The Credit Channel of Fiscal Policy Transmission[*]

Andrew Bird, Stephen A. Karolyi, Stefan Lewellen, Thomas Ruchti

November 21, 2018

Abstract

We propose and test a new channel through which fiscal policy changes affect the supply of intermediated credit and the real economy. Banks that have greater exposure to firms expected to repatriate a significant amount of foreign income as a result of a 2004-2005 U.S. tax holiday subsequently increase lending to *other*, purely domestic firms during the period of the tax holiday, leading to higher investment at these firms. Our results complement the existing literature on the credit channel of monetary policy transmission and highlight an important indirect spillover effect of fiscal policy changes on credit-constrained firms.

1 Introduction

How do fiscal policy changes affect the real economy? A large literature has studied this question.[1] However, the vast majority of these studies focus on either aggregate outcomes or on direct linkages between fiscal policies and the firms or households most affected by such policies.

In this paper, we propose and test a new channel for how fiscal policy changes can affect the real economy: namely, through the financial system.[2] Our argument has three components. First, some firms in the economy directly benefit from a fiscal policy change–in our case, a repatriation tax holiday on foreign earnings. Second, firms enable some of the benefits of the fiscal policy change to be intermediated through the banking system. Third, banks redistribute these fiscal benefits (in part) by increasing the supply of credit to *other* firms in the economy that did not directly benefit from the fiscal policy change. Hence, we argue that financial intermediaries can help to amplify fiscal policy changes by spilling over these changes to other firms in the economy through changes in the supply of credit. We refer to this channel as the credit channel of fiscal policy transmission.

The fiscal policy change that we consider is the 2004 American Jobs Creation Act ("AJCA"). The AJCA temporarily reduced the taxes owed by U.S. multinational firms on foreign income repatriated to the United States in 2004 and 2005. We focus on the

---

[1]See, e.g., Blanchard and Leigh (2013), Cohen, Coval, and Malloy (2011), Duchin and Sosyura (2014), Ilzetzki, Mendoza, and Vegh (2013), Mertens and Ravn (2014), Nakamura and Steinsson (2014), Riera-Crichton, Vegh, and Vuletin (2016), Romer and Romer (2010), and Serrato and Wingender (2016). Ramey (2011) provides a more comprehensive overview of this literature.

[2]While there are now many macroeconomic models containing both firms and banks (see, e.g., Brunnermeier and Sannikov (2014); Christiano and Ikeda (2014); Nguyen (2014); Begenau (2016); Boissay, Collard, and Smets (2016); Gertler, Kiyotaki, and Prestipino (2016); Begenau and Landvoigt (2017); Corbae and D'Erasmo (2017); Dávila and Hébert (2017); Dávila and Korinek (2017); Moreira and Savov (2017); Stavrakeva (2017); Elenev, Landvoigt, and Van Nieuwerburgh (2018)), we are not aware of any models of the credit spillover channel documented in this paper.

AJCA because this fiscal policy change has several appealing empirical properties. First, the temporary tax holiday created under the AJCA was largely unexpected, and was hence plausibly unrelated to both firms' and banks' domestic investment opportunities. Second, the temporary nature of the holiday created a large increase in the amount of repatriated foreign earnings, thereby allowing us to measure the effects of a large fiscal policy shock on affected firms. Finally, since the AJCA only affected a subset of U.S. firms, we can isolate spillover effects on firms that were not directly affected by the AJCA.

Our results suggest that financial intermediaries play an important role in the transmission of fiscal policy to the real economy. First, banks with significant pre-AJCA lending exposures to firms with foreign earnings subsequently increase lending during the AJCA's tax holiday period. The magnitude of this effect is significant: lending increases by approximately 5%, or approximately $30 billion. In addition, while lending increases to existing clients that are affected by AJCA, bank lending increases by *even more* to *other* firms that were not directly affected by the AJCA, such as firms with only domestic operations. Loan terms also improve, consistent with a supply channel: loan amounts are greater, spreads are lower, maturities are longer, loans are more likely to be unsecured, and loans are more likely to include a revolving credit facility when they are originated by banks with significant exposure to AJCA-eligible borrowers. Finally, we find evidence that firms receiving additional credit (even purely domestic firms) subsequently increase investment. Collectively, these results suggest the existence of a credit channel through which fiscal policy changes can affect the supply of credit to firms that are not directly affected by the policy change itself.

To estimate the credit channel of fiscal policy transmission, we begin by calculating a pre-AJCA measure of lenders' exposure to firms with foreign earnings (net of foreign taxes

paid) using data from Dealscan and Compustat. We refer to this time-invariant, lender-level measure as *Exposure*. Using syndicated loan data from Dealscan, we then examine whether loan origination volumes during the AJCA tax holiday period are larger at lenders with high levels of *Exposure*. Consistent with the hypothesis that tax holiday benefits were intermediated (in part) through the banking system, we find that lenders with high values of *Exposure* increase credit supply following the passage of the AJCA. The spike in credit supply coincides exactly with the beginning of the tax holiday period and ends immediately after the temporary holiday expires. Parallel trends tests confirm that there are no material differences in lending volumes between high- and low-*Exposure* lenders prior to the beginning of the AJCA tax holiday in 2004. Loan-level tests also indicate that firms borrowing from high-*Exposure* banks receive more favorable loan terms during the tax holiday. In addition, consistent with our proposed intermediation channel, we find that high-*Exposure* commercial banks account for the entirety of the increase in post-AJCA lending. These results are robust across different definitions of *Exposure* and across a variety of empirical specifications with differing fixed effects.

Our next set of tests attempts to identify which borrowers benefit from the increase in credit supply at high-*Exposure* lenders. We construct a borrower-lender-time panel that allows us to include borrower × time and borrower × lender fixed effects in our empirical specifications. These fixed effects help us to separate changes in credit supply from changes in credit demand, and to account for any preferential lender treatment awarded to certain borrowers. We then separate our sample along three dimensions. First, and most importantly, we split our sample into borrowers with foreign earnings (who might expect to benefit from the AJCA) and purely domestic borrowers with no foreign earnings. This latter group

of borrowers should not be directly affected by the AJCA, and hence, did not directly benefit from its passage.[3] However, consistent with the existence of a credit channel of fiscal policy transmission, we find that high-*Exposure* banks increased lending to *even purely domestic* borrowers. Furthermore, domestic borrowers obtained *more* credit from high-*Exposure* lenders than firms with foreign earnings during the AJCA tax holiday. Hence, we find evidence that fiscal policy changes (in this case the AJCA) can be transmitted through the financial system to affect firms that were not directly affected by the policy change itself.

Our evidence also suggests that lenders expanded credit access to marginal, credit-constrained borrowers that might not have received credit in the absence of the AJCA tax holiday. First, firms with purely domestic operations are likely to be smaller and more credit-constrained than the large, multinational firms that benefited from the passage of the AJCA. As such, our results on domestic firms are supportive of the idea that credit supply increased for marginal borrowers with weaker pre-AJCA access to credit markets. To supplement these tests, we also examine differences in post-AJCA credit access across public and private firms. Private firms tend to be smaller than public firms and arguably do not possess the same level of access to capital markets. Hence, private firms are likely more subject to financial constraints than public firms. Following the passage of AJCA, we find that high-*Exposure* banks expanded lending to private firms relative to low-*Exposure* banks. Furthermore, private firms also obtained larger amounts of credit from high-*Exposure* banks relative to public firms during the tax holiday. Collectively, these results provide strong support for the hypothesis that the AJCA allowed high-*Exposure* banks to expand credit supply

---

[3]In fact, if multinational firms used the AJCA tax holiday to expand their domestic investments – which was the stated goal of the AJCA – this would potentially have a *negative* effect on purely domestic firms' competitive positions.

in a manner that benefited marginal, credit-constrained borrowers.

Finally, we estimate the effects of increased credit supply on borrowers' subsequent investment. Since borrowers' investment opportunities (and hence, their demand for credit) are endogenous, we instrument for credit supply using a borrower's exposure to high-*Exposure* lenders. Intuitively, a borrower's exposure to high-*Exposure* lenders might affect the borrower's post-AJCA access to credit, but this measure should be unrelated to the borrower's post-AJCA investment opportunities or credit demand. We find strong evidence that the credit channel of fiscal policy transmission has real economic effects: borrowers with higher instrumented access to credit during the AJCA tax holiday subsequently increase their capital expenditures, R&D spending, and spending on acquisitions. The magnitudes of these effects are also large: for example, firms increase their investment by $0.14 for every dollar of additional lending they receive. Hence, the credit channel of fiscal policy transmission appears to be associated with economically-significant real effects. To place the magnitude of this effect in context, the Joint Committee on Taxation estimated that the cost of the AJCA in terms of lost government tax revenue was approximately $3.3 billion over a ten-year period.[4] According to our estimates, the credit channel alone resulted in an extra $30 billion in lending and an extra $4.2 billion in investment.

The spillover effects we document are distinct from, but are related to, the credit channel of monetary policy transmission (Bernanke and Gertler, 1995). In the monetary policy version of the credit channel, interest rate changes can have two effects on the real economy. First, in both direct and indirect ways, a tightening of monetary policy (that is, an increase in interest rates) can weaken firms' balance sheets, thereby affecting firms' ability to finance

---

[4]http://www.jct.gov/x-69-04.pdf

investment through internal cash flows and impacting their ability to borrow from lenders (the so-called "balance sheet channel").[5] Second, a tightening of monetary policy can reduce bank credit supply, particularly at small banks (the "bank lending channel").[6]

Like the credit channel of monetary policy transmission, our proposed channel relates fiscal policy changes to both firms' balance sheets and the supply of bank loans. However, in our proposed channel, changes in firms' balance sheets are transmitted through the banking system to affect other firms' balance sheets. In particular, we find that high-*exposure* banks subsequently experience lower default rates, suggesting that wealth effects can explain a material proportion of the expansion in credit supply. In contrast, neither of the monetary policy credit channels involve shocks to firms affecting the supply of bank credit. Our proposed channel is also purely cross-sectional in nature – a policy change affecting some firms spills over through the banking sector to affect other firms. In contrast, changes in monetary policy affect all firms (and banks) in the economy, though heterogeneity exists in the strength of these effects.

Our study contributes to at least four areas of the literature. First, we contribute to the literature on the real effects of fiscal policy changes (see Ramey (2011) for an overview of this literature). In particular, our paper joins a number of other papers that have focused on the outcomes of the AJCA (see, e.g., Blouin and Krull (2009), Dharmapala, Foley, and Forbes (2011), Faulkender and Peterson (2012), and Dyreng and Hills (2017)).[7] However, all of these studies look at effects of the holiday on "exposed" firms with foreign earnings,

---

[5]See, e.g., Gertler and Gilchrist (1993, 1994) and Bernanke, Gertler, and Gilchrist (1996).

[6]See, e.g., Bernanke and Blinder (1988), Kashyap and Stein (2000), Khwaja and Mian (2008), and Williams (2018).

[7]Other studies of the effects of the AJCA include Albring, Mills, and Newberry (2011), Brennan (2014), Graham, Hanlon, and Shevlin (2010), Morrow and Ricketts (2014), and Oler, Shevlin, and Wilson (2007).

such as how the repatriated funds were used. We instead study how the effects of the AJCA are transmitted from directly affected firms through financial intermediaries to the rest of the corporate sector.

Second, our results contribute to the literature on bank lending. Unlike the existing literature, which has primarily focused on adverse shocks to bank funding to explore the effects of bank financial health on lending outcomes (see, e.g., Khwaja and Mian (2008)), we exploit a unique situation in which a fiscal policy-related gain by some bank customers results in additional lending to other, non-affected borrowers.[8]

Third, our results suggest that "credit channels" can extend beyond the realm of monetary policy transmission. While existing studies of fiscal stimulus policies such as TARP have also focused on bank lending (see, e.g., Duchin and Sosyura (2014)), both the nature of the stimulus we study and the mechanism through which the stimulus passes through the banking sector differentiate our paper from the existing literature.

Finally, our paper relates to the literature on tax policy decisions and how such policies affect business outcomes. We document a spillover of tax law changes through the financial system that affect the real economy, which we believe is new.

---

[8]Agarwal, Chomsisengphet, Mahoney, and Stroebel (2018) also examine how borrowers benefit from credit expansions. However, the focus of their paper (credit card borrowers) differs significantly from our focus, and they do not examine lending spillovers from one set of borrowers to another set of borrowers.

2 Data and Summary Statistics

2.1 Corporate loan data

We obtain data on syndicated corporate loans from Loan Pricing Corporation's Dealscan database. Our primary sample period spans Q3 2003 to Q2 2005, which represents the period from one year before the passage of the AJCA to one year after the AJCA's passage. We exclude loan facilities with unidentified lenders. Hence, for a given loan in our sample, we have data on the identities of the borrower and lender(s) as well as loan terms such as spread, maturity, and covenants, and information on the type and purpose of the loan. Our final sample includes 22,574 loan facilities (14,375 loan packages) covering 4,646 borrowers and 1,336 financial institutions. Financial institutions with at least one bank subsidiary comprise 701 of these lenders. Of the borrowers in our sample, 54.9% are public firms, and, for the subset that are public, we identify 71.2% of them as domestic-only or multinational based on their exposure to the repatriation tax holiday.

2.2 Borrower financial data

We obtain annual data on borrowers' earnings, investment, R&D spending, acquisition spending, and other variables from Compustat for the sample period 2003–2005. We map annual accounting data by fiscal year end to the Q3 2003–Q2 2004 pre-AJCA period and the Q3 2004–Q2 2005 post-AJCA period. Using this mapping, we construct a borrower-level measure of potential repatriated earnings as the difference between the cumulative foreign earnings earned by the borrower between Q3 2001 and Q2 2003 and cumulative foreign taxes paid during the same period, and we censor this measure (from below) at zero.

2.3 Summary statistics

Table 1 provides summary statistics for our main data samples. Panel A presents summary statistics for our sample of lenders. The median lender in our sample participates in four loans per year, but this distribution is highly skewed, as the average lender participates in 61 loans per year. Only a subset of these lenders are active lead arrangers in the syndicated loan market; the average lender leads 18 syndicates per year. The average lender supplies $2.2 billion in loan capital per year, but, consistent with the skewed distribution in loan participation, the median lender supplies $62 million per year. The median lender is not exposed to borrowers with potential repatriated earnings, but the mean exposure across lenders is $52 million.

Panel B presents summary statistics for the loans that underlie the lender-level statistics. These loans are representative of the Dealscan universe of syndicated loans. The median loan has a maturity of five years and a spread of 150 basis points over the base rate (i.e., Treasuries or LIBOR). The median loan facility is $125 million, or 62.5% of the median loan amount issued in a median borrower-year observation. This is consistent with borrowers issuing loan packages comprised of multiple loan facilities or issuing multiple loan packages in a given year. Panel C shows that while most of the borrowers in our sample are large, a significant amount of heterogeneity exists in terms of borrower size and loan size.

3 Bank Exposure to the AJCA

3.1 The AJCA

The American Jobs Creation Act of 2004 was introduced in the U.S. House of Representatives on June 4, 2004 and was passed by Congress on July 15, 2004. The Senate and the House of Representatives reached final reconciliation on their two versions of the bill on October 11, 2004, and the Act took effect on October 22, 2004 after being signed by President George W. Bush.

The AJCA contained a number of provisions related to the U.S. tax code. Most prominently, a component of the AJCA called the Homeland Investment Act (HIA) exempted 85% of repatriated corporate earnings from U.S. taxes for the tax year following the passage of the Act. When U.S. companies earn money abroad, this income is normally taxed by the host country at that country's prevailing rate. If a company wishes to repatriate its earnings to the U.S., it is required to pay any difference between the U.S. tax rate (35%) and the foreign tax rate. For example, if a company earned $1,000 in a country with a tax rate of 10% and subsequently repatriated this income to the U.S., it would owe $250 in U.S. taxes ($1,000 × (35% − 10%)). The HIA exempted 85% of corporate earnings from U.S. taxes. Hence, in the example above, the company would only owe $37.50 ($150 × (35% − 10%)) in U.S. taxes rather than $250. Hence, the AJCA provided firms with a strong incentive to repatriate their income during the one-time tax holiday. Indeed, repatriations jumped from approximately $62 billion in the four years preceding the tax holiday to $299 billion during the holiday (Dharmapala, Foley, and Forbes, 2011).

To ensure that firms did not simply pass through repatriated earnings to managers or

shareholders, the AJCA included a number of restrictions that required firms to use repatriated income for investment, R&D, or to hire U.S. workers. However, since money is fungible, a firm could earmark repatriated funds for *existing* investment projects (thereby complying with the Act) while freeing up other funds for different purposes, such as increasing cash reserves, paying down debt, or buying back stock. Importantly, while there is considerable debate in the literature over the effects of the AJCA on firms' behavior (see, e.g., Dharmapala, Foley, and Forbes (2011) and Faulkender and Peterson (2012)), it is likely that some (or most) of this money passed through the U.S. banking system. In addition, by allowing firms to free up "trapped cash," the AJCA reduced the cost of internally financing projects, thereby improving firms' financial flexibility. In effect, the AJCA reduced the credit risk borne by banks that had previously lent money to repatriating firms.

3.2   Bank-level AJCA Exposure

We are interested in determining whether firms' exposure to the AJCA tax holiday led to increased credit availability, particularly for firms that were not directly affected by the AJCA. To examine this hypothesis, we begin by constructing a measure, *Exposure*, that captures the strength of a lender's relationship with borrowers that are likely to benefit from passage of the AJCA. We then perform a number of tests to link our *Exposure* measure with the pre- and post-AJCA characteristics of borrowers and lenders.

The sample period for our tests is Q3 2003 to Q2 2005. For each lending institution in Dealscan, we construct annual measures of the lender's participation in syndicated loan originations (in any role) and use those measures as our outcome variables. To better align

our tests with the exact timing of the AJCA legislation, we define Q3 2003 - Q2 2004 as the pre-AJCA period and Q3 2004 - Q2 2005 as the post-AJCA period (since the law first passed Congress on July 15, 2004). Specifically, for an outcome variable Y, we aggregate lender l's originations for the periods Q3 2003 - Q2 2004 (which corresponds to the 12 months prior to the passage of AJCA) and Q3 2004 - Q2 2005 (which corresponds to the 12 months following the passage of AJCA).

We measure *Exposure* as of Q2 2004, just before the passage of the AJCA. This variable is then held constant for each lender across our sample period. We define *Exposure* as the cumulative foreign income net of foreign taxes paid across *all* of a lender's borrowers for the three years prior to Q2 2004. For example, if Citibank had outstanding loans to three firms (A, B, and C) as of Q2 2004, Citibank's *Exposure* would be calculated as the cumulative foreign income net of foreign taxes paid (censored below at zero) for firms A, B, and C in the three years prior to Q2 2004. Hence, if firm A possessed $1 billion in net foreign earnings, firm B possessed $100 million in net foreign earnings, and firm C possessed $-100 million in net foreign earnings, Citibank's *Exposure* would be calculated as $1 billion + $100 million + $0 = $1.1 billion. Our definition of exposure is similar to the approach taken by Dharmapala, Foley, and Forbes (2011), who use a similar definition to instrument for whether firms repatriated income under the AJCA. The distribution of our *Exposure* measure is reported in Figure 1. The figure shows that significant variation exists across lenders in their pre-AJCA exposure to borrowers with foreign earnings. While *Exposure* is unsurprisingly highly correlated with total origination volumes, Figure 2 shows that significant variation in *Exposure* still exists even among lenders of similar size.

For robustness, we also examine alternative definitions of *Exposure* by constructing the

variable using different definitions of foreign income. Specifically, we examine cumulative foreign earnings (ignoring foreign taxes paid), and we examine cumulative net foreign income (where taxes are netted within rather than across years, meaning that each year's net income is censored from below at zero).

4 The Credit Channel of Fiscal Policy Transmission

4.1 Loan Volumes

Our first set of tests examines whether lenders with high *Exposure* subsequently change their lending patterns during the tax holiday period. Under the fiscal policy version of the credit channel, a lender with higher exposure to repatriating firms (i.e. high *Exposure*) should be able to supply more credit to borrowers following the passage of the AJCA. In particular, the credit channel postulates that credit availability should improve not only for repatriating firms, but also for purely domestic firms that were not directly affected by the AJCA's repatriation tax holiday.

To explore these hypotheses, we construct a lender-year panel by aggregating various outcome variables across all of the loans originated by a lender (in any role) within a given year. We focus on three outcome variables: loan origination amounts, the number of new loan originations, and the number of borrowers in a lender's portfolio that subsequently default on their loans. To examine the link between *Exposure* and post-AJCA lending outcomes, we employ a differences-in-differences specification of the form:

$$ln\ Y_{lt} \quad = \quad \alpha + \beta Exposure_l + \delta Post_t + \phi Exposure \times Post_{lt} + \Gamma FE + \varepsilon_{lt}\ , \qquad (1)$$

where Y_{lt} represents an outcome (such as loan origination volume) for lender l at time t, $Exposure_l$ represents a lender's pre-AJCA exposure to borrowers with foreign income, $Post_t$ is a dummy variable taking the value of one following the passage of AJCA, FE represents a variety of fixed effects (discussed below), and ε_{lt} represents the error term. Our main variable of interest is the interaction term $Exposure \times Post$, which captures the differential change in outcomes following the passage of AJCA between lenders with larger or smaller pre-AJCA foreign income exposure. For example, a positive value of ϕ would indicate that lenders with larger pre-AJCA foreign income exposure subsequently increased lending volumes (or the number of new loans) following the passage of AJCA relative to other lenders with lower pre-AJCA exposures. Since we use annual data that does not correspond to calendar years, the variable $Post$ takes the value of zero for the "year" 2003 (Q3 2003 - Q2 2004) and one for the "year" 2004 (Q3 2004 - Q2 2005).

We employ a number of different fixed effects in our main tests. First, we include lender fixed effects to capture any lender-specific trends such as differences in origination volumes between small and large lenders. We also include lender-type fixed effects to capture time-invariant differences in lending preferences across different types of lending institutions (for example, commercial banks versus insurance companies). Finally, we include time fixed effects to capture any common trends in loan origination across the different time periods in our sample. Since our primary specification includes both lender and time fixed effects, the $Exposure_l$ and $Post_t$ variables are absorbed by our fixed effects. Hence, the actual specification that we estimate takes the form:

$$ ln \ Y_{lst} \ = \ \alpha + \phi Exposure \times Post_{lst} + \mu_l + \mu_s + \mu_t + \varepsilon_{lst} \ , \tag{2} $$

where s represents lender type and μ_l, μ_s, and μ_t represent lender, lender type, and time fixed effects, respectively.

As a robustness check, we also construct an interacted fixed effect $Lender\ Size \times Year$ that is used in some of our tests. $Lender\ Size$ is measured as the log of a lender's total origination volume in 2003 (the year before the AJCA was passed). The inclusion of this fixed effect helps to alleviate any remaining concerns that our $Exposure$ variable is simply proxying for lender size.

Our primary identifying assumption in these tests is that lenders did not set $Exposure$ prior to the AJCA based on expectations that the AJCA would be introduced and would later become law. This assumption is similar to the main identifying assumption in other studies of the AJCA, which take the bill's introduction (and subsequent passage) as an exogenous, unexpected event. In our setting, given significant stickiness in lending relationships, this assumption seems to be particularly innocuous. Consistent with our identifying assumption, parallel trends tests in Figures 3 and 4 show that there were no material differences in either loan origination volumes or average borrower riskiness for high-$Exposure$ and low-$Exposure$ lenders prior to the passage of the AJCA in 2004.

Table 2 presents fixed effect regression estimates based on equation (1). In these tests (and all remaining tests), we measure exposure as the log of cumulative foreign income net of foreign taxes paid across all of a lender's borrowers for the years 2001-2003 (i.e. we use $lnExposure$ rather than $Exposure$). Our explanatory variable is interacted with $Post$, an indicator variable taking the value of one in the year following the tax holiday.

In columns (1) and (2) of Table 2, we find that a 100% increase in exposure is associated with a 4.6 to 4.8% increase in the dollar volume of lending during the tax holiday. From Table

1, this relative increase in exposure is less than a standard deviation of exposure for both an average and a median lender within our sample. In column (1), we include lender and year fixed effects, controlling for any correlation across lenders in exposure and loan amounts, and year-over-year changes in average loan amounts, respectively. We further include lender type indicators[9] in column (2), confirming that our effect is not driven by the types of lenders making loans. In columns (3) and (4), we find that a 100% increase in exposure is associated with a 1.1 to 1.2% increase in the number of loans made, depending on the inclusion of fixed effects, though these estimates fall below traditional levels of statistical significance. Overall, our findings indicate that lenders with more exposure to firms that can take advantage of the 2004-2005 tax holiday subsequently increase their lending volumes. In particular, our results suggest that not only are lenders with more exposure to the tax holiday making more loans, but the loans they are making are larger.[10]

In Table 3, we document the robustness of our results to different measures of exposure to the AJCA tax holiday. In Panel A, we investigate loan amounts, and in Panel B, we investigate the number of loans made. In column (1), we depart from our baseline measure of *Exposure* by measuring this variable as the log of cumulative foreign income of the lenders' borrowers, without netting out taxes paid. Taxes paid is correlated with a firm's tax strategy, so one would want to control for this endogeneity in any tests of the effects of exposure on lending outcomes. We find quantitatively similar effects in column (1) of both Panels A and B to the respective coefficients in columns (2) and (4) of Table 2, indicating that our results do not appear to be driven by measurement. In calculating tax exposure net of taxes

---

[9]These variables are equal to one if the lender has at least one subsidiary of the respective lender type of the set {*Mutual fund, Institution, Insurance Company, Corporation, Trust, Bank*}.

[10]This is because the percentage increase in loan amounts is greater than the percentage increase in the number of loans.

paid, we take the cumulative income subtracting cumulative taxes paid over the three years 2001-2003. In column (2), we measure exposure by instead first netting by year, and then summing net foreign income over years, for each borrower of a given lender. We again find quantitatively similar coefficients.

Finally, there may be trends in lending across lender size, so in column (3), we introduce lender size-by-year fixed effects into our baseline specification to allow loan growth to differ by the size of the bank from before to after the reform. In this specification, we find that a 100% increase in exposure for a lender leads to a 20.9% increase in the dollar volume of loans and a 3.3% increase in the number of loans, both of which are larger effects than we find without the inclusion of size-by-year fixed effects. Because these fixed effects control for bank size-related trends in lending, it appears that large banks face more exposure to the tax holiday, and that without this exposure, their change in lending from before to after would be relatively negative.

We are also interested in understanding which institutions are engaging in increased lending as a result of exposure to the tax holiday through their borrowers. In Table 4, we include in our sample all non-bank lenders as well. Here, we introduce a triple interaction for exposure and the post-tax holiday period. This third interaction term is *Bank*, which is an indicator that equals one if the lender has at least one subsidiary that is a bank, and zero otherwise. We find that lenders exposed to this shock with no banking subsidiaries actually reduce lending, both in dollar loan volume and in the number of loans, whereas lenders with a bank subsidiary increase their lending, as indicated by adding up the two effects for each column. In particular, in columns (1) and (2), we show that the effect of a 100% increase in exposure for lenders without a bank subsidiary corresponds with an 8.9% decrease in dollar

volume of lending during the tax holiday, but relative to that, lenders with a bank subsidiary increase this amount by over 13%, whether controlling for lender type or not. In columns (3) and (4), we find a similar result for the number of loans made. Namely, a 100% increase in exposure for lenders without a bank subsidiary have a 4.5% decrease in dollar volume of lending, but incremental to that, lenders with a bank subsidiary see a 5.6% increase. These findings are suggestive that banks respond by making more and larger loans, whereas non-bank lending institutions invest their money elsewhere in response to their exposure to the holiday.

4.2   Credit Supply vs. Credit Demand

While the tests above attempt to measure the effects of *Exposure* on post-AJCA lending outcomes, one might still be concerned that differences in lending outcomes could be caused by changes in credit demand rather than changes in credit supply. To address this concern, we also construct a borrower-lender-year panel in which we recompute loan origination metrics for every borrower-lender pair that is active in a given year. This allows us to saturate our previous regression specification with both borrower $\times$ year and borrower $\times$ lender fixed effects. In particular, we estimate the regression:

$$ ln\ Y_{blt} \quad = \quad \alpha + \phi Exposure \times Post_{blt} + \mu_b \times \mu_t + \mu_b \times \mu_l + \varepsilon_{blt} \ , \qquad (3) $$

where b represents borrowers, $\mu_b \times \mu_t$ represents a borrower $\times$ year fixed effect, and $\mu_b \times \mu_l$ represents a borrower $\times$ lender fixed effect.

The inclusion of borrower $\times$ year fixed effects allows us to account for a given borrower's

18

time-varying demand for credit, ensuring that any effects we observe are caused by shifts in loan supply rather than loan demand (Khwaja and Mian, 2008). Identification in this setting comes from comparing changes in loan amounts (or other outcome variables) across low- and high-*Exposure* banks making loans to *the same* borrower at the same point in time. Similarly, the inclusion of borrower × lender fixed effects accounts for any preferential (or deferential) treatment that a lender might give to certain borrowers.

Table 5 presents the results of these tests. In column (1), we include no fixed effects, and find that a 100% increase in exposure is associated with a 0.84% increase in the loan(s) that a particular lender gives to a borrower. This indicates that loan amounts are driven, at least in part, by the supply-side exposure of lenders to the tax holiday. In columns (2) and (3), we add lender, borrower, and year fixed effects, and then lender-by-borrower fixed effects. In both specifications we get similar results, meaning that the effects are not due to specific lender-borrower relationships. Finally, in column (4) we employ borrower-by-year fixed effects, as in Khwaja and Mian (2008), which controls for any time-varying loan demand at the borrower level. In particular, our results show that within-borrower trends in borrowing are not driving our findings, further indicating that differences in exposure across lenders affects the amount loaned. A 100% increase in exposure is associated with a 1.02% increase in loan amounts.

We also exploit heterogeneity in post-AJCA lending outcomes across different types of borrowers. First, we code a dummy variable called *Domestic* that takes the value of one if a borrower did not have any cumulative foreign earnings (net of foreign taxes paid) in the three years prior to Q2 2004. We also construct a second dummy variable, *Private*, that takes the value of one if the borrower does not appear in the Compustat database.

We then examine whether our regression estimates differ in the cross-section across borrowers in the two categories described above. Our tests take two forms. First, we re-estimate equation (3) after restricting our borrower-lender-year sample to only include borrower-lender pairs where the borrowers are domestic or private, respectively. Second, we interact the *Domestic* and *Private* variables (respectively) with our main effect (*Exposure* × *Post*) in a series of triple-difference specifications. Both sets of tests allow us to determine whether certain types of borrowers are more likely to receive additional credit from high-*Exposure* lenders following the passage of the AJCA. In particular, these tests allow us to ascertain whether *purely domestic* borrowers benefited from increased credit availability following the passage of the AJCA.

Table 6 presents the results of these tests. In column (1), we find a coefficient of 1.10% looking at only the subsample of private firms, and 0.84% for the subsample of only public domestic firms in column (2). The evidence in column (2) confirms that even in the case of restrictive Khwaja-Mian fixed effects, the credit channel of fiscal policy transmission exists.

In columns (3) and (4) of Table 6, we perform similar tests using the entire sample and an employing a triple-difference specification to isolate the incremental effects of *Exposure* based on whether borrowers are private, and only have domestic operations, respectively. In column (3), we find an incremental effect of being a private firm. Private firms are more likely to be those that banks lend to when they have excess funds because they are not exposed in the same ways as public firms are on average. First, the tax holiday affected most private firms differently than it did public firms (Redmiles, 2008). Further, private firms are less likely than public firms to have foreign income. In column (4), we restrict our sample to loans taken out by public firms in order to estimate the incremental effect of the

holiday for domestic firms (i.e., Compustat firms with no after-tax foreign income). We find that the incremental effects of the policy for domestic firms is roughly 50% greater than for public firms in general. Domestic firms see no direct benefits from the tax holiday, like many private firms, and so are more likely to need additional money in loans. As such, the effect of exposure of the lender is more relevant to these borrowers.

4.3   Loan Terms

We also hypothesize that high-*Exposure* lenders might supply credit to borrowers at more favorable terms following the passage of the AJCA. To test this hypothesis, we perform a differences-in-differences analysis on a series of loan terms including all-in-drawn credit spreads, loan maturities, collateral requirements, and fixed versus revolving credit agreements (the money terms of the loan). For robustness, we also examine loan amounts to ensure that the results from our other specifications continue to hold at the level of an individual loan. The specification we estimate is:

$$Y_{lnprt} \;\; = \;\; \alpha + \phi Exposure \times Post_{lt} + \mu_l + \mu_p + \mu_r + \mu_t + \varepsilon_{lnprt} \; , \qquad (4)$$

where n represents a loan, p represents the purpose of the loan as indicated by the Loan Purpose field in Dealscan, r represents the loan type as indicated by the Loan Type field in Dealscan, μ_p represents a loan purpose fixed effect, and μ_r represents a loan type fixed effect. Similar to our lender-year panel, identification in this setting comes from variation in lenders' *Exposure* prior to the passage of the AJCA.

Table 7 contains the results of these tests. In column (1) of Table 7, we find that a

100% increase in exposure to the tax holiday leads to loan spreads that are 1.98% lower. This is consistent with lenders affected by the holiday being able to offer cheaper financing. Further, after controlling for loan purpose as well as loan type, in column (2) we find that the effect of exposure on loan amounts is still positive and statistically significant at 2.82%. We also see in column (3) that the maturity of these loans increases by 1.70%, and in column (4) that the likelihood the loan is secured goes down by -0.90%. Exposed lenders are more willing to accept longer maturity loans, and are less concerned with receiving collateral for the loans they make. Finally, lenders are more willing to offer revolving lines of credit to their borrowers. In column (5), we show a 1.20% increase in the probability of a loan being a revolver. These loan term results provide further evidence that the increase in lending that we find is supply- rather than demand-driven.

4.4 Real Effects

4.4.1 Compustat Sample

Next, we examine whether changes in the availability of credit following the passage of the AJCA are associated with changes in borrowers' post-AJCA investment levels. Since investment is not observable for private firms, the sample for these tests is restricted to borrowers with data in Compustat. We evaluate three measures of investment: capital expenditures, research and development expenses, and acquisitions. We also combine these variables to obtain a measure of total investment at the borrower-year level.

We employ an instrumental variables approach to identify the effects of credit supply changes on borrowers' post-AJCA investment. Since credit demand and credit supply are

not determined at random, naïve OLS regressions would potentially suffer from selection issues, omitted variables issues, and reverse causality problems. To address these concerns, we instrument for credit supply using the total level of $Exposure$ across all of a borrower's banks, which we refer to as $TotalExposure$. Intuitively, a high level of $TotalExposure$ should be correlated with increased credit availability, but should be unrelated to the borrower's investment opportunities or other factors affecting credit demand. We then run specifications of the form:

$$ ln\ Investment_{bt} \quad = \quad \alpha + \beta\ ln\widehat{Amount}_{bt} + \mu_t + \varepsilon_{bt}\ , \tag{5} $$

where $ln\widehat{Amount}_{bt}$ represents the instrumented level of credit availability.

In Table 8, Panel A, we first regress the log of variables of interest on the log of predicted loan amounts for public firms. This allows us to interpret our findings in percent terms, similar to our previous tables. In column (1), we study the effects of loan amount on capital expenditures. We find that a 100% increase in loan amounts leads to roughly a 31% increase in capital expenditures. This relative increase in predicted loan amounts is substantially less than a standard deviation for both an average and a median borrower within our sample. In column (2), we find that this increase in loan amounts leads to an 8.5% increase in R&D. We also see a 7.3% increase in acquisition expenditures in column (3). In column (4), we estimate the effect of increased loan amounts on total investment, or the sum of these three variables, and find that it results in a 29.9% increase in total investment. Of note, the previous three percentages need not sum to the fourth, given that in Panel A we measure changes in percentage terms.

While percentage changes may be illustrative, they do not directly show how the policy affects actual dollar spending. In Panel B of Table 8, we model dollar expenditures on dollar loan amounts to find the per-dollar spending induced by additional loans. In column (1), we find that one dollar increase in loan amounts leads to 10.2 cents of additional capital expenditures. We further find an increase in R&D of 1.9 cents in column (2) and an increase in acquisition spending of 2.1 cents in column (3). Summing these three expenditures together, we get a total investment effect of 14.3 cents of increased investment for every additional dollar in loan funds. These numbers show that the actual pass through of increased lending in dollar terms is substantive.

4.4.2   Dealscan Sample

We next use the Loan Purpose field in Dealscan to provide additional evidence on how post-AJCA changes in credit availability affect real outcomes. In particular, we identify loans with a stated purpose of capital expenditures, acquisition (or takeover) financing, working capital investment, or debt repayment. For every borrower, we then aggregate the total volume of loans earmarked for each purpose. While these variables are not exact measures of firms' total capital expenditures, acquisition expenses, working capital investments, or debt repayment, the use of the Loan Purpose variable allows us to include all borrowers (and not just Compustat borrowers) in our sample. This allows us to separately examine the real effects of credit supply shocks on public and private borrowers using the same instrumental variables procedure described above.

In Table 9, we model the proportion of loaned funds under a loan purpose category on instrumented loan amounts. In Panel A, we investigate loan purpose for private firms. In

24

column (1), we see that an increase in loan amounts of one dollar is associated with 5.5 cents of loans purposed for capital expenditures. In columns (2), (3), and (4), we see 8.8 cents of these loans are used on average for acquisitions, 62.8 cents of these loans are taken out for the purpose of increasing working capital, and 23.0 cents for debt repayment. In Panel B, we find similar stated uses from public firms. For comparison purposes, of the average dollar borrowed unconditionally, 2.5 cents are borrowed for capital expenditures, 13 cents are borrowed for acquisitions, 50.8 cents are borrowed for working capital purposes, and 33.7 cents are borrowed for debt repayment. Hence, firms obtaining additional credit due to the AJCA appear to be more likely to borrow for working capital purposes and less likely to borrow for debt repayment relative to the average borrower in our sample. As with our results on relaxed loan terms, the fact that the incremental loans are disproportionately made for working capital purposes suggests that lenders credit supply expansion induces them fund loans with less tangible or verifiable uses.

4.5   Mechanism

A number of possible economic mechanisms could explain the credit supply effects that we have documented. First, the AJCA provided a potential windfall for companies with foreign income by giving firms the option repatriate income at a reduced tax rate. This option creates value in at least two ways: by increasing the after-tax value of repatriated income and by giving firms the opportunity to signal the future profitability of their foreign operations. In Table 10, we investigate the degree to which exposure to the tax holiday improves outcomes for the portfolio of borrowers of a lender. We measure the count of

future defaults to determine whether or not lenders have a reason to increase their lending. In columns (1) and (2), we investigate defaults one year ahead, and find that a 100% increase in exposure to the tax holiday for a lender leads to a 7.2% decline in the number of defaults. In columns (3) and (4), we find that the decrease in the number of defaults three years after the tax holiday is roughly 5.6%. We include lender, year, and then lender type fixed effects in our regressions, and the reduction in default risk stays the same. This finding is clearly illustrated in Figure 4. In particular, we see that the difference in pre-period default rates is statistically insignificant. However, during and following the tax holiday, we see that default rates in the loan portfolios of lenders with above median exposure to the tax holiday are statistically significantly smaller, and this is maintained for three years. Overall, exposure to this windfall provides direct impetus to lenders to increase lending, as their respective loan portfolios are now less risky. This finding is consistent with Oler et al. (2007), who look at the market response to the announcement of the AJCA and find that the market values of the affected firms increased by approximately the amount of the potential tax savings from the holiday. Notably, this price effect occurred in advance of any announcements by firms concerning their plans for repatriation under the holiday, and so suggests that market participants were sufficiently sophisticated to anticipate how the holiday would affect firms.

Another possible mechanism is that multinational firms use repatriated earnings to pay down debt (or otherwise reduce their demand for loans), thereby freeing up capital that banks can lend to other borrowers. However, this channel does not appear to materially explain the observed increase in credit supply. First, data from the loan purpose field in Dealscan shows that the incremental loans made by high-*Exposure* banks are less likely to be used for debt repayment relative to the sample average. In addition, we find that multinational firms'

lending volumes actually increase following the passage of AJCA, suggesting that reduced loan demand from repatriating firms cannot explain the observed increases in credit supply.

Finally, it is possible that exposed lenders are relatively better informed about the effects of the AJCA through their lending relationships with affected borrowers. For example, they may know more about the quality of foreign investments undertaken by these firms, and so better understood how the tax rate reduction would affect future operating, investing, and financing choices. However, given that the increased lending response is actually stronger for domestic borrowers, who were not directly affected by the reform, this mechanism does not appear to provide a first order explanation for our findings.

5   Conclusion

We propose and test a channel through which fiscal policy changes affect the supply of intermediated credit and the real economy. Banks that have greater exposure to firms expected to repatriate a significant amount of foreign income as a result of a 2004-2005 U.S. tax holiday subsequently increase lending to *other* domestic, private firms during the period of the tax holiday, leading to higher investment at these firms. Our results complement the existing literature on the credit channel of monetary policy transmission and highlight an important indirect spillover effect of fiscal policy changes on firms that might otherwise have difficulty accessing credit markets.

References

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel (2018), "Do Banks Pass through Credit Expansions to Consumers Who Want to Borrow?" *Quarterly Journal of Economics*, 133, 129–190.

Albring, Susan M., Lillian F. Mills, and Kaye J. Newberry (2011), "Do Debt Constraints Influence Firms Sensitivity to a Temporary Tax Holiday on Repatriations?" *Journal of the American Tax Association*, 33, 1–27.

Begenau, Juliane (2016), "Capital Requirements, Risk Choice, and Liquidity Provision in a Business Cycle Model." Working Paper, Stanford University.

Begenau, Juliane and Tim Landvoigt (2017), "Financial regulation in a quantitative model of the modern banking system." Working Paper, Stanford University.

Bernanke, Ben S. and Alan Blinder (1988), "Credit, Money, and Aggregate Demand." *American Economic Review*, 78, 435–439.

Bernanke, Ben S. and Mark Gertler (1995), "Inside the Black Box: The Credit Channel of Monetary Policy Transmission." *Journal of Econonmic Perspectives*, 9, 27–48.

Bernanke, Ben S., Mark Gertler, and Simon Gilchrist (1996), "The Financial Accelerator and the Flight to Quality." *Review of Economics and Statistics*, 78, 1–15.

Blanchard, Olivier J. and Daniel Leigh (2013), "Growth Forecast Errors and Fiscal Multipliers." *American Economic Review Papers and Proceedings*, 103, 117–120.

Blouin, Jennifer and Linda Krull (2009), "Bringing It Home: A Study of the Incentives Surrounding the Repatriation of Foreign Earnings under the American Jobs Creation Act of 2004." *Journal of Accounting Research*, 47, 1027–1059.

Boissay, Frédéric, Fabrice Collard, and Frank Smets (2016), "Booms and Banking Crises." *Journal of Political Economy*, 124, 489–538.

Brennan, Thomas J. (2014), "Where the Money Really Went: A New Understanding of the AJCA Tax Holiday." Working Paper, Northwestern University.

Brunnermeier, Markus K. and Yuliy Sannikov (2014), "A Macroeconomic Model with a Financial Sector." *American Economic Review*, 104, 379–421.

Christiano, Lawrence and Daisuke Ikeda (2014), "Leverage Restrictions in a Business Cycle Model." NBER Working Paper 18688.

Cohen, Lauren, Joshua Coval, and Christopher Malloy (2011), "Do Powerful Politicians Cause Corporate Downsizing?" *Journal of Political Economy*, 119, 1015–1060.

Corbae, Dean and Pablo D'Erasmo (2017), "Capital Requirements in a Quantitative Model of Banking Industry Dynamics." Working Paper, University of Wisconsin.

Dávila, Eduardo and Benjamin Hébert (2017), "Optimal Corporate Taxation Under Financial Frictions." Working paper, Stanford University.

Dávila, Eduardo and Anton Korinek (2017), "Pecuniary Externalities in Economies with Financial Frictions." *Review of Economic Studies*, 85, 352–395.

Dharmapala, Dhammika, C. Fritz Foley, and Kristin J. Forbes (2011), "Watch What I Do, Not What I Say: The Unintended Consequences of the Homeland Investment Act." *Journal of Finance*, 66, 753–787.

Duchin, Ran and Denis Sosyura (2014), "Safer Ratios, Riskier Portfolios: Banks' Response to Government Aid." *Journal of Financial Economics*, 113, 1–28.

Dyreng, Scott and Robert Hills (2017), "Evidence that Corporate Repatriations under the American Jobs Creation Act of 2004 Benefited the Domestic Economy." Working Paper, Duke University.

Elenev, Vadim, Tim Landvoigt, and Stijn Van Nieuwerburgh (2018), "A Macroeconomic Model with Financially Constrained Producers and Intermediaries." NBER Working Paper 24757.

Faulkender, Michael and Mitchell Peterson (2012), "Investment and Capital Constraints: Repatriations Under the American Jobs Creation Act." *Review of Financial Studies*, 25, 3351–3388.

Gertler, Mark and Simon Gilchrist (1993), "The Role of Credit Market Imperfections in the Transmission of Monetary Policy: Arguments and Evidence." *Scandinavian Journal of Economics*, 95, 43–64.

Gertler, Mark and Simon Gilchrist (1994), "Monetary Policy, Business Cycles, and the Behavior of Small Manufacturing Firms." *Quarterly Journal of Economics*, 109, 309–340.

Gertler, Mark, Nobuhiro Kiyotaki, and Andrea Prestipino (2016), "Wholesale Banking and Bank Runs in Macroeconomic Modeling of Financial Crises." In *Handbook of Macroeconomics*, volume 2, 1345–1425, Elsevier.

Graham, John R., Michelle Hanlon, and Terry Shevlin (2010), "Barriers to Mobility: The Lockout Effect of U.S. Taxation of Worldwide Corporate Profits." *National Tax Journal*, 63, 1111–1144.

Ilzetzki, Ethan, Enrique G. Mendoza, and Carlos A. Vegh (2013), "How Big (Small?) are Fiscal Multipliers?" *Journal of Monetary Economics*, 60, 239–254.

Kashyap, Anil and Jeremy Stein (2000), "What Do A Million Observations on Banks Say About the Transmission of Monetary Policy?" *American Economic Review*, 90, 407–428.

Khwaja, Asim Ijaz and Atif Mian (2008), "Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market." *American Economic Review*, 98, 1413–1442.

Mertens, Karel and Morten O. Ravn (2014), "A Reconciliation of SVAR and Narrative Estimates of Tax Multipliers." *Journal of Monetary Economics*, 68, S1–S19.

Moreira, Alan and Alexi Savov (2017), "The Macroeconomics of Shadow Banking." *Journal of Finance*, 72, 2381–2432.

Morrow, Michaele and Robert C. Ricketts (2014), "Financial Reporting versus Tax Incentives and Repatriation under the 2004 Tax Holiday." *Journal of the American Tax Association*, 36, 63–87.

Nakamura, Emi and Jon Steinsson (2014), "Fiscal Stimulus in a Monetary Union: Evidence from U.S. Regions." *American Economic Review*, 104, 753–792.

Nguyen, Thien (2014), "Bank Capital Requirements: A Quantitative Analysis." Working Paper.

Oler, Mitchell, Terry Shevlin, and Ryan Wilson (2007), "Examining Investor Expectations Concerning Tax Savings on the Repatriations of Foreign Earnings under the American Jobs Creation Act of 2004." *Journal of the American Taxation Association*, 29, 25–55.

Ramey, Valerie (2011), "Can Government Purchases Stimulate the Economy?" *Journal of Economic Literature*, 49, 673–685.

Redmiles, Melissa (2008), "The One-time Received Dividend Deduction." *Statistics of Income Bulletin*, 27, 103–117.

Riera-Crichton, Daniel, Carlos A. Vegh, and Guillermo Vuletin (2016), "Tax Multipliers: Pitfalls in Measurement and Identification." *Journal of Monetary Economics*, 79, 30–48.

Romer, Christina D. and David H. Romer (2010), "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks." *American Economic Review*, 100, 763–801.

Serrato, Juan Carlos Suarez and Philippe Wingender (2016), "Estimating Local Fiscal Multipliers." NBER Working Paper No. 22425.

Stavrakeva, Vania (2017), "Optimal Bank Regulation and Fiscal Capacity." Working Paper, London Business School.

Williams, Emily (2018), "Monetary Policy Transmission and the Funding Structure of Banks." Working Paper, Harvard Business School.

Figure 1: Bank Exposure to Potential Repatriated Income

This figure presents a histogram of bank exposure to the repatriation tax holiday, which we measure as the natural log of their borrowers' cumulative foreign income net of foreign taxes paid in between 2001 and 2003.

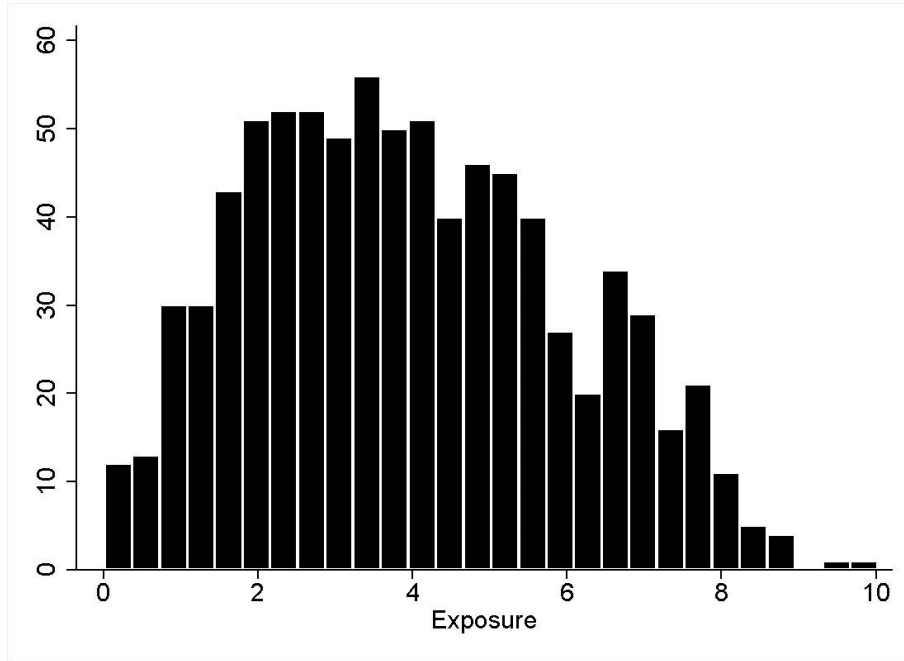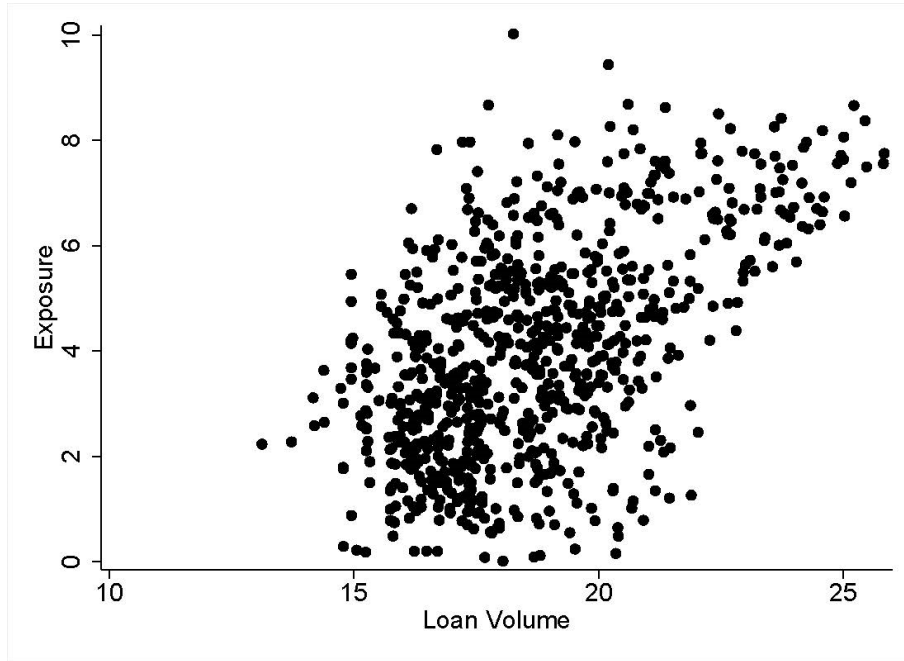Figure 2: Bank Size and Exposure to Potential Repatriated Income

This figure presents a scatter plot of bank exposure to the repatriation tax holiday, which we measure as the natural log of their borrowers' cumulative foreign income net of foreign taxes paid in between 2001 and 2003, against bank size, which we measure as the natural log of the bank's total loan supply in 2003.
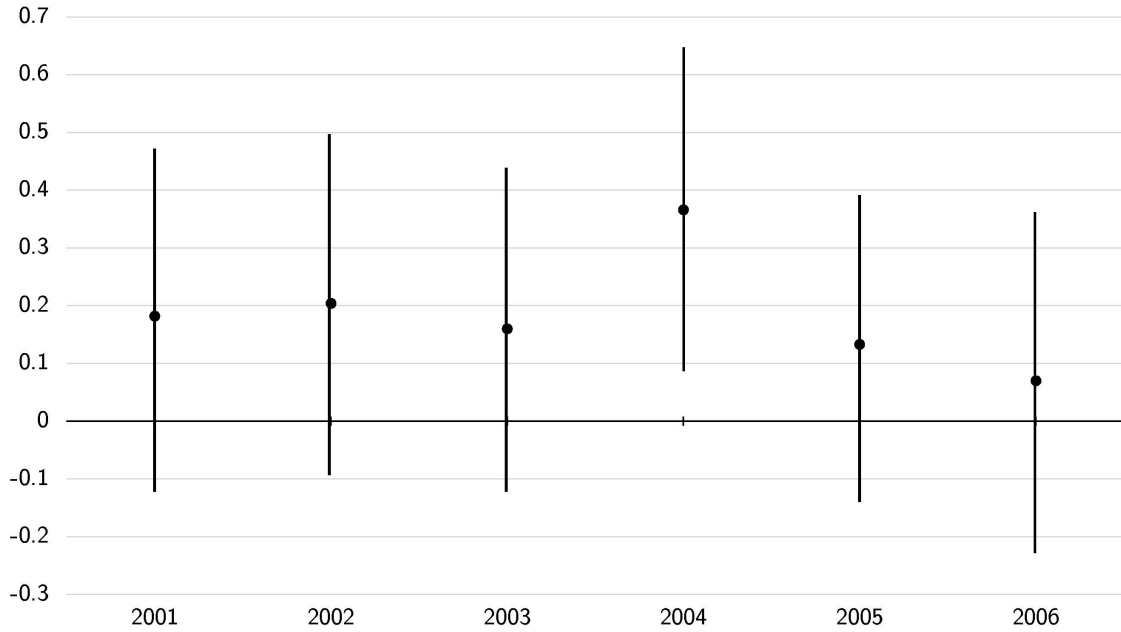
Figure 3: Parallel Trends in Bank Credit Supply

This figure plots estimates from the dynamic parallel trends test for each year around the repatriation tax holiday, which passed in 2004. The estimates are based on median splits in bank exposure to the repatriation tax holiday via their borrowers' cumulative foreign income net of foreign taxes paid. The dependent variable is the natural log of credit supply, which is measured in dollar volume of loans. The difference in coefficients between 2003 and 2004 correspond to a 20.6% increase in credit supply for banks with above-median exposure to the repatriation tax holiday relative to banks with below-median exposure to the repatriation tax holiday.
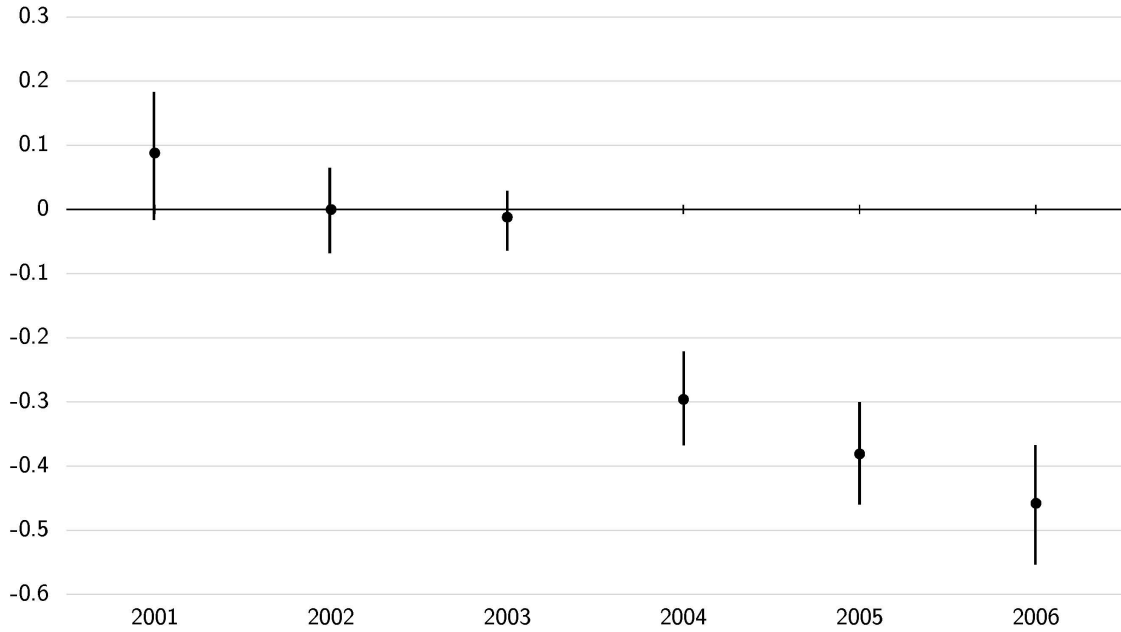
Figure 4: Parallel Trends in Bank Portfolio Defaults

This figure plots estimates from the dynamic parallel trends test for each year around the repatriation tax holiday, which passed in 2004. The estimates are based on median splits in bank exposure to the repatriation tax holiday via their borrowers' cumulative foreign income net of foreign taxes paid. The dependent variable is the natural log of the count of portfolio defaults, which are measured using S&P long-term credit ratings. The difference in coefficients between 2003 and 2004 correspond to a 28.4% decrease in portfolio defaults for banks with above-median exposure to the repatriation tax holiday relative to banks with below-median exposure to the repatriation tax holiday.

Table 1: Summary Statistics

This table presents summary statistics for the main regression variables of interest for the three samples used in the paper. First, we present summary statistics for the lenders in the sample, which comprise syndicates in any role (e.g., lead arranger or syndicate participant). We observe their exposure to the repatriation tax holiday via their borrowers' cumulative foreign income net of foreign taxes paid, their credit supply in dollar and loan volume, and various time-invariant characteristics, including indicators for whether a subsidiaries of the lender include banks or other lender types. Second, we present summary statistics for the money terms of the loans issued during the sample period. Third, we present the borrower sample, for which we observe total borrowing and multiple dimensions of investment.

Panel A. Lenders

	Mean	SD	P25	P50	P75
Exposure ($M)	52	362	0	0	12
Loan Supply ($M)	2,188	12,548	16	62	294
Number of Loans	61	248	3	7	22
Number of Loans as Lead	18	112	0	0	1
Non-Bank	47.2%				

Panel B. Loans

	Mean	SD	P25	P50	P75
Spread (bps)	179	159	55	150	250
Amount ($M)	393	931	38	125	932
Maturity (months)	60	49	36	60	72
Secured	78.73%				

Panel C. Borrowers

	Mean	SD	P25	P50	P75
Total Loan Amount ($M)	1,374	6,324	19	200	808
Capital Expenditures	364	1,554	0	7	128
R&D Expense	79	526	0	0	0
Acquisitions	96	722	0	0	1

Table 2: Fiscal Policy Effect on Bank-level Credit Supply

This table presents fixed effects regression estimates of the effect of bank exposure to the repatriation tax holiday on credit supply. The bank-year sample includes 701 lenders, all of which have at least one subsidiary that is a bank, and covers the period spanning 2003Q3 to 2005Q2. *Exposure* is defined as the cumulative foreign income net of foreign taxes paid of all of the banks' borrowers as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. *Post* is an indicator that equals one if the observation is from the 2004Q3—2005Q2 period and zero otherwise. Credit supply is measured using the natural log of total dollar volume of loans, ln*Amount*, or total number of loans, ln*Loans*. Both measures of credit supply are comprised of loans in which the lender takes any role in the syndicate. *Lender type* fixed effects refers to a series of indicator variables that equal one if the lender has at least one subsidiary of the respective lender type of the set {*Mutual fund*, *Thrift*, *Institution*, *Insurance Company*, *Corporation*, *Finance Company*, *Trust*, *Bank*}. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	ln$Amount$		ln$Loans$	
	(1)	(2)	(3)	(4)
Exposure × Post	0.0462**	0.0475**	0.0111	0.0117
	(0.0187)	(0.0189)	(0.0086)	(0.0088)
Fixed effects:				
Lender type	No	Yes	No	Yes
Lender	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Adj. R^2	0.777	0.776	0.937	0.937
Obs.	1,402	1,402	1,402	1,402

Table 3: Fiscal Policy Effect on Bank-level Credit Supply: Measurement Robustness

This table presents fixed effects regression estimates of the effect of bank exposure to the repatriation tax holiday on credit supply. Panel A measures credit supply using the total dollar volume of loans, $\ln Amount$, and Panel B measures credit supply using the total number of loans, $\ln Loans$. Both measures are comprised of loans in which the lender takes any role in the syndicate. The bank-year sample includes 701 lenders, all of which have at least one subsidiary that is a bank, and covers the period spanning 2003Q3 to 2005Q2. Whereas *Exposure* is defined as the cumulative foreign income net of foreign taxes paid of all of the banks' borrowers as of 2004Q2 in all other tables, this table provides robustness for this measurement choice. In each panel of this table, column (1) uses cumulative foreign earnings, column (2) uses cumulative net foreign income (i.e., tax netting within rather than across years), and column (3) saturates the baseline model with $\ln Amount \times Year$ fixed effects. Post is an indicator that equals one if the observation is from the 2004Q3— 2005Q2 period and zero otherwise. Lender type fixed effects refers to a series of indicator variables that equal one if the lender has at least one subsidiary of the respective lender type of the set {*Mutual fund*, *Thrift*, *Institution*, *Insurance Company*, *Corporation*, *Finance Company*, *Trust*, *Bank*}. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. $\ln Amount$

	(1)	(2)	(3)
Exposure × Post	0.0482***	0.0505**	0.2087***
	(0.0181)	(0.0202)	(0.0666)
Exposure measure:	Cumulative For. Inc. (CFI)	Cumulative Net For. Inc. (CNFI)	Baseline
Fixed effects:			
Lender type	Yes	Yes	Yes
Lender	Yes	Yes	Yes
Year	Yes	Yes	Yes
Lender Size × Year	No	No	Yes
Adj. R^2	0.776	0.776	0.798
Obs.	1,402	1,402	1,402

Panel B. $\ln Loans$

	(1)	(2)	(3)
Exposure × Post	0.0116	0.0120	0.0329***
	(0.0083)	(0.0089)	(0.0116)
Exposure measure:	Cumulative For. Inc. (CFI)	Cumulative Net For. Inc. (CNFI)	Baseline
Fixed effects:			
Lender type	Yes	Yes	Yes
Lender	Yes	Yes	Yes
Year	Yes	Yes	Yes
Lender Size × Year	No	No	Yes
Adj. R^2	0.937	0.937	0.938
Obs.	1,402	1,402	1,402

Table 4: Fiscal Policy Effect on Bank-level Credit Supply: Banks vs. Non-Banks

This table presents fixed effects regression estimates of the effect of bank exposure to the repatriation tax holiday on credit supply. The lender-year sample includes 1,336 lenders, including non-bank lenders, and covers the period spanning 2003Q3 to 2005Q2. *Exposure* is defined as the cumulative foreign income net of foreign taxes paid of all of the banks' borrowers as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. *Post* is an indicator that equals one if the observation is from the 2004Q3—2005Q2 period and zero otherwise. *Bank* is an indicator that equals one if the lender has at least one subsidiary that is a bank and zero otherwise. Credit supply is measured using the natural log of total dollar volume of loans, ln*Amount*, or total number of loans, ln*Loans*. Both measures of credit supply are comprised of loans in which the lender takes any role in the syndicate. *Lender type* fixed effects refers to a series of indicator variables that equal one if the lender has at least one subsidiary of the respective lender type of the set {*Mutual fund*, *Thrift*, *Institution*, *Insurance Company*, *Corporation*, *Finance Company*, *Trust*, *Bank*}. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	ln*Amount*		ln*Loans*	
	(1)	(2)	(3)	(4)
Exposure × Post	-0.0891**	-0.0891**	-0.0454***	-0.0446***
	(0.0417)	(0.0419)	(0.0165)	(0.0164)
... × Bank	0.1353***	0.1382***	0.0565***	0.0564***
	(0.0457)	(0.0459)	(0.0186)	(0.0187)
Fixed effects:				
Lender type	No	Yes	No	Yes
Lender	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Adj. R^2	0.752	0.751	0.913	0.913
Obs.	2,672	2,672	2,672	2,672

Table 5: Fiscal Policy Effect on Credit Supply Within Borrower

This table presents fixed effects regression estimates of the effect of lender exposure to the repatriation tax holiday on credit supply for borrowers. Because these regressions are performed using within borrower variation in exposure, this essentially holds borrowing demand as fixed. The firm-lender-year sample matches every active lender to every active borrower during the 2003Q3—2005Q2 period. *Exposure* is defined as the cumulative foreign income net of foreign taxes paid of all of the lenders' borrowers as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. *Post* is an indicator that equals one if the observation is from the 2004Q3—2005Q2 period and zero otherwise. Credit supply is measured using the natural log of total dollar volume of loans, $\ln Amount_{ijt}$, between the borrower i and bank j pair in year t, and is comprised of loans to borrower i in which bank j takes any role in the syndicate. Column (4) includes $Firm \times Year$ fixed effects as in Khwaja and Mian (2008), which controls for time-varying loan demand at the borrower level. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	ln*Amount*			
	(1)	(2)	(3)	(4)
Exposure × Post	0.0084***	0.0084***	0.0102***	0.0102***
	(0.0016)	(0.0016)	(0.0020)	(0.0020)
Exposure	0.0357***			
	(0.0070)			
Post	0.0030**			
	(0.0013)			
Fixed effects:				
Lender	No	Yes	No	No
Borrower	No	Yes	No	No
Year	No	Yes	Yes	No
Lender × *Borrower*	No	No	Yes	Yes
Borrower × *Year*	No	No	No	Yes
Adj. R^2	0.005	0.054	0.449	0.452
Obs.	4,236,900	4,236,900	3,118,080	3,118,080

Table 6: Fiscal Policy Effect on Credit Supply: Cross-sectional Heterogeneity

This table presents Khwaja and Mian (2008) fixed effects regression estimates of the effect of bank exposure to the repatriation tax holiday on credit supply. The firm-bank-year sample matches every active bank to every active borrower during the 2003Q3—2005Q2 period. Exposure is defined as the cumulative foreign income net of foreign taxes paid of all of the banks' borrowers as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. Post is an indicator that equals one if the observation is from the 2004Q3—2005Q2 period and zero otherwise. Credit supply is measured using the natural log of total dollar volume of loans, $\ln Amount_{ijt}$, between the borrower i and bank j pair in year t, and is comprised of loans to borrower i in which bank j takes any role in the syndicate. Columns (1) and (2) condition the sample on inclusion criteria based on characteristics of the borrower. Columns (3) and (4) use the full sample, but interact these borrower characteristics in a triple difference design. The characteristics are $Private$, an indicator that equals one if the borrower is a privately-held firm, and $Domestic$, an indicator that equals one if the borrower has no cumulative foreign income net of foreign taxes paid. These characteristics capture the borrower's direct exposure to the repatriation tax holiday, and the intensity of their exposure to lenders with and without indirect exposure to the repatriation tax holiday via their corporate loan portfolios. The regressions concerning $Domestic$ include observations only if the associated borrower is publicly listed. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

| | $\ln Amount$ | | | |
| | Private only | Domestic only | Full sample | |
	(1)	(2)	(3)	(4)
Exposure × Post	0.0121***	0.0091***	0.0092***	0.0065***
	(0.0023)	(0.0022)	(0.0018)	(0.0017)
... × Private			0.0029***	
			(0.0010)	
... × Domestic				0.0025*
				(0.0013)
Fixed effects:				
$Lender \times Borrower$	Yes	Yes	Yes	Yes
$Borrower \times Year$	Yes	Yes	Yes	Yes
Adj. R^2	0.393	0.525	0.452	0.534
Obs.	1,263,456	702,576	2,938,880	1,035,496

Table 7: Fiscal Policy Effect on Loan Terms

This table presents fixed effects regression estimates of the effect of bank exposure to the repatriation tax holiday on loan terms and types. The sample includes loans from the 2003Q3—2005Q2 period. *Exposure* is defined as the cumulative foreign income net of foreign taxes paid of all of the banks' borrowers as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. *Post* is an indicator that equals one if the observation is from the 2004Q3—2005Q2 period and zero otherwise. *Spread* is the all-in-drawn spread (in bps) over the base rate, Amount is the dollar amount of the loan facility, *Maturity* is the time (in months) until the loan facility matures, *Secured* is an indicator that equals one if the loan is backed by some form of collateral, and *Revolver* is an indicator variable that equals one if the loan type is a revolving credit facility. Observation counts vary due to the availability of data on outcome variables. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	ln$Spread$	ln$Amount$	ln$Maturity$	$Secured$	$Revolver$
	(1)	(2)	(3)	(4)	(5)
Exposure × Post	-0.0279***	0.0239**	0.0077	-0.0085*	0.0079**
	(0.0078)	(0.0110)	(0.0050)	(0.0051)	(0.0032)
Fixed effects:					
Lender	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Loan purpose	Yes	Yes	Yes	Yes	Yes
Loan type	Yes	Yes	Yes	Yes	No
Sample restriction					
Adj. R^2	0.497	0.326	0.511	0.279	0.118
Obs.	25,163	40,762	39,797	11,947	41,063

Table 8: Real Effects of Fiscal Policy Transmission: Instrumental Variables

This table presents instrumental variables regression estimates of the effect of bank exposure to the repatriation tax holiday on borrower investment. We instrument for the total loan amount issued by borrower i using the total exposure to the repatriation tax holiday of borrower i's banks, interacted with $Post$, an indicator variable that equals one if the borrower-year observation is during the 2004Q3—2005Q2 period. The firm-year sample includes 2,742 borrowers and covers the period spanning 2003Q3 to 2005Q2. $TotalExposure$ is defined as the cumulative $Exposure$ of borrower i's banks as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. Investment is measured as $\ln CAPEX$, the natural log of capital expenditures, $\ln R\&D$, the natural log of research and development expense, $\ln Acquisitions$, the natural log of acquisition expenditures, or $\ln TotalInvestment$, the natural log of the sum of these three components. Total credit supply to borrower i is measured using the natural log of total dollar volume of loans issued by borrower i, $\ln Amount$. Panel A presents estimates in which each variable is measured in logs, and, for quantification, Panel B presents estimates in which each variable is measured in dollars. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Logs

	$\ln CAPEX$	$\ln R\&D$	$\ln Acquisitions$	$\ln TotalInvestment$
	(1)	(2)	(3)	(4)
$\widehat{ln Amount}$	0.3119***	0.0848***	0.0731***	0.2982***
	(0.0291)	(0.0156)	(0.0154)	(0.0301)
Fixed effects:				
$Year$	Yes	Yes	Yes	Yes
$F^{First-Stage}$	123.96			
Obs.	5,484	5,484	5,484	5,484

Panel B. Dollars

	$CAPEX$	$R\&D$	$Acquisitions$	$TotalInvestment$
	(1)	(2)	(3)	(4)
$\widehat{Amount}$	0.1024***	0.0192***	0.0214**	0.1430***
	(0.0161)	(0.0058)	(0.0097)	(0.0233)
Fixed effects:				
$Year$	Yes	Yes	Yes	Yes
$F^{First-Stage}$	327.16			
Obs.	5,484	5,484	5,484	5,484

Table 9: Real Effects of Fiscal Policy Transmission: Public vs. Private

This table presents instrumental variables regression estimates of the effect of bank exposure to the repatriation tax holiday on borrower investment. We instrument for the total loan amount issued by borrower i using the total exposure to the repatriation tax holiday of borrower i's banks, interacted with $Post$, an indicator variable that equals one if the borrower-year observation is during the 2004Q3—2005Q2 period. The firm-year sample includes 8,308 borrower-year observations, including 3,809 from private firms and 4,499 from public firms, and covers the period spanning 2003Q3 to 2005Q2. $TotalExposure$ is defined as the cumulative Exposure of borrower i's banks as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. We capture borrower usage using the loan purpose field from Dealscan, aggregating loan amounts by purpose. We identify loans with capital expenditure ($CAPEX$), acquisition or takeover ($Acquisitions$), working capital or corporate purposes ($WorkCap$), and debt repayment or recapitalization ($DebtRepay$). Total credit supply to borrower i is measured using the natural log of total dollar volume of loans issued by borrower i, ln$Amount$. Panel A presents estimates for private firms, while Panel B presents estimates for public firms. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Private firms

	$CAPEX$	$Acquisitions$	$WorkCap$	$DebtRepay$
	(1)	(2)	(3)	(4)
$\widehat{Amount}$	0.0548*	0.0880**	0.6279***	0.2302***
	(0.0316)	(0.0392)	(0.0879)	(0.0828)
Fixed effects:				
$Year$	Yes	Yes	Yes	Yes
$F^{First-Stage}$	71.34			
Obs.	3,809	3,809	3,809	3,809

Panel B. Public firms

	$CAPEX$	$Acquisitions$	$WorkCap$	$DebtRepay$
	(1)	(2)	(3)	(4)
$\widehat{Amount}$	0.0277**	0.0895**	0.6563***	0.2268***
	(0.0117)	(0.0417)	(0.0471)	(0.0486)
Fixed effects:				
$Year$	Yes	Yes	Yes	Yes
$F^{First-Stage}$	162.22			
Obs.	4,499	4,499	4,499	4,499

Table 10: The Credit Channel: Future Default Rates

This table presents fixed effects regression estimates of the effect of bank exposure to the repatriation tax holiday on future default rates. The bank-year sample includes 701 lenders, all of which have at least one subsidiary that is a bank, and covers the period spanning 2003Q3 to 2005Q2. *Exposure* is defined as the cumulative foreign income net of foreign taxes paid of all of the banks' borrowers as of 2004Q2, immediately before the repatriation tax holiday was voted on in congress. *Post* is an indicator that equals one if the observation is from the 2004Q3—2005Q2 period and zero otherwise. Future default rates are measured using S&P long-term credit ratings data; $Defaults^{1yr}$ is the count of borrowers in the bank's portfolio within the next year that have a default rating, and $Defaults^{3yrs}$ is the count of borrowers in the bank's portfolio over the next 3 years with a default rating. These measures incorporate defaults based on borrower exposures via any syndicate role. Lender type fixed effects refers to a series of indicator variables that equal one if the lender has at least one subsidiary of the respective lender type of the set {*Mutual fund*, *Thrift*, *Institution*, *Insurance Company*, *Corporation*, *Finance Company*, *Trust*, *Bank*}. Robust standard errors are clustered at the bank level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	$\ln Defaults^{1yr}$		$\ln Defaults^{3yrs}$	
	(1)	(2)	(3)	(4)
Exposure $\times$ Post	-0.0739***	-0.0724***	-0.0568***	-0.0563***
	(0.0088)	(0.0089)	(0.0079)	(0.0081)
Fixed effects:				
Lender type	No	Yes	No	Yes
Lender	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Adj. R^2	0.910	0.911	0.939	0.939
Obs.	1,402	1,402	1,402	1,402