The Credit Channel of Fiscal Policy Transmission*

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Abstract

We propose and test a new channel through which fiscal policy changes affect the supply of intermediated credit and the real economy. Lenders 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.

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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, the effects of the fiscal policy change are transmitted to affected firms' lenders (by, for example, changing the credit risk of affected firms). Third, lenders 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 in this paper 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 AJCA

[1] See, e.g., Romer and Romer (2010), Cohen, Coval, and Malloy (2011), Blanchard and Leigh (2013), Ilzetzki, Mendoza, and Vegh (2013), Duchin and Sosyura (2014), Mertens and Ravn (2014), Nakamura and Steinsson (2014), Riera-Crichton, Vegh, and Vuletin (2016), and Serrato and Wingender (2016). Ramey (2011, 2019) provides a more comprehensive overview of this literature.

[2] While there are now many macroeconomic models containing both firms and banks (see, e.g., Christiano and Ikeda (2014); 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.

because this fiscal policy change has several appealing properties. First, the temporary tax holiday created under the AJCA was largely unexpected, and was hence plausibly unrelated to both firms' and lenders' 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. Lenders 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%. In addition, while lending increases to existing clients that are affected by AJCA, 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 lenders with significant exposure to AJCA-eligible borrowers. Finally, we find evidence that firms receiving additional credit–and in particular, purely domestic firms–subsequently increase investment. Collectively, these results suggest the existence of a credit channel through which fiscal policy changes can have real effects on otherwise unrelated firms.

To document these effects, 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 tax holiday 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 tax holiday in 2004. Loan-level tests indicate that firms borrowing from high-*Exposure* lenders also receive more favorable loan terms during the tax holiday. In addition, we find that high-*Exposure* commercial banks (rather than, say, corporate lenders) account for the entirety of the increase in post-AJCA lending. Our 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 credit supply increase at high-*Exposure* lenders. We construct a borrower-lender-time panel that allows us to include borrower $\times$ time and borrower $\times$ lender fixed effects in our empirical specifications.[3] These fixed effects help us to isolate 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

[3]As such, we are comparing lending outcomes across two banks with different pre-AJCA *Exposure* levels lending to the same firm at the same point in time.

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.[4] However, consistent with the existence of a credit channel of fiscal policy transmission, we find that high-*Exposure* lenders increased lending to *even purely domestic* borrowers. Furthermore, domestic borrowers obtained *more* credit from high-*Exposure* lenders than did 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.

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 pre-AJCA 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.

There are at least three mechanisms through which repatriated earnings could affect the

[4]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.

supply of credit to otherwise unrelated institutions. First, Oler, Shevlin, and Wilson (2007) find that the market value of repatriating firms increases as a result of the tax holiday. This increase in value reduces the risk of lending to such firms, thereby freeing up creditors' capital. Second, multinational firms may use repatriated earnings to pay down debt (or otherwise reduce their demand for loans), thereby freeing up capital that lenders can lend to other borrowers.[5] Finally, it is possible that exposed lenders are relatively better informed about the effects of the AJCA through their lending relationships with affected borrowers.

Our results are most consistent with the first mechanism. We find that default rates on existing loans to multinational firms go down sharply after the commencement of the tax holiday, suggesting that the riskiness of existing loans to multinational borrowers declined significantly following the announcement of the tax holiday. In contrast, we find little evidence that multinational firms reduced their demand for credit or used repatriated earnings to pay down their existing loans. We also find larger effects for purely domestic borrowers than for the multinational borrowers who would be the most likely beneficiaries of having informed lenders.

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, an increase in interest rates can weaken firms' balance sheets, thereby affecting firms' ability to finance investment through internal cash flows and impacting their ability to borrow from lenders (the so-called "balance sheet channel").[6] In addition,

[5]Multinational firms may have also deposited repatriated funds in U.S bank accounts after the tax holiday, thereby providing affected lenders with a positive funding shock. However, a 2011 staff report by the United States Senate Permanent Subcommittee on Investigations found that 46% of foreign earnings repatriated under the AJCA were already held in the United States at U.S. financial institutions prior to repatriation.

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

an increase in interest rates can reduce bank credit supply, particularly at small lenders (the "bank lending channel").[7]

In our channel, changes in firms' balance sheets are transmitted through the banking system to affect *other* firms' balance sheets. Hence, our proposed channel is purely cross-sectional in nature. In addition, neither of the monetary policy credit channels involve shocks to firms affecting the supply of bank credit. Hence, while both the fiscal and monetary policy versions of the credit channel involve credit supply shocks and firms' balance sheets, the mechanisms underpinning these two channels are completely different.

Our study contributes to four different areas of the literature. First, we contribute to the literature on the real effects of fiscal policy changes (see Ramey (2011, 2019) 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., Oler, Shevlin, and Wilson (2007), Blouin and Krull (2009), Dharmapala, Foley, and Forbes (2011), Faulkender and Peterson (2012), and Dyreng and Hills (2017)).[8] However, all of these studies look at effects of the holiday on "exposed" firms with foreign earnings, 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

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

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

results in additional lending to other, non-affected borrowers.[9]

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 lending outcomes (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.

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

[9] 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. Another relevant paper is Alfaro, García-Santana, and Moral-Benito (2019), who examine how credit supply shocks spill over across buyer-supplier relationships. However, our channel is completely different: we examine how *firm* shocks spill over to affect other firms through the banking system.

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 seven 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 Lender 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 \times (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 \times (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 lenders that had previously made loans to repatriating firms.

3.2 Lender-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* for each lender 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 the reform, censored at zero, and weighted by the fraction of each loan held by that lender. 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 for firms A, B, and C in the three pre-reform years.[10] 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.[11] Our definition of exposure is similar to the approach taken by Dharmapala, Foley, and Forbes (2011), which uses a similarly constructed variable to instrument for whether firms repatriated income under the AJCA. The distribution of our *Exposure* measure is illustrated 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.

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

[10] We measure cumulative foreign income in the three years up to Q2 of 2003 rather than the time the reform passed because the tax holiday was limited to foreign income earned as of that quarter, which was done to limit firms' ability to build up foreign earnings to repatriate in anticipation of the holiday.

[11] This example assumes that Citibank was the sole lender on each loan; if, for example, it held 50% of each of the loans, its *Exposure* would be $550M. When loan share data are not available, we assume that the loan is held equally by all participants in the syndicate.

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 primary 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 difference-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 lending institutions with different types of subsidiaries (for example, commercial banking versus insurance). 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:

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

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

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 in the three pre-reform years (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[12] 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.[13] Overall, our findings indicate that lenders with more exposure to firms that can take advantage of the tax holiday subsequently increase their lending volumes.

In the first part of Table 3, we document the robustness of our results, on both loan amounts and number of loans, to different measures of exposure to the AJCA tax holiday. We first construct alternative measures that vary the number of years of borrower foreign

[12]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*}.

[13]In certain robustness tests, we find evidence that the number of loans increased. Either way, it seems reasonable to believe that the AJCA in isolation is unlikely to have a significant effect on the number of firms wanting to take out a syndicated loan.

earnings used to calculate exposure. For either one or five years (versus the baseline of three years), we find similar results. Next, we depart from the baseline by changing how income taxes enter the calculation. First, we do not use the tax information at all, so that exposure is calculated just based on pre-tax foreign earnings. Second, we change how taxes are netted out from earnings. Specifically, we take the sum of earnings in the three year window and subtract the sum of taxes in the three year window, rather than netting year by year. Both of these alternatives yield similar estimates, suggesting that our inferences are not driven by the exact steps in the exposure calculation.

In our next set of robustness tests, we construct lender exposure to the AJCA tax holiday in several conceptually different ways. First, we start with a dummy variable equal to one if a borrower has foreign income in any of 2001, 2002, or 2003, and then aggregate it up to the lender level using the share of loans held by the lender in 2004. This is effectively a simplified version of our baseline measure that is not driven by the size or profitability of the borrowers but should nonetheless pick up exposure to the tax holiday. Second, we use similar logic to aggregate up a dummy variable equal to one if a borrower has at least one subsidiary in a tax haven, calculating share of loans as before. This should pick up borrowers most likely to benefit from the tax holiday, though again without directly using borrower size or foreign profitability. Both of these measures confirm our baseline inference that exposure to the tax holiday leads to increases in lending by affected banks. The marginal effects appear to be much larger, though the standard deviation of these measures is also lower, so that the economic effect of exposure is similar across measures.

Finally, despite the use of the alternative measures described above, one may remain concerned that our findings reflect some unobservable characteristics of lenders that might

be correlated with secular trends in lending around the tax holiday period. To address this issue, we start by including lender size by time fixed effects, where lender size is a dummy variable equal to one if the lender had an aggregate loan amount greater than the mean in 2003 (before the holiday). Interestingly, the magnitude of the estimated effect of exposure actually increases when these fixed effects are included, suggesting that if anything, bigger lenders were actually decreasing their lending activity after the holiday. We obtain similarly larger effects when we instead drop these large lenders from the sample. A related concern is that lenders active in foreign jurisdictions might have increased their lending for other reasons around this time. Specifically, one might worry that these lenders were directly affected by the tax holiday. To deal with this issue, we include a dummy variable equal to one if the lender had any loans active in 2004 where the country of syndication was not the U.S.[14] and interact it with *Post*. We obtain similar results when including these fixed effects or when dropping these lenders altogether, suggesting that lenders' own exposures to the tax holiday cannot explain our findings.

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

[14]We use this definition because we want to pick up loans that might generate foreign income for the U.S. lender, but we find similar results if we instead use non-U.S. dollar loans or loans made to borrowers with non-U.S. headquarters to define the dummy variable.

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.[15]

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, given both the borrower and lender are 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:

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

[15]One possible reason for this difference in outcomes is that banks are "special": they possess screening and monitoring technologies that other types of borrowers do not possess (Diamond (1984)).

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 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* lenders making loans to *the same* borrower at the same point in time. Similarly, the inclusion of borrower $\times$ 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 ($Exposure \times Post$) 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 employing a triple-difference specification to isolate the incremental effects of *Exposure* based on whether borrowers are private or only have domestic operations, respectively. Column (3) shows that the post-AJCA increase in credit supply is incrementally stronger for private firms. Private firms are more likely to be bank-dependent than public firms. Given an unexpected extra dollar to lend, banks may therefore prefer lending to private firms since

these loans may on average be more profitable (see, e.g., Allen and Paligorova (2015)).[16] In column (4), we restrict our sample to loans taken out by public firms in order to estimate the incremental effect of the tax holiday for purely domestic firms (i.e., Compustat firms with no after-tax foreign income) versus firms with foreign income. Consistent with our proposed channel, we find that the the post-AJCA increase in credit supply is nearly 40% larger for purely domestic firms than for firms with foreign earnings.

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 difference-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:

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

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

[16] In addition, the tax holiday affected most private firms differently than it did public firms (Redmiles (2008)), and private firms are less likely than public firms to have foreign income.

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 2.8% 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 2.4%. We also see in column (3) that the maturity of these loans increases by 0.8% (though this increase is insignificant), and in column (4) that the likelihood the loan is secured goes down by 0.9%. 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 0.8% 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 lenders, 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 at domestic firms. We then run specifications of the form:

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

where $\widehat{lnAmount}_{bt}$ 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 29.8% 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 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 a 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 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, 2.8 cents are borrowed for capital expenditures, 9.0 cents are borrowed for acquisitions, 65.6 cents are borrowed for working capital purposes, and 22.7 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 to fund loans with less tangible or verifiable uses.

4.5 Mechanism

There are at least three economic mechanisms that could be consistent with our main results. First, Oler, Shevlin, and Wilson (2007) find that the market value of repatriating firms increases as a result of the tax holiday.[17] This increase in value would reduce the risk of lending to such firms, thereby freeing up creditors' capital.

[17]Notably, this price effect occurred in advance of any announcements by firms concerning their plans for repatriation under the holiday, which suggests that market participants were sufficiently sophisticated to anticipate how the holiday would affect firms. Moreover, provided that the company does not simply burn the tax savings, the increase in value should be long-lived.

To test this hypothesis, we examine default rates for loans originated by high-*Exposure* versus low-*Exposure* banks in the years prior to and after the AJCA tax holiday. In columns (1) and (2) of Table 10, we investigate defaults one year ahead, and find that a 100% increase in exposure to the tax holiday for a lender leads to a 0.33 percentage point decline in the default rate on the lender's loan portfolio. In columns (3) and (4) of Table 10, we investigate default rates over the subsequent three years and find that a 100% increase in exposure leads to 0.42 percentage point reduction in lenders' default rates. These magnitudes are large relative to the baseline average default rates of 2.6% and 3.1%, respectively. We also plot these results in Figure 4. The figure shows that high-*Exposure* and low-*Exposure* lenders have similar borrower default rates prior to the tax holiday, but default rates decline in a statistically and economically significant fashion for high-*Exposure* lenders relative to low-*Exposure* lenders both during and after the tax holiday. Overall, these findings suggest that the AJCA makes multinational firms less risky, which in turn allows lenders to expand loan origination volumes.[18]

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 lenders can distribute 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* lenders are less likely to be used for debt repayment relative to the sample average. In addition, Table 6 shows that multinational firms' lending volumes actually increase following the passage of AJCA,

[18]In addition, if firms believe that the passage of the tax holiday will increase the probability of another tax holiday occurring, they would most likely repatriate *less* during the AJCA holiday than they otherwise would. This implies that our results might actually underestimate the true causal effect (Hennessy and Strebulaev (2019)).

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 strongest for *domestic* borrowers, the "informed lender" mechanism does not appear to provide a first-order explanation for our findings. For this mechanism to be consistent with our results, a cash flow windfall derived from the foreign earnings of multinational firms would have to somehow convey even more positive information about purely domestic firms, such that lenders would prefer to lend to these domestic firms. It is highly unlikely that (say) domestic firms' product market positions or investment opportunity sets would improve even more than those of multinational firms in the same industry when it is the multinational firms that are directly obtaining positive cash flow shocks. Hence, we view the "informed lender" channel as being unlikely to explain our results.

5 Conclusion

In this paper, we propose and test a new channel through which fiscal policy changes affect the real economy. In our proposed channel, fiscal policy changes affecting a subset of firms are transmitted through financial intermediaries to affect other firms that were not directly affected by the policy change itself. We refer to our proposed channel as the credit channel

of fiscal policy transmission. Our paper departs from the existing literature by measuring the indirect (rather than direct) effects of fiscal policy changes on otherwise unaffected firms.

Our empirical design exploits a temporary U.S. tax holiday in which multinational firms were allowed to pay reduced taxes when repatriating foreign earnings. This tax holiday, which was implemented as part of the American Jobs Creation Act of 2004 (AJCA), resulted in large cash flow windfalls for many multinational firms with significant foreign earnings. Purely domestic firms with no foreign earnings were therefore not directly affected by the AJCA. However, we find that following the passage of the AJCA, lenders with higher exposure to multinational borrowers subsequently increased credit supply, and a significant fraction of this increase in credit supply went to purely domestic firms. In addition, the recipients of these "extra" loans – including purely domestic recipients – subsequently increased investment. Hence, our paper shows that a policy change affecting a subset of firms was transmitted through the banking system to affect the investment activity of other, otherwise unrelated firms. Our results highlight an important, previously undocumented spillover channel that policymakers should consider when estimating the welfare effects of future policy changes.

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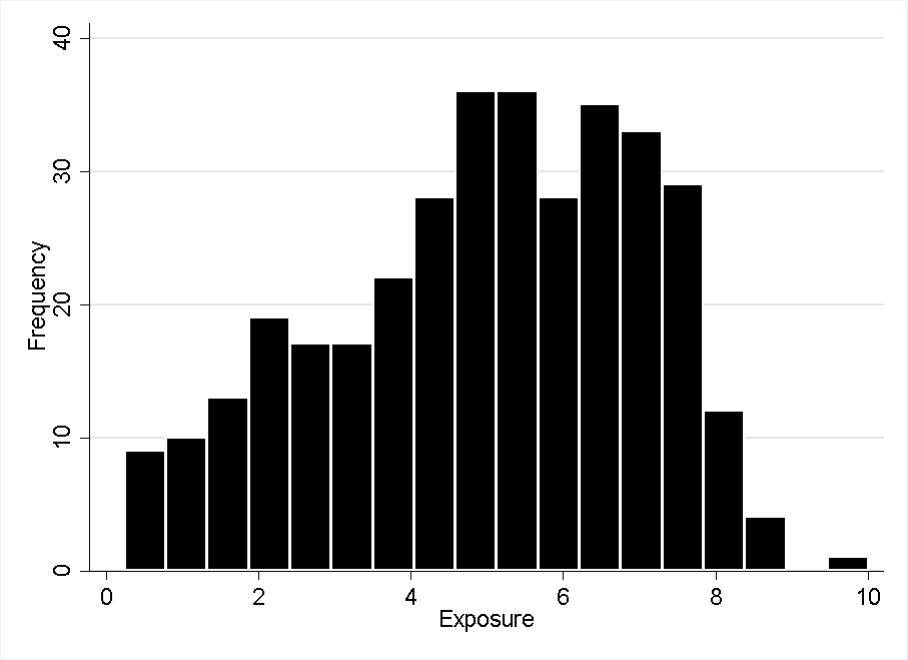

Figure 1: Lender Exposure to Potential Repatriated Income

This figure presents a histogram of lender 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 the three years prior to 2003Q2.

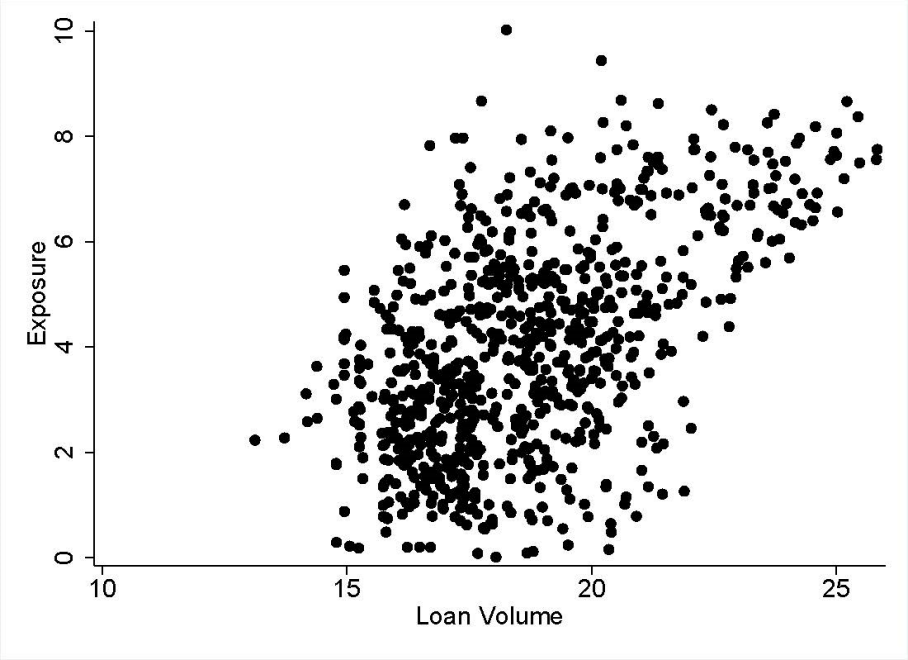

Figure 2: Lender Size and Exposure to Potential Repatriated Income

This figure presents a scatter plot of lenders' 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 the three years prior to 2003Q2, versus lender size, which we measure as the natural log of the lenders total loan supply in 2003.

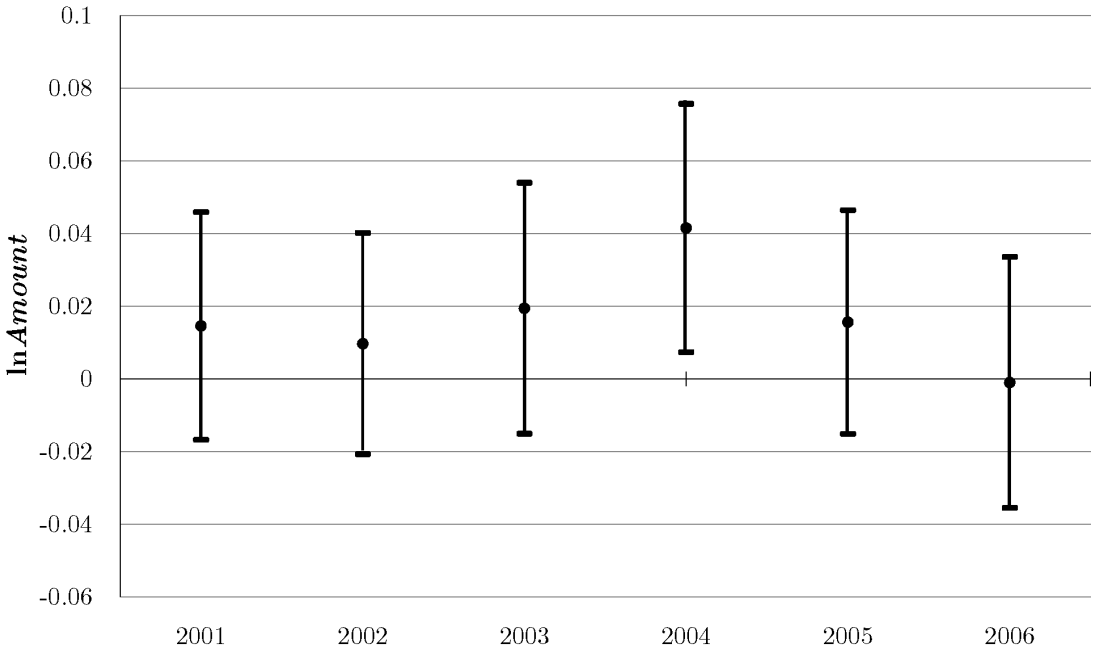

Figure 3: Parallel Trends in Lender Credit Supply

This figure plots estimates from the dynamic parallel trends test for each year around the repatriation tax holiday, which passed in 2004. Each point represents the estimated coefficient on the interaction between *Exposure* and a dummy variable for the particular year, from our baseline regression covering the 2001-2006 period where the dependent variable is the natural log of credit supply.

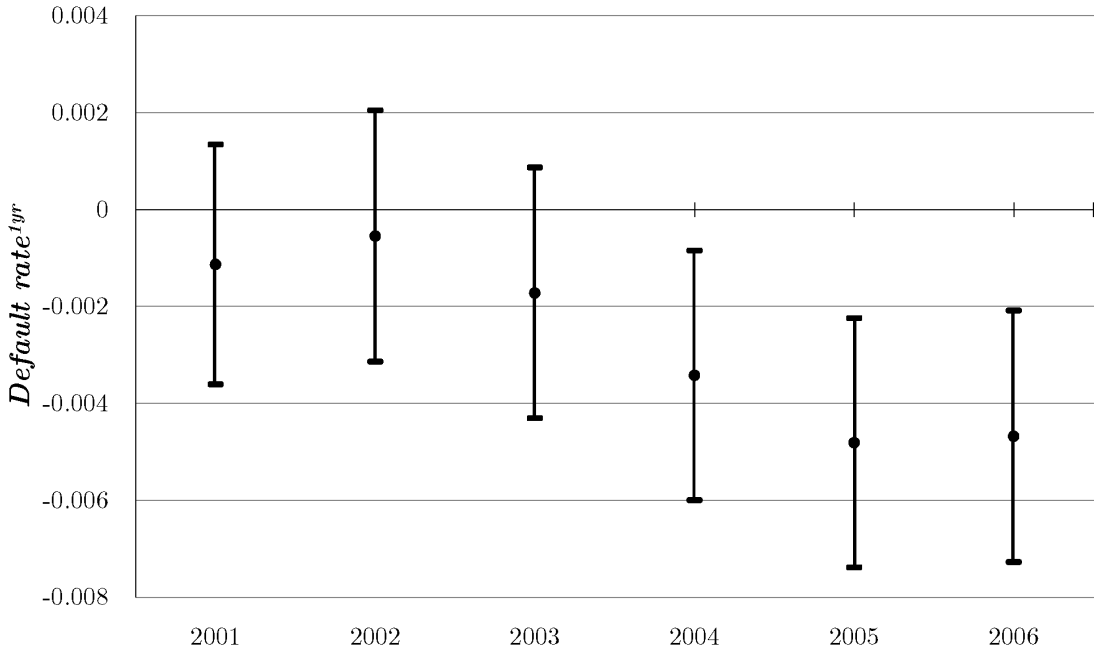

Figure 4: Parallel Trends in Lender Portfolio Default Rates

This figure plots estimates from the dynamic parallel trends test for each year around the repatriation tax holiday, which passed in 2004. Each point represents the estimated coefficient on the interaction between *Exposure* and a dummy variable for the particular year, from our baseline regression covering the 2001-2006 period where the dependent variable is the future default rate for that lender in the next year.

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 Lender-level Credit Supply

This table presents fixed effects regression estimates of the effect of lender exposure to the repatriation tax holiday on credit supply. The lender-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 net foreign income of all of the lenders borrowers as of 2004Q2, weighted by the lender's share of the loan. *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 lender 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 Lender-level Credit Supply: Measurement Robustness

This table presents fixed effects regression estimates of the effect of lender exposure to the repatriation tax holiday on credit supply, using the total dollar volume of loans in columns (1) and (2), and the total number of loans in columns (3) and (4). The lender-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. The first row reproduces our baseline estimates of the coefficient on Exposure × Post. The rest of the rows present estimates of this coefficient with either a different definition of exposure or a different set of fixed effects or sample. *1 year of foreign income* and *5 years of foreign income* adjust the baseline by using one or five years of borrower foreign income, respectively, instead of three years in the baseline. *Cumulative foreign income* modifies baseline exposure by not subtracting foreign taxes and *Cumulative net foreign income* modifies baseline exposure by aggregating income and taxes over the three years before netting them (rather than year-by-year netting in the baseline). *% of borrowers w/ foreign income* defines exposure using the fraction of the lender's borrowers in 2004 with foreign income in any of 2001, 2002, or 2003. *% of borrowers w/ tax haven subsidiary* defines exposure using the fraction of the lender's borrowers in 2004 that had a subsidiary in a tax haven. For *Large lender trend*, we include a dummy variable for large lenders in 2003 (defined by an aggregate loan amount greater than the mean) interacted with *Post*; the following row instead excludes observations from these large lenders. For *Any foreign borrowers trend*, we include a dummy variable for lenders with any foreign borrowers in 2003, as defined by a non-U.S. syndication country, interacted with *Post*; the following row excludes observations from these lenders. Fixed effects are as defined in Table 2. Robust standard errors are clustered at the lender level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	ln*Amount*		ln*Loans*	
	(1)	(2)	(3)	(4)
Baseline:				
Exposure × Post	0.0462**	0.0475**	0.0111	0.0117
	(0.0187)	(0.0189)	(0.0086)	(0.0088)
Alternative exposure measure:				
1 year of foreign income	0.0485**	0.0496**	0.0181**	0.0191**
	(0.0212)	(0.0214)	(0.0074)	(0.0075)
5 years of foreign income	0.0467***	0.0476***	0.0158***	0.0163***
	(0.0174)	(0.0176)	(0.0061)	(0.0062)
Cumulative foreign income	0.0471***	0.0482***	0.0111	0.0116
	(0.0179)	(0.0181)	(0.0082)	(0.0083)
Cumulative net foreign income	0.0494**	0.0505**	0.0117	0.0120
	(0.0200)	(0.0202)	(0.0087)	(0.0089)
% of borrowers w/ foreign income	0.5490**	0.5466*	0.2899***	0.2886***
	(0.2787)	(0.2793)	(0.1065)	(0.1066)
% of borrowers w/ tax haven subsidiary	0.4409**	0.4459**	0.0920	0.0957
	(0.1911)	(0.1917)	(0.0716)	(0.0718)
Trend controls:				
Large lender trend	0.0788***	0.0798***	0.0266**	0.0269**
	(0.0174)	(0.0177)	(0.0107)	(0.0108)
Exclude large lenders	0.1128**	0.1134**	0.0071	0.0067
	(0.0499)	(0.0498)	(0.0273)	(0.0273)
Any foreign borrowers trend	0.0450**	0.0459**	0.0081	0.0085
	(0.0184)	(0.0186)	(0.0105)	(0.0106)
Exclude if any foreign borrowers	0.0535*	0.0541*	0.0006	0.0021
	(0.0292)	(0.0293)	(0.0153)	(0.0155)
Fixed effects:				
Lender type	No	Yes	No	Yes
Lender	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes

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

This table presents fixed effects regression estimates of the effect of lender 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 net foreign income of all of the lenders borrowers as of 2004Q2, weighted by the lender's share of the loan. *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 lender 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. The firm-lender-year sample matches every active lender to every active borrower during the 2003Q3—2005Q2 period. *Exposure* is defined as the net foreign income of all of the lenders borrowers as of 2004Q2, weighted by the lender's share of the loan. *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 lender j pair in year t, and is comprised of loans to borrower i in which lender j takes any role in the syndicate. Column (4) includes $Borrower \times Year$ 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 lender 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 lender exposure to the repatriation tax holiday on credit supply. The firm-lender-year sample matches every active lender to every active borrower during the 2003Q3—2005Q2 period. *Exposure* is defined as the net foreign income of all of the lenders borrowers as of 2004Q2, weighted by the lender's share of the loan. 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 lender j pair in year t, and is comprised of loans to borrower i in which lender j takes any role in the syndicate. Columns (1) and (2) condition the sample on inclusion criteria based on characteristics of the borrower. Column (3) uses the full sample (including private borrowers) and column (4) uses the sample of public borrowers. 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 borrowers 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 lender 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 × *Borrower*	Yes	Yes	Yes	Yes
Borrower × *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 lender 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 net foreign income of all of the lenders borrowers as of 2004Q2, weighted by the lender's share of the loan. *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 lender 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 lender exposure to the repatriation tax holiday on borrower investment. We instrument for the total loan amount issued by borrower$i$ using $TotalExposure$ to the repatriation tax holiday of borrower is lenders, 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 is lenders as of 2004Q2. 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 lender 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)
$\widehat{lnAmount}$	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 lender exposure to the repatriation tax holiday on borrower investment. We instrument for the total loan amount issued by borrower i using $TotalExposure$ to the repatriation tax holiday of borrower is lenders, 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 is lenders as of 2004Q2. 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 lender 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 lender 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 net foreign income of all of the lenders borrowers as of 2004Q2, weighted by the lender's share of the loan. *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; $DefaultRate^{1yr}$ is the fraction of borrowers in the lenders portfolio within the next year that have a default rating, and $DefaultRate^{3yrs}$ is the fraction of borrowers in the lenders portfolio over the next three 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 lender level and reported in parentheses. ***,**,* represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	$DefaultRate^{1yr}$		$DefaultRate^{3yrs}$	
	(1)	(2)	(3)	(4)
Exposure × Post	-0.0033***	-0.0032**	-0.0043***	-0.0042***
	(0.0013)	(0.0013)	(0.0013)	(0.0014)
Fixed effects:				
Lender type	No	Yes	No	Yes
Lender	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Adj. R^2	0.528	0.529	0.521	0.526
Obs.	1,402	1,402	1,402	1,402