both bank *i* and bank *j* are nationalized at or before time *t* and both banks are from the same country; and
- $v_{i,j,t}$ represents the myriad of omitted factors determining the similarity of the cross-country portfolio mix for a pair of banks.

The parameters of interest are ψ and ϕ. If both banks *i* and *j* are nationalized and the home countries of *i* and *j* are engaged in different kinds of financial protectionism, one expects ψ to be negative, since the

cross-country portfolio mixes diverge upon banks nationalization. But if the banks are from the same country, then financial protectionism would be manifest in convergence of the cosine similarity of the assets, so we expect ϕ and $(\phi+\psi)$ to be positive.

Results

Estimates of (3) are tabulated in Table 3 below. Our default estimates are computed with least squares and are provided in the top row.

The effect of a single bank's nationalization has little detectable effect on the similarity of banks' assets (γ), as expected. However, if both banks are nationalized, then the similarity of banks' cross-country portfolio mixes falls by an economically and statistically large amount. Since the average value of cosine similarity is .46 (with a standard deviation of .35), our estimate of ψ=-.22 means that cosine similarity is approximately halved when both banks are nationalized, an economically large amount. The estimate is also statistically significant, having a t-statistic exceeding 11. Perhaps more striking is the fact that ϕ is estimated to be positive and even larger (in absolute value) at .31, again an amount that is both economically and statistically significant. That is, the similarity of a pair of banks' cross-country portfolio mixes is significantly lower when both banks are nationalized, but this effect is more than completely offset if the banks are from the same country (a p-value for the hypothesis of no combined effect is tabulated at the right of the table). These results are consistent with the idea that the external lending preferences of banks from the same country converge after nationalization. This is exactly what one would expect if authorities impose their lending preferences on nationalized institutions, so we view this as indirect support for the hypothesis that government intervention affects the international breadth of banking globalization. This finding is also consistent with prior studies which document a "flight-to-home" or "flight-to-core-markets" effect during the financial crisis by banks engaged in cross-

border lending (Giannetti and Laeven, 2012; De Haas and Van Horen, 2013; Ongena, Peydro and Van Horen, 2013).

The remainder of the table contains sensitivity analysis, to show that our results are insensitive to minor perturbations of our methodology. Our first set of (five) checks changes the dependent variable. Our default measure of cosine similarity relies on having observations on assets for both banks (*i* and *j*) for at least thirty countries (*k*) of the forty countries possible. We test the importance of this threshold by varying it from 26 to 34 in successive rows. This has little effect on our results; ϕ is positive, statistically significant, and larger than ψ in all cases; their sum is significantly positive in all cases except the first (which is the least demanding in terms of observations).

We next substitute as a regressand the analogue to asset cosine similarity but using foreign liabilities in place of assets:

$$COSL_{i,j,t} \equiv [\Sigma_k\, Liab(k)_{i,t} \cdot Liab(k)_{j,t}] / \{[\Sigma_k (Liab(k)_{i,t})^2]^{.5}\} \{[\Sigma_k (Liab(k)_{j,t})^2]^{.5}\} \qquad (2')$$

where $Liab(k)_{i,t}$ denotes the liabilities that bank *i* owes in country *k* at time *t*. The results here are similar in economic direction and magnitude but lack statistical significance at any reasonable confidence level; that is, this manifestation of financial protectionism is visible *only* in assets, not liabilities.

Our next set of (six) checks changes the estimation strategy. First, we weight observations by the number of country-observations (*k*) that underlie the dependent variable. Since the regressand is constrained to be positive but less than unity, we also estimate our equation with a censored (Tobit-like) technique. We then show how our results change if we use conventional standard errors in place of robust ones. We also replace our dyadic bank pair-specific fixed effects with random effects; then we drop pair effects altogether. For symmetry, we also drop time effects. None of these perturbations change the confidence levels much. The sum of ψ and ϕ becomes insignificant if we use either conventional standard errors or a regression without dyadic effects. But in all cases, our estimates of

both ψ and ϕ are correctly signed, and both economically and statistically significant with $\phi>|\psi|$; that is, our results are insensitive to these various perturbations.

Third, we check the sensitivity of our estimates by varying the precise sample of observations. Successively, we drop: a) early observations (defined as those from 2001 or earlier); b) observations with small banks (defined as a pair where either or both banks have less than £2 billion in assets, where the average bank size in our sample is £2.6 billion); and c) observations where the residual lies more than 2.5 standard deviations from 0. Essentially all our key results are robust; when we drop the early observations in our sample ($\phi+\psi$) remains positive but statistically insignificant.

Finally, we add some controls to the right-hand side of (3). Since we are interested in understanding the similarity between the cross-country portfolios of a pair of banks, we examine (the absolute value of) differences between the pair of banks in terms of fundamental bank characteristics. Our first set of (five) controls includes: a) size (measured as the natural logarithm of total assets); b) wholesale funding dependency (measured as the ratio of repos to total liabilities); c) the loan/deposit ratio; d) liquidity (measured as the ratio of [cash + market loans + bills + short-term paper + reverse repos] to total assets); and e) capital adequacy (measured as the ratio of capital to total assets). Adding these controls has little effect on our results, which remain essentially unchanged. The final row adds a further pair of controls; since these are only available for a limited number of observations, the sample size is reduced considerably (from almost 59,000 to less than 26,000). The two extra controls are (as always, measured as absolute values of banks' differences): a) net interest (the ratio of net interest income to total assets); and b) profitability (measured as the ratio of operating profits to total assets).[11] These controls, in conjunction with the reduced sample leave correctly signed and statistically significant estimates of ϕ and ψ but the sum of these is now indistinguishable from zero, and the magnitudes are reversed. All in all, we find these results reassuring.

We conclude from all this that our evidence on the similarity of cross-country portfolios before and after bank nationalization constitutes evidence for the idea that government ownership may have an adverse impact on the cross-country *breadth* of banking globalization. This evidence is only indirect. Nevertheless, it is new, economically and statistically strong, and consistent with the idea that the authorities impose nationalistic loan preferences on nationalized banks. More importantly, the reduction in the breadth of financial globalization is likely to be associated with an increase in systemic risk since banks have less diversified portfolios, a troubling consequence of bank nationalization.[12]

4. Examining Persistence with the TARP

If government ownership is indeed behind the adverse impact on the breadth and depth of banking globalization, then one might expect these effects to disappear once a bank is sold back to private owners; at that point, profits replace politics as the overarching motive. Most of the banks operating in the UK are still affected by large government interventions, so it is not possible to test the *persistence* hypothesis with British data. But a number of banks that received TARP funds have already repaid them and left the program.[13] We therefore test the persistence hypothesis with American data; taking advantage of bank-level data publicly available from the Federal Reserve Bank of Chicago (and used in numerous studies including Kashyap and Stein, 2000 and Cetorelli and Goldberg, 2012).[14]

The TARP was an American government program designed to purchase assets on banks' balance sheets in exchange for an equity or an equity-like stake in their operations. To participate in the TARP, banks must have been deemed healthy by the regulators and agreed to restrictions on dividends and executive compensation. One objective of this policy was to improve resilience and stability in the US banking system.[15] The second objective of TARP was to "encourage US financial institutions to build capital to increase the flow of financing to US business and consumers and support the US economy."[16] Black and Hazelwood (2013) find that large banks, classified as those with in excess of $10 billion in

assets, originated relatively riskier loans when those banks entered TARP. However, they do not find an impact on aggregate lending, which suggests that banks lent to riskier borrowers than they would have in the absence of the TARP. In other words, without the TARP, lending by the affected banks would have probably declined. This is consistent with Contessi and Francis (2011), who analyze aggregate lending flows and show that commercial banks participating in the TARP increased lending compared to other domestic commercial banks.[17]

Taken together, the findings from the literature suggest that the TARP allowed participating banks to build resilience, while simultaneously expanding loan supply. Bank capital ratios can be raised through a) retained earnings, b) raising equity from outside investors or c) cutting back on (risk-weighted assets). The first is a slow process, especially after a financial crisis; crises also tend to increase the cost of raising equity.[18] Reducing assets is therefore typically the most *immediate* channel of adjustment. Since some types of domestic lending expanded as a result of the TARP, other types of lending might have shrunk. All this suggests that it is plausible to think that American banks contracted foreign lending, perhaps as an unintended consequence of political pressure to rebuild their capital-asset ratios, while simultaneously maintaining or raising domestic lending (consistent with the intra-portfolio risk-shifting found by Duchin and Sosyura, 2014). We search for this effect below. We also exploit another noteworthy feature of the TARP, namely exit. If banks were subjected to political pressure and as a result (whether unintended or not) cut back on foreign lending while in TARP, then these effects should become weaker following exit from TARP. Thus, TARP seems like an ideal program to test for the persistence effect of large government interventions on banking globalization.

We estimate the following model to examine the effects of TARP entry and exit:

$$\Delta L_{i,t} = \alpha_i + \beta_t + \gamma TARPEntry_{i,t} + \theta TARPExit_{i,t} + \varepsilon_{i,t}, \quad (4)$$

where:

- $\Delta L_{i,t}$ is the growth rate of lending;

- α_i is a comprehensive set of bank-specific fixed effects;
- β_t is an analogous set of time fixed effects;
- $TARPEntry_{i,t}$ is a dummy variable that takes a value of one when bank *i* receives TARP funds at or before time *t* and is zero otherwise;
- $TARPExit_{i,t}$ is an analogous dummy variable that takes a value of one when bank *i* repays TARP funds at or before time *t* and zero otherwise;[19]
- $\varepsilon_{i,t}$ is a well-behaved disturbance term; and
- γ and θ are the coefficients of interest.

Two dependent variables are of interest to us, the growth rates of domestic and foreign loans. We create these from the consolidated financial statements of bank holding companies (FR Y-9C) reports available from the website of the Federal Reserve Bank of Chicago (defining foreign lending as the difference between domestic and total lending); we also use form FR Y9-C for balance sheet control variables. Our dataset is quarterly and starts in 1991Q1. Data on which banks received TARP funds, the amounts they received, and when they repaid these funds, were obtained from the website of the US Treasury. We use data at the level of the bank holding company; further details are available in the Data Appendix.[20]

One potential issue with testing for the impact of government intervention on global lending flows with these data is the paucity of banks with substantial amounts of foreign lending. Few American banks do any substantial foreign lending, so that the American analogue of the loan mix of (1) is heavily skewed towards zero. The growth rates of domestic and foreign lending, on the other hand, are almost normally distributed, which is why we choose these as our dependent variables, rather than the loan mix.[21, 22]

We estimate our default model for banks in the top 5% of the asset distribution.[23] Cetorelli and Goldberg (2012) show that large global banks actively use their foreign subsidiaries to circumvent liquidity shocks associated with US monetary policy.[24] We wish to examine banks whose foreign operations are substantive, so that contracting them can make a difference to the bank's ratio of capital to risk-weighted assets; thus large banks seem to be the right set to examine. We conduct sensitivity analysis to check for robustness.

The coefficients of interest to us are γ and θ. If entry into TARP led banks to expand domestic lending at the expense of foreign lending, then we expect a positive (negative) sign on γ when domestic (foreign) loan growth is the dependent variable. If the impact of TARP wears off after exit, we expect θ to have similar magnitude but opposite sign.

Table 4 shows estimates of equation (4). The default estimates are provided in the top row. The effect of TARP entry on domestic loan growth is small and not statistically different from zero. In contrast, the effect on foreign lending growth is negative and significantly different from zero at the 5% (but not the 1%) confidence level. The 4% foreign loan contraction associated with TARP entry is non-trivial but smaller than the asset mix results we document for the UK. The coefficients for TARP exit are economically and statistically small for both domestic and foreign loan growth. The sum of entry and exit coefficients is not statistically different from zero, both for domestic and foreign loan growth, suggesting that the effects of TARP wear off upon exit. However, we do not wish to over-interpret this result since almost all the estimates are economically and statistically small. Overall, they provide weak but limited evidence in favor of our hypotheses, and no evidence inconsistent with our views.

The other rows of Table 4 show that our default results are robust to several perturbations of the baseline methodology. Our first set of perturbations changes the sample. First, we restrict our attention to even larger banks, defined as those in the top-1% in terms of total assets (instead of the

top-5%). This delivers somewhat stronger results in line with our priors; TARP entry reduces foreign loan growth significantly, while TARP exit reduces domestic loan growth. Next, we switch tack and include all banks in the sample. In that case, the negative coefficient measuring the effect of TARP on foreign loan growth becomes smaller and insignificant, but the effect on domestic lending becomes positive and statistically significant. This last finding is consistent with previous work that finds a positive domestic loan supply of TARP, since those papers typically use all of the banks in the sample. In both experiments, the effect of TARP entry is significantly higher for domestic than foreign loan growth. Dropping observations from the big-6 banks (Bank of America, Citigroup, Goldman Sachs, JP Morgan Chase, Morgan Stanley, and Wells Fargo) has little effect on our point estimates, but reduces our precision such that nothing is statistically significant. Finally, we drop data from the 1990s and again find consistent, albeit weaker, results.

Our default measure of domestic loan growth only includes lending to households, commercial and industrial firms, since previous work by Garrica, Puddu and Waelchli (2013) found that TARP led to a loan expansion to those sectors. When we expand our definition of domestic loans to include mortgage loans to households, we still find robust results.[25] We then drop our fixed time-and bank-specific effects. Our final robustness check is to drop all outliers, defined as observations where the residual lies at least two standard deviations from zero. Throughout most of these perturbations, we find limited effects of either TARP entry or exit, though TARP entry often has a significantly negative effect on foreign loan growth, and the net effect of TARP entry and exit on both domestic and foreign loan growth is typically zero. Still, these are weak results in the large; there are few clear indications that the TARP had an economically or statistically significant effect one way or another.

Banks accessing TARP had to be in good standing with their respective regulators to receive TARP support; however, a potential concern remains that banks that received TARP funds were banks

that took excessive risks prior to the financial crisis and therefore were weaker banks. One of these potentially risky activities could have been exposure to cross-border lending. We address this concern in two ways: 1) by controlling for bank quality, and hence cost of their debt in the bond market, using banks' credit default swap data and 2) by constructing a propensity-score matched and weighted sample of banks that were not involved in foreign lending prior to the crisis as a control group.[26]

Credit default swaps (CDS) have been used in the literature as a measure of default risk since credit default spreads incorporate the likelihood of a firm's default. For a subset of banks for which CDS data are available, we observe that controlling for the CDS spread; we find weak results, though they are consistent with our default findings.[27]

Since not all banks have CDS contracts, we construct a propensity-score matched sample of banks that look similar to banks that are engaged in foreign lending prior to TARP. We then use the score from the propensity score estimate to construct a weight for the second stage regression. In selecting observables, for the propensity-score regression we use variables that are likely to capture the business model choices of a bank and estimate the following model:

$$For\_Lender_i = \alpha_0 + \alpha_1 Tier1_i + \alpha_2 NPL_i + \alpha_3 ROA_i + \alpha_4 Size_i + \alpha_5 RWA_i + \alpha_6 Trading\ Assets_i + \alpha_7 Real\ Estate_i + \varepsilon_{i,}, \quad (5)$$

where:

- For_Lender_i is the dummy variable which takes the value of one if a banks has foreign lending during the quarters of 2006Q1 to 2008Q2;
- $Tier1_i$ is a regulatory capital ratio measured as a ratio of Tier 1 capital to risk-weighted assets;
- NPL_i is a ratio of non-performing loans to total loans;
- ROA_i is a measure of a bank's profitability computed as a ratio of net income to total assets;

- $Size_i$ is a natural logarithm of total assets;
- RWA_i is a ratio of risk-weighted assets to total assets;
- $Trading\ Assets_i$ is a share of bank's trading assets in total assets;
- $Real\ Estate_i$ is a share of bank's real estate assets in total assets; and
- ε_i is a well-behaved disturbance term.

The results from this analysis are shown at the bottom of Table 4. We show weighted results both for our sample of large banks and for the entire sample of banks. Unfortunately, once again, results are weak.

Finally, we construct a counterfactual scenario. We use the default regression results estimated and tabulated in the top row of Table 4, as well as a counterfactual series constructed by setting γ and θ to zero. We then aggregate the individual foreign lending time-series up to the total value for the US banking system as a whole. Figure 2 presents our findings and shows that following the entry into TARP in October 2008, aggregate lending to foreign borrowers decreases by 16.8% by December 2008. In the absence of the TARP, however, this decrease would have been approximately 13.5%. Furthermore, during the TARP, the differential between actual and counterfactual levels of aggregate lending to foreign borrowers remains non-trivial, with an average difference of approximately 3% at the aggregate level over the entire period. This persistence arises, since our regression model is estimated in growth rates. As a result any effect in levels will be permanent. These findings show that, at the aggregate level, the decrease in aggregate lending to foreign borrowers was reasonably large during the period of TARP intervention.

Our findings suggest, but do not definitively show, that the TARP seemed to benefit domestic lending at the expense of foreign lending. This is consistent with Contessi and Francis (2011), who show that domestic aggregate lending by TARP recipients increased during the time they were in receipt of

TARP funds. To the best of our knowledge, we are the first to study the aggregate impact on lending to foreign borrowers. However, our results on persistence are weak, especially by way of contrast with our strong depth and breadth evidence; none of our persistence effects are overwhelmingly significant in either the statistical or economic senses.

In summary, we find that entry into TARP may plausibly have led to discrimination against foreign lending, but TARP exit means that this effect also may have worn off. Since it is theoretically plausible and there is empirical evidence that foreign loan discrimination may be exacerbated when governments have an equity stake in the bank, these results suggest that any financial protectionism may be transitory. However, our results regarding the impact of TARP, while pointing in the expected direction, are insufficiently strong and only provide tentative support to this view.

5. Summary and Conclusion

Following the financial crisis of 2007-2009, the growth in global banking has slowed significantly, and non-bank intermediation has risen to fill this gap. Is this the result of new frictions in the global banking system? Rose and Wieladek (2014) suggest financial protectionism –political preferences for greater domestic lending following bank nationalization, could lead to discrimination against foreign lending. In this paper, we expand upon previous work by examining three dimensions of this effect: depth, breadth and persistence.

We hypothesize that large government bank interventions can affect the *depth* of financial globalization (external lending) in terms of both the asset and liability side of banks' balance sheet. In times of crisis, nationalized banks constitute the safest home for deposits; to the extent that these banks are faced with an excess supply of deposits as a result, discrimination against foreign deposits is a

type of financial protectionism. We find strong evidence of this for foreign nationalized banks in the UK. Similarly, if cross-country asset allocation, the *breadth* of banking globalization, is determined by government preferences, banks' foreign asset allocations to a particular country should converge following nationalization. This is exactly what we find. Finally, if government ownership is an underlying reason for the retrenchment in foreign lending, this effect should not *persist* once the government ceases to own any part of the bank. We examine this hypothesis with American bank balance sheet data around the dates that banks entered and exited the TARP. We find weak evidence supporting the hypothesis and none opposing it.

Clearly, government ownership is not the only possible friction or reason why cross-border bank lending has remained stagnant since the 2007-2009 crisis (Forbes, 2014). In this paper, we show that government ownership could be an important friction inhibiting cross-border bank activity in both the UK and the US. We found weak evidence that any discrimination towards foreign lending by American banks wore off once TARP was exited, and nothing inconsistent with this claim. If the same mechanism applies to other countries around the world, and if government intervention is indeed an important friction, then global banking intermediation may rebound once again, when the current unconventional intervention measures are unwound.

Table 1: Effect of Large Public Interventions on External Assets and Liabilities

	Assets	Liabilities	Net
Foreign Nationalization	.15* (.06)	.14* (.06)	.02 (.08)
Foreign Capital Injection	.00 (.02)	.10** (.03)	-.09* (.04)
Foreign Access to Lending	.00 (.04)	.07 (.08)	-.01 (.09)
British Nationalization	-.03 (.02)	.03 (.09)	-.07 (.09)
British Capital Injection	-.05* (.02)	.02 (.06)	-.07 (.06)
British Access to Lending	.00 (.02)	-.01 (.03)	.01 (.03)

Least squares estimation, unless otherwise noted. As described in the text, the regressand in column one is the ratio of foreign assets to total assets (CC1/BT40), the regressand in column two is the ratio of foreign liabilities to total liabilities (CL1/BT20), and finally the regressand in column three is the foreign net exposure (asset minus liabilities) divided by total assets ((CC1-CL1)/BT40). Fixed bank- and time-specific effects are included but not recorded. Robust standard errors (clustered by bank) are recorded in parentheses. One (two) asterisk(s) indicate(s) coefficients significantly different from 0 at the .05 (.01) level. Panel of 334 banks, 1999Q1-2011Q4.

Table 2: Effect of Bank Nationalization on External Assets and Liabilities: Robustness

Regressand:	External/Total Assets		External/Total Liabilities	
	Foreign Nationalization	British Nationalization	Foreign Nationalization	British Nationalization
Default	.15*	-.03	.14*	.03
	(.06)	(.02)	(.06)	(.09)
Ignore >1	.14*	-.03	.13*	.03
	(.06)	(.02)	(.06)	(.09)
Winsorize	.14*	-.03	.12*	.03
	(.06)	(.02)	(.06)	(.09)
Only in (1,99)	.19**	-.04	.14*	.03
	(.03)	(.03)	(.06)	(.09)
Drop 3σ outliers	.14*	-.04	.14*	.02
	(.06)	(.02)	(.06)	(.08)
Drop 2.5σ outliers	.14*	-.04	.15**	-.01
	(.06)	(.02)	(.06)	(.05)
Drop early obs	.14*	-.03	.15**	.04
	(.06)	(.02)	(.05)	(.08)
Drop late obs	.13**	-.03	.13	-.01
	(.05)	(.02)	(.09)	(.05)
Drop small banks	.14*	-.04	.14*	.03
	(.06)	(.02)	(.06)	(.09)
Drop large banks	.15*	-.04**	.14*	.24
	(.06)	(.01)	(.06)	(.16)
Add controls	.12*	-.04	.14*	.04
	(.06)	(.03)	(.06)	(.09)
Also add profit (smaller sample)	.10*	-.05	.09*	.02
	(.05)	(.03)	(.03)	(.02)
Weight by size	.09	.00	.17*	-.04**
	(.06)	(.04)	(.08)	(.01)
Random bank effects	.15*	-.04	.14*	.03
	(.06)	(.02)	(.06)	(.09)
No bank effects	-.16**	-.14**	-.21**	-.19**
	(.03)	(.04)	(.02)	(.06)
No time effects	.20**	-.03	.15**	.04
	(.06)	(.02)	(.06)	(.08)
Conventional standard errors	.15**	-.03	.14**	.03
	(.02)	(.02)	(.02)	(.02)
Tobit (random effects)	.22**	-.04	.14**	.03
	(.02)	(.03)	(.02)	(.02)

Least squares estimation, unless otherwise noted. Foreign and domestic dummy variables for capital injection and unusual access to loans included in regressions but not recorded. Controls include: Deposit ratio (retail deposits to total liabilities, (BT2H+BT3H)/BT40), liquidity ratio ((BT21+BT32D)/BT40), log of size (natural logarithm of total assets, ln(BT40) and the ratio of capital/liabilities (BT19/BT20). Fixed bank- and time-specific effects are included but not recorded. Robust standard errors (clustered by bank) recorded in parentheses. One (two) asterisk(s) indicates coefficient significantly different from 0 at the .05 (.01) level.

Table 3: Effect of Bank Nationalizations on Cosine Similarity of Foreign Assets (across countries)

Nationalizations:	One (γ)	Both (ψ)	Both, Same Nation (ϕ)	Both + Both Same Nation ($\psi+\phi$) = 0 (p-value)
Default	-.01	-.22**	.31**	.00
	(.02)	(.02)	(.04)	
≥26 obs	-.02	-.14*	.18*	.49
	(.01)	(.07)	(.09)	
≥28 obs	-.02	-.18**	.28**	.00
	(.01)	(.06)	(.07)	
≥32 obs	-.01	-.21**	.31**	.00
	(.02)	(.02)	(.03)	
≥34 obs	.02	-.08*	.18**	.00
	(.02)	(.03)	(.04)	
Liabilities	-.03*	-.10	.10	.90
	(.01)	(.08)	(.10)	
Weight by observations	-.01	-.22**	.31**	.00
	(.02)	(.02)	(.03)	
Tobit	-.01*	-.22**	.31**	.00
	(.01)	(.04)	(.08)	
Conventional Standard Errors	-.01*	-.22**	.31**	.19
	(.01)	(.04)	(.08)	
Random Effects	-.01	-.22**	.31**	.00
	(.02)	(.02)	(.04)	
No pair effects	-.02	-.28**	.34**	.15
	(.02)	(.03)	(.05)	
No time effects	-.02	-.22**	.30**	.01
	(.02)	(.02)	(.04)	
Drop early obs	-.03*	-.19**	.21**	.57
	(.02)	(.02)	(.05)	
Drop small banks	-.01	-.22**	.32**	.00
	(.02)	(.02)	(.04)	
Drop 2.5σ outliers	-.01	-.21**	.30**	.00
	(.02)	(.02)	(.04)	
Add controls	-.01	-.22**	.31**	.00
	(.02)	(.02)	(.04)	
Add more controls	-.03	-.14**	.10*	.46
	(.02)	(.02)	(.05)	

Least squares estimation, unless otherwise noted. Cosine similarities computed with minimum of 30 countries except where marked. Bank-pair- and time- fixed effects are included except where noted. Controls include absolute value of difference between banks': a) log total assets; b) wholesale dependence ratio (repo liabilities/total liabilities); c) loan/deposit ratios; d) liquidity ratios ([cash+market loans+bills+ST paper+reverse repos]/assets, with outliers removed); and e) capital adequacy ratios (capital/total liabilities, with outliers removed). Extra controls added to bottom row include absolute value of difference between banks': a) net interest ratio (net interest income/total assets, with outliers removed); and b) profit ratio (operating profits/total assets, with outliers removed). One (two) asterisk(s) indicate(s) coefficient significantly different from 0 at the .05 (.01) level.

Table 4: Effect of TARP on Bank Lending Growth

	Domestic Loan Growth		Foreign Loan Growth		Coefficient Sums		
	TARP Entry	TARP Exit	TARP Entry	TARP Exit	Domestic Entry+Exit	Foreign Entry+Exit	Domestic-Foreign Entry
Default	-.01	-.03	-.04*	.02	-.04	-.02	.04
	(.02)	(.02)	(.02)	(.04)	(.03)	(.05)	(.02)
Bigger (1%) Banks	-.00	-.13*	-.07*	.01	-.13	-.06	.07*
	(.06)	(.07)	(.03)	(.05)	(.11)	(.06)	(.03)
All Banks	.04**	-.04*	-.05	.03	.00	-.01	.09*
	(.02)	(.01)	(.04)	(.03)	(.01)	(.02)	(.04)
No Big-6 Banks	-.01	-.02	-.04	.02	-.02	-.02	.04
	(.02)	(.02)	(.02)	(.04)	(.03)	(.06)	(.03)
After 2000	-.01	-.04	-.04*	.02	-.05	-.02	.03
	(.02)	(.02)	(.02)	(.04)	(.04)	(.05)	(.02)
HCI + Mortgage Loans	-.04	-.04*	-.04*	.02	-.08*	-.02	.01
	(.02)	(.02)	(.02)	(.04)	(.04)	(.05)	(.02)
Drop Time Effects	-.01	-.02	-.05*	.03	-.03**	-.02	.03
	(.01)	(.01)	(.02)	(.02)	(.01)	(.01)	(.02)
Drop Bank Effects	-.01	-.00	-.00	.04	-.00	-.03	-.00
	(.02)	(.02)	(.02)	(.03)	(.03)	(.04)	(.02)
Drop 2σ outliers	.00	.00	-.00	.03	.01	.03	.01
	(.01)	(.01)	(.02)	(.03)	(.02)	(.04)	(.02)
CDS Spread Control	.05*	-.04	-.02	.03	.01	.02	.07**
	(.01)	(.03)	(.02)	(.04)	(.04)	(.05)	(.02)
Weighted Sample (Top 5% Banks)	.01	-.02	-.04	.01	-.01	-.02	.04
	(.01)	(.01)	(.02)	(.04)	(.02)	(.05)	(.02)
Weighted Sample (All Banks)	.01*	.01**	-.02	-.00	.02**	-.02	.03
	(.00)	(.00)	(.02)	(.03)	(.01)	(.04)	(.02)

Least squares estimation. Regressand is first-difference in log of domestic (household, commercial and industrial) or foreign lending. Fixed bank- and time-specific effects are included but not recorded. Robust standard errors (clustered by bank) recorded in parentheses. One (two) asterisk(s) indicate(s) coefficient significantly different from 0 at the .05 (.01) level. Quarterly panel data for US banks, 1991Q1-2012Q4; default sample restricted to 138 large banks (in top 5% of assets distribution) with foreign loans and |quarterly loan growth|<100%.

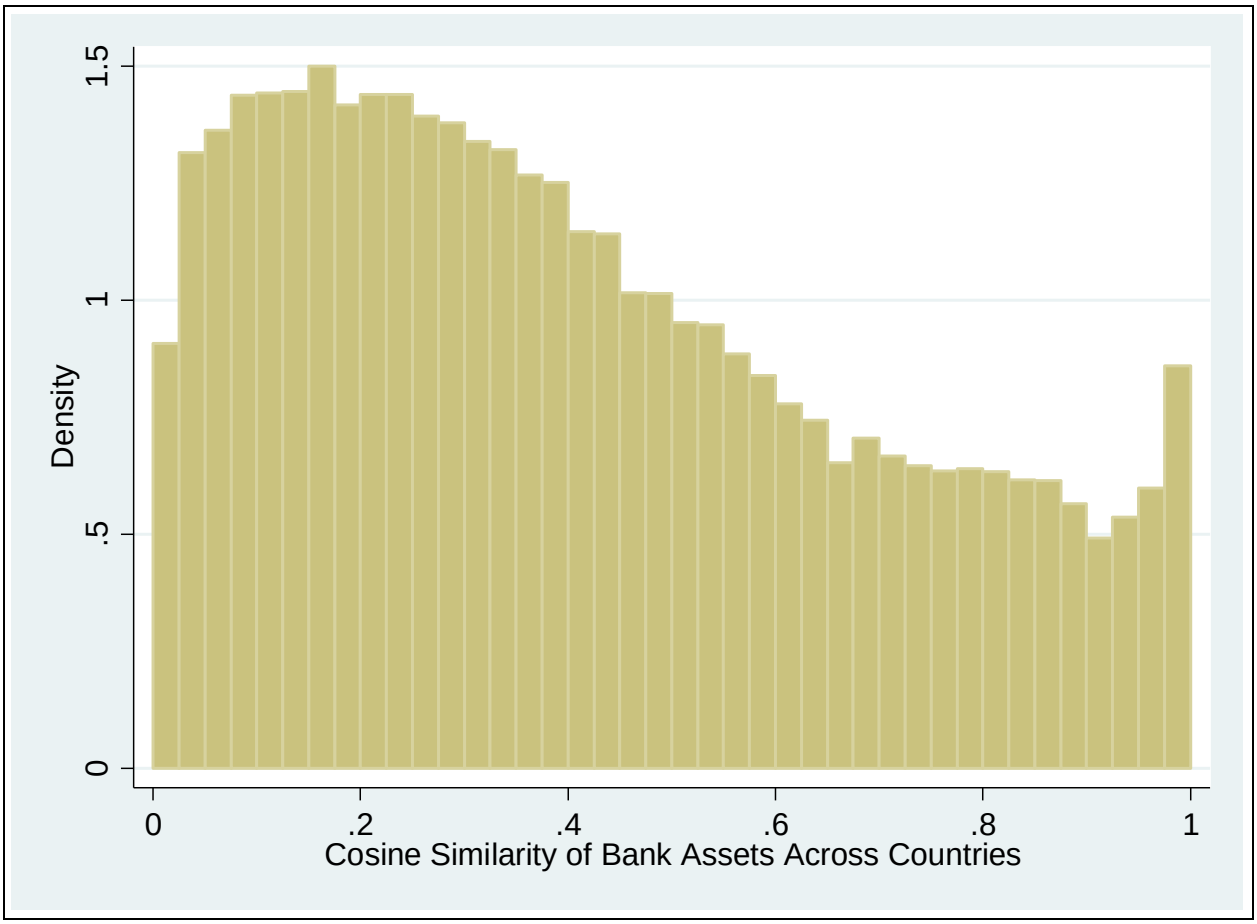

Figure 1: Histogram of Cosine Similarity for UK Banks

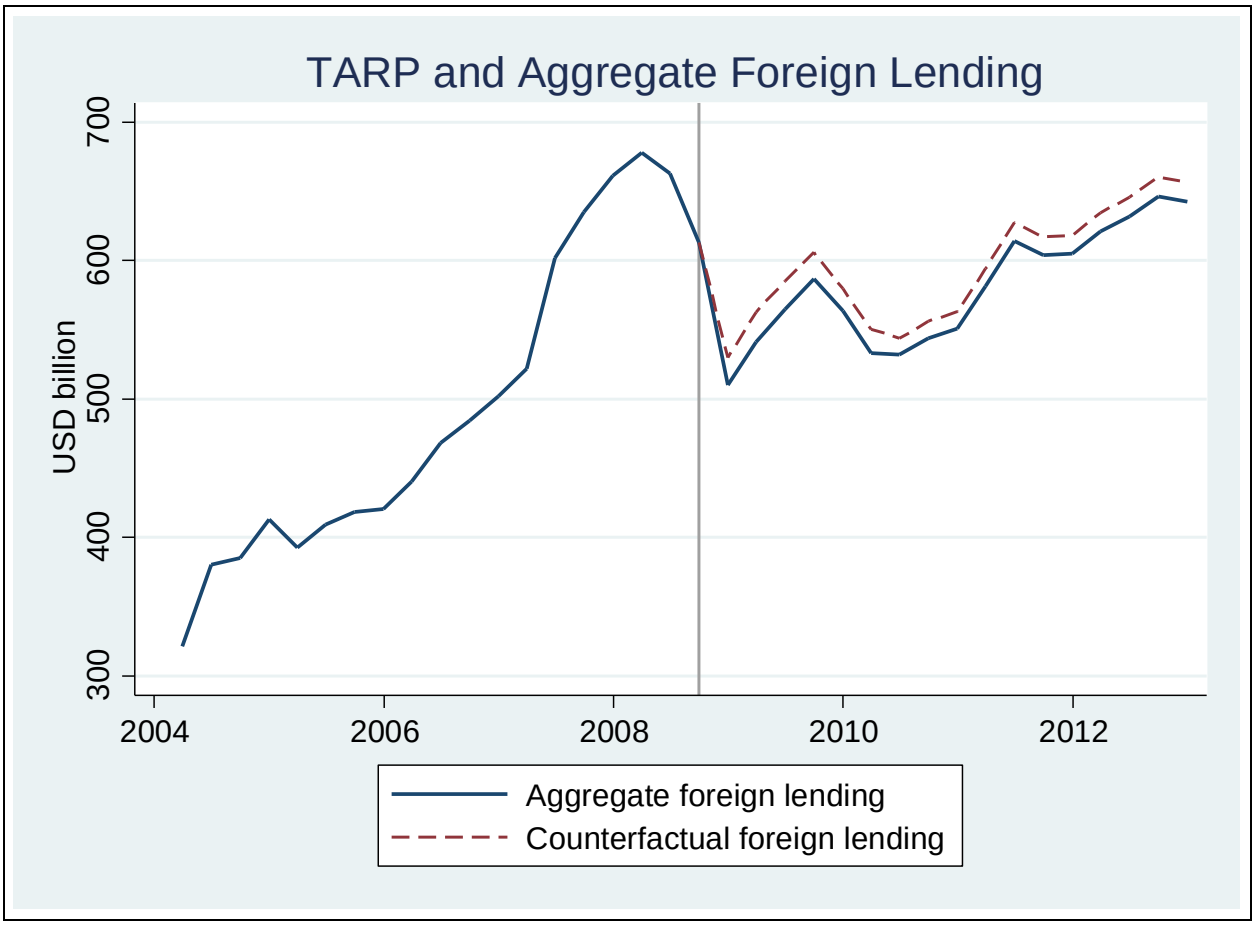

Figure 2: Aggregate foreign lending around TARP (USD, billion)

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Data Appendix

We obtain the UK data from the forms that each bank operating in the UK is legally required to report to the Bank of England:

http://www.bankofengland.co.uk/statistics/Pages/reporters/defs/default.aspx

Variable	Definition	Source	Code
Assets	Total assets of the bank	BT form	BT40
Deposit ratio	Retail deposit to total asset ratio	BT form	(BT2H+BT3H)/BT40
Liquidity Ratio	Cash + Government Bonds to total asset ratio	BT form	(BT21+BT32D)/BT40
Capital/Liabilities	Capital/Liabilities	BT form	BT19/BT20
Net Interest Income/Assets	Net Interest Income/ Total Assets	PL and BT form	PL3/BT20
Operating Profit/Assets	Operating Profit before Provisions for bad and doubtful debts and tax/ Total Assets	PL and BT form	PL13/BT20
Aggregate Foreign Lending	Total Loans & Advances, and claims under sale and repurchase agreements to non-residents	CC form	CC1
Aggregate Foreign Liabilities	Total Liabilities to non-residents	CL form	CL1
Foreign Lending by Country	Total Loans & Advances, and claims under sale and repurchase agreements to non-residents by country	CC form	CC1 lines 17 - 249
Foreign Liabilities by Country	Total Liabilities to non-residents by country	CL form	CL1 lines 20 - 252
Nationalization, Public Capital Injection, Access to Liquidity Insurance Facilities	Dummy variable that takes the value of one when bank *i* when either event occurs and zero otherwise.	Rose and Wieladek (2014)	

We obtain the data for the TARP sample from Form FR Y9-C available for US bank holding companies from the Federal Reserve Bank of Chicago. All of the variables and the corresponding summary statistics are described in detail below.

Variable	Definition	Source	Code
Assets	Total assets of the bank (or natural log of total assets)	FR Y-9C	BHCK2170
ln (Loans to foreign borrowers)	Natural logarithm of the difference between aggregate and total domestic lending	FR Y-9C	BHCK1590-BHDM1590
ln (Loans to domestic borrowers)	Natural logarithm of household, commercial and industrial loans to domestic borrowers	FR Y-9C	BHDM1766 + BHDM1975
Domestic loan growth rate (household, commercial and industrial lending)	$lnLoans\_dom_t$ - $lnLoans\_dom_{t-1}$	FR Y-9C	
Foreign loan growth rate	$lnLoans\_fgn_t$ - $lnLoans\_fgn_{t-1}$	FR Y-9C	
ln (Loans to domestic Commercial & Industrial borrowers)	Natural logarithm of total loans to domestic Commercial & Industrial borrowers	FR Y-9C	BHDM1975
ln (Loans to domestic households)	Natural logarithm of total loans to domestic households	FR Y-9C	BHDM1766
Tier 1 Capital	Tier 1 Equity as a proportion of risk-weighted assets	FR Y-9C	BHCK8274 / BHCKA223
NPL / Total Loans	Non-performing loans as a proportion of total loans	FR Y-9C	(BHCK5526 - BHCK3507 + BHCK1616 + BHCK5525 - BHCK3506) /BHCK2122
ROA	Return on assets (Net Income divided by average total assets)	FR Y-9C	BHCK4340 / BHCK3368
Cash / Deposits	Ratio of cash and cash equivalents to total deposits	FR Y-9C	(BHCK0081 + BHCK0395 + BHCK0397) / (BHCB2210 + BHOD3189 + BHFN6636 + BHCB3187 + BHOD3187 + BHCB2389 + BHOD2389 + BHCB6648 + BHOD6648)
Interest rate sensitivity	Sensitivity to changes in the interest rates measures as a ratio of short-term interest-sensitive net assets to total assets	FR Y-9C	(BHCK3197 + BHCK3296 + BHCK3298 + BHCK3408 + BHCK3409) / BHCK2170
TARP participant	Dummy variable that takes the value of one when bank *i* receives TARP funds and zero otherwise.	US Treasury	
TARP exit	Dummy variable that takes the value of one when bank *i* repays TARP funds and zero otherwise.	US Treasury	

Table A1: Descriptive statistics for the UK sample

Variable	N	SD	Mean	p25	Median	p75
Assets (GBP, thousand)	11544	78,400,000	23,000,000	742,997	3,015,274	11,200,000
Foreign Assets (GBP, thousand)	11544	27,400,000	8,030,158	192,606.8	804,363.1	4,515,177
Foreign Liabilities (GBP, thousand)	11544	32,400,000	1,223,622	388,019.5	1,223,622	4,624,659
Asset mix	11544	0.29	0.44	0.18	0.45	0.68
Liabilities Mix	11544	0.29	0.56	0.33	0.60	0.80
Deposit ratio	11514	0.90	0.24	0.065	0.17	0.35
Liquidity Ratio	11544	0.28	0.49	0.26	0.49	0.71
Capital/Liabilities	11515	0.15	0.078	0.01	0.038	0.10
Profits	5361	0.008	0.0003	0.00025	0.001	0.003
ROA	5361	0.0078	0.0006	-0.0001	0.0006	0.0016
British Access to Liquidity Insurance Facilities	11544	0.10	0.01	0	0	0
British Public Capital Injection	11544	0.078	0.006	0	0	0
British Nationalization	11544	0.069	0.005	0	0	0
Foreign Access to Liquidity Insurance Facilities	11544	0.064	0.004	0	0	0
Foreign Public Capital Injection	11544	0.18	0.035	0	0	0
Foreign Nationalization	11544	0.064	0.004	0	0	0

Table A2: Descriptive statistics for the TARP sample

Variable	N	SD	Mean	p25	Median	p75
Assets (USD, thousand)	2998	371,000,000	186,000,000	25,500,000	58,800,000	157,000,000
ln (Assets)	2998	1.3501	18.0207	17.0555	17.8896	18.8735
ln (Loans to foreign borrowers)	2998	3.0338	13.4014	11.8075	13.6613	15.3590
ln (Loans to domestic borrowers)	2998	1.3457	17.1592	16.2165	17.0817	17.9862
Domestic loan growth rate (household, commercial and industrial lending)	2998	0.1198	0.0165	-0.0231	0.0105	0.0395
Foreign loan growth rate	2911	0.4661	0.0064	-0.0565	0.0106	0.0769
ln (Loans to domestic Commercial & Industrial borrowers)	2989	1.6718	15.5404	14.7179	15.7262	16.6016
ln (Loans to domestic households)	2981	2.0831	14.8590	13.6524	14.9692	16.2429
Tier 1 Capital	2111	0.0701	0.1028	0.0799	0.0927	0.1171
NPL /Total Loans	2998	0.0232	0.0221	0.0083	0.0138	0.0267
ROA	2998	0.0077	0.0062	0.0029	0.0057	0.0092
Cash/Deposits	2981	2.3404	0.4275	0.0691	0.1052	0.2503
Interest rate sensitivity	2998	0.1939	0.7078	0.5955	0.6931	0.8334
TARP participants	2998	0.4919	0.4099	0.0000	0.0000	1.0000
TARP exit	2998	0.3019	0.1014	0.0000	0.0000	0.0000

Endnotes

[1] See the 2014 annual report of the BIS, http://www.bis.org/publ/arpdf/ar2014e.htm.

[2] The April 2015 *Global Financial Stability Report* provides evidence of financial fragmentation and links it to tighter cross-border regulation, especially due to European bank retrenchment; see chapter 2 of the *GFSR*.

[3] The BT form contains a full set of balance sheet data. The AL form gives a breakdown of lending by 18 different sectors. The CC and CL forms provide data on external lending and liabilities, respectively. For more information and description of the items reported on these forms, please see: http://www.bankofengland.co.uk/statistics/Pages/reporters/defs/default.aspx . Unfortunately our data are aggregated to the bank level; we cannot test e.g., whether there are differences between the extensive and intensive margins of bank activity.

[4] As a result of data restrictions, we can only capture foreign cross-border lending, but not lending by local foreign subsidiaries abroad.

[5] This interpretation is subject to the caveat that the Icelandic banks essentially defaulted on their UK-based deposits and were also nationalized. For these banks, our interpretation does not apply. However, our results are robust to excluding the Icelandic banks. The fact that they also hold for banks which received public capital injections suggests that our interpretation is broadly correct.

[6] Our definition of capital comes from the Bank of England's BT form and corresponds to line BT19 ("Capital and other funds"). Total assets, which are also equal to total liabilities, correspond to line BT20. Please see the Data Appendix for more detail.

[7] These variables are taken from the Bank of England's PL form. Data from this form are only available since 2004 onwards and only cover the largest institutions, meaning the 75% of banks that make up 90% of total banking system assets.

[8] We are forced to replace fixed effects with random effects.

[9] As part of government support, banks were often asked to increase domestic lending. For example, French banks in receipt of government support pledged to increase lending domestically by three to four percent, and the Dutch bank ING announced that it would lend EUR25 billion to Dutch businesses and households as part of receiving government assistance (World Bank, 2009).

[10] We exclude offshore financial centers: the Bahamas, Bermuda, British Virgin Islands, Cayman Islands, Cyprus, Gibraltar, Guernsey, Isle of Man, Jersey, and Luxembourg.

[11] Outliers for liquidity, capitalization, interest income and profitability have been removed. For instance, we draw observations where the liquidity ratio for a bank is above unity.

[12] We thank Takeo Hoshi for pointing this out to us.

[13] Details of the TARP are available from a variety of sources, including http://www.treasury.gov/initiatives/financial-stability/TARP-Programs/Pages/default.aspx and http://www.federalreserve.gov/bankinforeg/tarpinfo.htm

[14] Quarterly consolidated financial statements for bank holding companies (FR Y-9C reports) are publicly available on the Federal Reserve Bank of Chicago website.

[15] Li (2013) uses political and regulatory connections as an instrument and finds that banks participating in the TARP used two-thirds of the capital injection to improve their balance sheets.

[16] Troubled Asset Relief Program (TARP) Information, Board of Governors of the Federal Reserve System, available online at http://www.federalreserve.gov/bankinforeg/tarpinfo.htm

[17] In the same vein, Duchin and Sosyura (2014) find that following the receipt of TARP funds, banks shifted asset allocation within the same asset class to risker assets. Cornett, Li and Tehranian (2013) find that liquidity constraints prior to the receipt of TARP funds prevented otherwise healthy banks from continuing to lend. Li (2013) finds that affected banks with below median Tier 1 capital ratios raised loan supply by an annualized rate of 6.36%. Garrica, Puddu and Waelchli (2013) confirm this finding with county-level data and show that banks receiving TARP originated approximately 12% more lending to small businesses in the United States compared to banks that did not receive TARP funds. All of these findings suggest that participation in the TARP did indeed have positive loan supply effects in the US, consistent with the second objective of the program. Entry into the program was also associated with restrictions on executive pay and dividends payouts, in part to ensure that bank capital was rebuilt from retained earnings. Bayazitova and Shivdasani (2012) and Wilson and Wu (2012) find that banks with high levels of CEO pay were more likely to exit TARP early. Furthermore, Cadman, Carter and Lynch (2012) find that firms that would have been relatively more affected by the associated pay restrictions were less likely to accept TARP. Finally, Duchin and Sosyura (2012) find that strong political connections increased the probability that a bank would receive TARP funds.

[18] Financial markets might reasonably view a bank attempting to raise equity during a crisis as particularly vulnerable. This asymmetric information means that the affected bank would only be able to raise equity at a large discount. As a result, the cost of the signal that this would send might actually outweigh any benefit in terms of raising capital. Raising equity in a crisis situation is therefore only a last resort option for most banks.

[19] Since some banks repay TARP funds in installments, we use the quarter when the last TARP repayment was made to identify exit from TARP.

[20] Most bank holding companies are owned by American shareholders, so group level balance sheet data is easily available for most American banks and is typically referred to as bank holding company level data, in contrast to the situation for the UK.

[21] Since American banks are unlikely to lend abroad for real estate purposes, household, commercial and industrial lending is likely to be the domestic equivalent of foreign lending.

[22] For the US, foreign lending includes both cross-border lending and lending by foreign subsidiaries abroad.

[23] We follow Kashyap and Stein (2000) and Cetorelli and Goldberg (2012), who define large banks as those in the top 5% of the asset distribution.

[24] Similarly, Black and Hazelwood (2013) also examine large banks only in their study of the impact of TARP on risk taking.

[25] When we use our "loan mix" regressand, as in (1), we estimate a negative and statistically significant effect of TARP entry (with a tiny effect of TARP exit). This suggests that banks which entered TARP cut back on domestic, rather than foreign, lending, the opposite of what we would expect from financial protectionism. This may not be surprising, given that the distribution of this variable is heavily skewed towards unity, since few US banks do significant lending abroad. It is exactly for these reasons that we use the growth rate of foreign and domestic lending for US banks instead of the loan mix.

[26] Following Li (2013) we also conducted instrumental variables analysis following the same approach and using his instrumental variables. We are grateful to Li for providing us with his instrumental variables data for commercial banks. However, unlike Li (2013) who studies commercial banks, banks in our sample are large bank holding companies involved in cross-border lending for which these particular proxies for political connections appear to be less relevant and unfortunately these instruments do not pass IV validity requirements.

[27] For banks that issue bonds, however, controlling for their cost of bond issuance weakens our default results for foreign lenders most likely due to sample size restrictions.