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Unintended Consequences of Unemployment Insurance Benefits: The Role of Banks*

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Abstract

Many countries provide unemployment insurance (UI) to reduce individuals' income risk and to moderate fluctuations in the economy. However, to the extent that these policies are successful, they would be expected to reduce precautionary savings and hence bank deposits–households' main saving instrument. In this paper, we study this reduced incentive to save and uncover a novel distortionary mechanism through which UI policies affect the economy. In particular, we show that, when UI benefits become more generous, bank deposits fall. Since deposits are the main stable funding source for banks, this fall in deposits squeezes bank commercial lending, which in turn reduces corporate investment.

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1 Introduction

Unemployment risk is one of the most important economic risks that a person faces. To alleviate the associated large costs, governments provide unemployment insurance (UI) to their citizens by providing a certain level of income for a limited time during the unemployment spell. At the macro level, when the economy worsens, UI policies act like a countercyclical fiscal policy and transfers funds to those with the highest need and highest marginal propensities to consume. In that respect, it is an efficient fiscal policy tool. However, to the extent that UI lowers individual and macroeconomic risks, it should be reducing household savings as it weakens the precautionary motive.

Motivated by this households' lower incentive to save, we uncover a novel distortionary mechanism through which UI policies affect the economy. In particular, we show by using disaggregated US data that more generous UI benefits lower bank deposits, households' major saving instrument. Since deposits are the major and stable source of funding for most banks, this decrease leads banks to squeeze their commercial lending to firms. The contraction in lending then lowers firm investment.

These results are important for at least two reasons. First, the results highlight a new and previously unnoticed mechanism that is relevant for the policy discussions surrounding UI policies. On the benefits side of UI policies, most discussions have concentrated on consumer welfare through consumption smoothing. On the costs side, UI policies' distortionary effects on job search and job creation have been the main focus.[1] Our findings suggest that UI policies may have large negative macroeconomic implications via their effects on bank funding. Specifically, we show that firms may suffer from lower bank credit induced by generous UI policies and lower their investment in response.

Second, the mechanism uncovered in this paper suggests an externality that may create

[1] See Section 2 for more discussion on the literature's findings.

further inefficiencies akin to the well-known "the paradox of thrift." The externality can be described as follows. In the US, where our data comes from, each state can choose its own UI benefit generosity. Therefore, a state would prefer to have more generous UI benefits if it considers the benefits outweigh the costs in that state. As states are small compared to the whole U.S. economy, they will not take into account that such a policy will lower total savings, deposits and credit. However, on aggregate, if all states increase their UI benefits, total savings in the country will be lower, leading to lower deposits and credit on average. Still, states with more generous UI benefits might look relatively better, as is found in the literature that uses within-location identification strategies, yet aggregate welfare could be lower. Our results in this paper suggest that this externality may have quantitatively large welfare costs, and should be taken into account by policymakers.

We start our deposit analysis by using annual county-level deposit data from Summary of Deposits (SOD), and test the prediction that more generous UI benefits reduce bank deposits. However, the main identification challenge is contemporaneous changes in economic conditions. For instance, if economic conditions deteriorate contemporaneously as state UI benefits increase, then we would see that county deposits decline even if UI benefits have no impact on households' savings and hence on deposits.

We address this identification challenge by exploiting the discontinuous change in the level of UI benefits at state borders. Instead of simply comparing the deposits of counties with different levels of UI benefits, we compare the deposits of two contiguous counties at state borders, one of them in one state and the other in the neighboring state (*à la* Dube et al. 2010; Hagedorn et al. 2018). Since the level of UI benefits is determined at the state level, these neighboring counties in different states have different levels of UI benefits. However, being neighbors to each other, they share similar characteristics (e.g., geography, climate, access to transportation routes) that may affect economic conditions. The key identifying assumption in this contiguous county comparison is that state-level economic shocks that may be correlated with state-level UI benefit changes do not stop at the state border and affect the two contiguous counties at the border symmetrically. We provide a validation test to support this identifying assumption by empirically showing that relevant state-level variables in one state have symmetric effects on the deposits of the same-state border county and the neighbor-state border-county.

The empirical results confirm our prediction. In response to a one standard deviation increase in state UI benefits, county total deposits decline by 2.2 percent. In dollar terms, per capita deposit holdings in a median U.S. county decrease by 82 USD following a 1,000 USD expansion of state UI benefits. The results are robust to including additional county-level variables (county income, unemployment rate) to control for county economic conditions; and to including county fixed effects to control for time-invariant county-level characteristics. Furthermore, the results are robust to making our within county-pair comparison for a subset of counties that are more similar to each other along several dimensions, such as the distance between the centers of two counties in a pair, local banking competition, industrial composition, and core-based statistical area.

In addition to the county-level deposit analysis, we do a branch-level deposit analysis to rule out the alternative explanation that bank deposit demand, instead of household deposit supply, may drive our results. For instance, if the banks operating in the two contiguous counties at the state border have different characteristics and respond differently to changes in economic conditions, then bank deposit demand may differ across the two contiguous counties. In the branch-level analysis, instead of comparing the deposits of two contiguous counties, we compare the deposits of two branches of the same bank, one branch in one of the contiguous counties and the other branch in the other county. Since banks can allocate deposits that they collect in one branch to another branch for lending to exploit lending opportunities as much as possible, there is no reason for a bank to decrease its deposit demand in one branch, but increase it in another branch. Therefore, bank demand for deposits stays constant across its branches, which allows us to measure the impact of UI benefits on deposits supply. To make this within-bank estimation, we use only the sample of banks with branches in both contiguous counties at state borders, and exclude all others since the coefficient is not identified for single-county banks. The results verify our county-level findings.

To further establish the causal link between UI generosity and bank deposits, we exploit the design of UI policies in the U.S. and the different types of deposits that banks hold in their balance sheets. First, as the UI system in the U.S. imposes percentage caps on the maximum benefit payment an unemployed person can obtain, the change in the dollar amount of UI benefits is not binding for some people. For instance, the average percentage cap in our sample is 50 percent; that is, an unemployed worker is able to obtain UI benefits of up to 50 percent of his previous wage income. If the previous wage of this worker is too low, then the percentage cap would not allow him to benefit from increases in the dollar amount of state UI benefits. For this worker, the change in UI generosity should not have any impact on his saving behavior and hence his deposit holdings. To test this prediction, we use county-level realized UI payments from the Bureau of Economic Analysis (BEA) to identify UI-sensitive counties, and show that the effect of UI benefits on deposits is stronger in counties for which the change in state UI benefits is more binding. Second, since UI benefits target mainly low-income households exposed to unemployment risk, and hence the benefits are expected to influence the saving decisions of those households, we should find the impact of UI generosity on small deposits stronger than its impact on large deposits. Our results for bank-level small deposit and large deposit items from Call Reports confirm this prediction.

Next, we examine the impact of UI generosity on bank commercial lending. As the banking literature documents, deposits are unique for banks in the sense that they are the largest and most stable funding source that banks rely on (Hanson et al., 2015; Stein, 1998). We therefore predict that the contraction in deposits due to higher UI generosity should reduce bank loan supply to firms. To test this prediction, we first calculate bank-level UI exposure as banks can reallocate deposits that they collect from one branch to another branch for lending. We take the weighted average of the UI benefits of states where a bank operates by using the bank's deposit levels in those states as weights. This measure reflects the bank's overall exposure to changes in the level of UI benefits, and is referred as *bank-UI exposure* throughout the paper.

The common identification challenge in uncovering the effect on loan supply is to keep loan demand constant. For instance, if firm loan demand decreases as bank UI exposure increases, then the decline in loan demand would drive the decrease in the equilibrium amount of loans even if banks have no incentive to decrease loan supply. Following Khwaja and Mian (2008), we implement within-firm estimation using annual firm-bank level Dealscan data on commercial loans by banks. In particular, we use firm-year fixed effects, and compare the loan amounts to the same firm in the same year by banks with different UI exposure. This within-firm estimation holds firm loan demand fixed, and hence enables us to uncover the effect of bank UI exposure on their loan supply. We find that banks that collect deposits in states with generous UI benefits originate less commercial lending compared to other banks. The effect is economically significant, with a 2.5 percent decrease in commercial lending in response to a one standard deviation increase in bank UI exposure. Furthermore, we show that the link between bank UI exposure and loan supply is especially strong for two sets of banks: i) banks that have a higher small deposit share in their balance sheets and hence experience more reduction in their deposits in response to increase in UI benefits, and ii) financially constrained banks that have more difficulty in replacing the lost deposits with other sources of funding. These findings further support our causal interpretation.

Lastly, using annual Compustat data, we analyze the impact of UI generosity on firm investment. We document that firms served by banks with higher UI exposure have lower investment. More specifically, when a firm's UI exposure through its lenders increases by one standard deviation, its investment declines by 3 percent. The impact is stronger for financially constrained firms, consistent with the idea that these firms are not able to replace bank credit with other sources of external funding. Furthermore, in all investment regressions we include firm location-year fixed effects, which means that we compare the firms that face the same level of state UI benefits but have different UI exposure through their lenders. This is important in the sense that we control for the direct effects of state UI benefits on firm decisions documented in the literature (Agrawal and Matsa, 2013; Hagedorn et al., 2018), and hence measure only the bank channel of UI on firm outcomes.

The rest of the paper is organized as follows: Section 2 discusses the related literature, Section 3 describes the data and variables constructed, Section 4 presents results on deposits, Section 5 reports results on commercial lending, Section 6 presents results on firm investment, and Section 7 concludes.

2 Related literature

Our paper is related to the literature that studies the role of income risk on household precautionary savings. For example, Engen and Gruber (2001) use a calibrated life cycle model and find that reducing UI benefit replacement rates by 50 percent would increase financial asset holdings by 14 percent. In a more general context, Carroll and Samwick (1998) estimate that approximately 45 percent of wealth accumulation is attributable to precautionary motives; Zeldes (1989), Caballero (1990), and Weil (1990) establish that precautionary savings increase in response to higher income risk. Our paper uses changes in UI benefits as a source of variation in household precautionary saving motives; and complements this literature by linking precautionary savings to bank deposits, and by analyzing its effect on bank lending and firm investment.

Our paper contributes to the recent literature on the impact of UI policies on the economy. Using household-level data, Hsu, Matsa, and Melzer (2018) and Di Maggio and Kermani (2017) emphasize the stabilizing role of UI benefits. Hsu, Matsa, and Melzer (2018) show that UI benefits prevent the mortgage defaults of unemployed people, and hence insulate the housing market from labor market shocks. Di Maggio and Kermani (2017) find that household consumption and delinquencies become less responsive to local shocks when UI benefits are more generous.[2] They argue that generous UI benefits decrease the incentive of banks to tighten credit conditions in response to negative economic shocks. Our findings, however, suggest that while UI may stabilize the economy through its effect on the household sector, this is done at the expense of banks and firms. The reason is that deposits are the largest and most reliable source of funding for banks; hence, deprived of deposits, banks are less able to support firms through commercial lending.

We contribute also to the literature that studies the distortionary effects of UI benefits. Motivated by the slow recovery of the U.S. labor market in the aftermath of the financial crisis, several papers examine the role of higher UI generosity in increasing the reservation wages of employees, and therefore decreasing the job creation incentives of firms (Chodorow-Reich, Coglianese, and Karabarbounis, 2018; Hagedorn, Manovskii, and Mitman, 2015; Hagedorn, Karahan, Manovskii, and Mitman, 2018).[3] Our paper provides an alternative mechanism that may explain the slow recovery of the U.S. labor market. Our results imply

[2]See also Gruber (1997); Browning and Crossley (2001); Bloemen and Stancanelli (2005); Chetty and Szeidl (2007) for further findings about the benefits of UI policies.

[3]See also Mulligan (2012); Barro (2010); Card and Levine (2000); Ham and Rea Jr (1987); Johnston and Mas (2018); Lalive, Landais, and Zweimüller (2015); Inderbitzin, Staubli, and Zweimüller (2016); Zweimüller (2018) for further discussion about the distortionary effects of UI policies.

that higher UI benefits during the crisis may have reduced firms' access to bank credit, which in turn lowered firm investment and, hence, employment.

Our paper is also related to the large literature on the role of deposits in the banking industry and the consequences of deposit withdrawals.[4] The empirical strand of this literature offers evidence that both bank fundamentals and panics may lead to deposit outflows. (Iyer and Puri, 2012; Iyer, Puri, and Ryan, 2016; Calomiris and Mason, 1997, 2003). In this paper, the driving force behind the decline in deposits is not the deterioration of bank fundamentals or panics, but instead the change in the pre-cautionary saving motivation of households. However, consistent with the findings of the literature on the importance of deposits for bank funding,[5] the decline in deposits still leads to a reduction in bank loan supply to non-financial firms.

Finally, our paper provides additional support to the literature that emphasizes the role of bank credit in firm-level outcomes.[6] This literature shows that a contraction in bank loan supply negatively affects firm capital expenditures (Almeida, Campello, Laranjeira, and Weisbenner, 2009; Lemmon, Roberts, and Zender, 2008), R&D investment, and productivity (Braggion and Ongena, 2017; Banerjee and Duflo, 2014; Krishnan, Nandy, and Puri, 2014). This incentivizes firms, especially financially constrained and informationally opaque ones, to build relationships with banks in order to maintain their access to external funding (Diamond, 1991; Petersen and Rajan, 1994; Drucker and Puri, 2005). However, having relationships with banks does not completely eliminate the risk of losing access to external funding as these firms are still exposed to changes in the lending capacity of their lenders. For

[4]See for example Diamond and Dybvig (1983); Calomiris and Kahn (1991); Rochet and Vives (2004); Chari and Jagannathan (1988); Jacklin and Bhattacharya (1988).

[5]See for example Hanson, Shleifer, Stein, and Vishny (2015); Kashyap, Rajan, and Stein (2002); Gorton and Pennacchi (1990); Diamond and Rajan (2000)

[6]See for example Kaplan and Zingales (1997); Rajan and Zingales (1996); Campello, Graham, and Harvey (2010); Beck, Demirgüç-Kunt, Laeven, and Maksimovic (2006); Coluzzi, Ferrando, and Martinez-Carrascal (2015); Jiménez, Ongena, Peydró, and Saurina (2012, 2017); Faulkender and Petersen (2005); Garcia-Posada (2018).

instance, using the same data set that we use, Chodorow-Reich (2013) finds that the firms that had borrowing-lending relationships with unhealthy banks prior to the crisis lowered their employment more. In our case, it is bank UI exposure that drives the decline in bank deposits, which in turn reduces the lending capacity of banks and hence firm investment due to less access to bank credit.

3 Data

3.1 Data sources and variables

The analysis in this paper relies on numerous data sources that cover the period from 1994 to 2010. In this section, we detail only the main data sources and the variables that play the central role in the analysis, and postpone describing the others until when they are used.

State-level unemployment insurance data: The U.S. Department of Labor issues "Significant Provisions of State UI Laws" that provides information on state UI benefit schedules for the period after 1938.[7] There are mainly two types of unemployment insurance (UI) payments in the U.S.: regular benefit payments and extended benefit payments. The regular UI system in the U.S. provides payments to eligible workers when they involuntarily become unemployed. These are weekly payments, the duration and the level of which are determined by state governments. According to state UI schedules, an unemployed individual is paid a predetermined percentage of his previous wage income, which is capped at the state's maximum weekly benefit level. To be more precise, there are two caps that the state UI schedule imposes on the weekly UI payment that an individual can obtain: a percentage cap and a dollar cap. The unemployed individual obtains the minimum of the two. In our analysis throughout the paper, we follow the literature and use the product of the state's

[7]We use the data obtained and provided by Hsu et al. (2018).

maximum weekly payments (dollar cap) and the duration of the payments as the main independent variable, and refer to it as *state UI benefit*. This variable shows the maximum total UI payment an unemployed individual can obtain during his unemployment spell, and reflects the generosity of a state's UI system.

The extended benefit payments, on the other hand, are provided to the unemployed only during times of high unemployment. When the maximum number of weeks under the regular payments is reached during such times, the unemployed receive additional payments for an extended period of time. In our analysis, we exclude extended benefit payments periods, and focus only on regular UI payments. We do so mainly due to two considerations. First, the benefit extensions are triggered by the economic conditions (i.e., unemployment rate) of a state. Therefore, by the very nature of the UI system, the endogeneity concern that state economic conditions and state UI benefits are highly correlated is more severe for the periods in which extended benefit payments are triggered. Second, the novel mechanism that we propose needs the changes in UI benefits to be persistent to have an impact on household saving behavior. The generosity of the state UI system in non-crisis periods serves this purpose better since the extended benefit payments are in effect only during periods of high unemployment, and hence are of a more temporary nature.

County-level deposit data: The Federal Deposit Insurance Corporation (FDIC) issues the Summary of Deposits (SOD) survey, which provides data on the amount of deposits of U.S. bank branches at an annual frequency. The data set has information on branch characteristics such as location and parent bank, allowing us to do county-level and bank branch-level[8] analyses. As our dependent variable, we use county total deposits and branch total deposits in county-level deposit analysis and branch-level deposit analysis, respectively.

Loan-level data: The data on loans are from Dealscan and contain loan-level information

[8]If a bank has more than one branch in a county, then we aggregate those branch deposits into bank-county level. For ease of reference, however, we refer to them as branch level instead of bank-county level.

on syndicated loans in the U.S. market. The information is collected by the Loan Pricing Corporation (LPC) from SEC filings and lead lenders, and available on Wharton Research Data Services (WRDS). It provides detailed information on syndicated loans, i.e., amount, purpose, type, origination date, maturity, and the types of financial covenants included in the loan contract. The dataset also provides the name and location information for the borrowers and the lenders of syndicated loans, which are used to merge the loan data with other datasets.

In our commercial lending analysis, we use the annual total outstanding loan amount between a firm and its lender as the dependent variable. Unlike credit registry data, Dealscan is flow data, and provides information on loans only at their origination; hence, we do not directly observe the outstanding loan amount between a firm and its lender in each year. We follow the literature (Lin and Paravisini, 2013; Di Maggio et al., 2017; Chakraborty et al., 2018), and construct the annual outstanding loan amount by using the information on loan origination date, termination date, and loan amount.

Bank-level balance sheet data: The bank balance sheet data is from U.S. Consolidated Reports of Condition and Income filings (Call Report), submitted by banks regulated by the Federal Reserve System, Federal Deposit Insurance Corporation, and the Comptroller of the Currency, and available from WRDS.

We aggregate bank-level data into bank holding company level by using all subsidiary banks of a bank holding company. Both the banking industry practices and the data matching process necessitate our using bank holding company level data. First, the internal capital markets within a bank holding company imply that making a bank holding company level analysis is more consistent with the findings of the banking literature. Second, the information provided on lenders of loans in Dealscan is more complete on the ultimate owner of the lender. More specifically, although some loan observations provide information on the immediate lender, which allows us to know both the subsidiary bank and its parent bank holding company, some loan observations report only the ultimate lender, in which case we do not know the immediate lender, i.e., the subsidiary bank of the bank holding company.

To support our county- and branch-level deposit analysis, we also do bank-level deposit analysis. Namely, we exploit the granularity of the Call Reports regarding the deposit size (i.e., small versus large deposits) and average interest payments on deposits. We also use several balance sheet items to construct a set of additional control variables. Bank equity ratio (bank equity normalized by bank assets) and bank size (log of bank assets) are the two widely used variables in the literature on bank lending behavior. To further control for the structure of bank balance sheets that may impact bank lending practices, and hence mitigate the omitted variable bias, the share of securities in total assets, and the share of core deposits in liabilities are also controlled for.

Firm-level data: Firm characteristics and annual accounting information comes from Compustat, available on WRDS. The main firm-level variable is firm investment rate, which is defined by the total amount of capital expenditures divided by lagged total firm assets. Several firm-level variables are used to control for the firm's investment opportunities such as firm size (log of total assets), marginal Q, leverage, and Altman's Z-score.

Merged Datasets: We have three main analyses. First, we analyze how the changes in state unemployment insurance affect households' deposit holdings. This deposit analysis is based on comparing two border counties located at state borders. Therefore, we aggregate SOD's branch-level deposits into county deposits, and supplement the data with annual county-level income, unemployment rate, and annual state UI benefit payments.[9] We do the same analysis at the branch level without aggregating the deposit data at county level, in which case we are comparing the two branches of the same bank located in different counties

[9] We obtain the county-level income and unemployment rate data from Bureau of Economic Analysis (BEA), and Bureau of Labor Statistics (BLS), respectively.

at state borders.

Second, we study the effect of changes in unemployment insurance on bank commercial lending. The analysis is based on Dealscan's firm-bank level commercial loan data. Following Khwaja and Mian (2008) methodology, we use only the Dealscan firms that have outstanding loans from multiple banks in a given year, and make a within-firm comparison by including firm-year fixed effects in the regressions. To do the lending analysis, we supplement the loan data with bank-level UI exposure, which is calculated by taking the weighted average of the UI level of the states where a bank operates using the deposits of the bank in those states as weights. We refer to this variable as bank UI exposure throughout the paper. Furthermore, we merge this data with the bank balance sheet information from Call Reports to control lender characteristics that may affect loan outcomes. We manually match lead lenders in Dealscan with commercial banks in Call Reports data based on name and location. In the Call Reports data, commercial banks report their top-holder bank holding company, enabling us to aggregate bank-level variables into bank holding company level.

Third, we examine the effect of unemployment insurance on firm investment decisions. The analysis is at firm-year level and based on annual Compustat files that provide both the firm investment information and other firm-level controls. By using the link file provided by Chava and Roberts (2008), we match Compustat firms with their Dealscan borrowers, which allows us to calculate the unemployment insurance exposure of a Compustat firm through its lenders. To calculate the exposure, first we determine the banks the firm works with by using Dealscan loan origination date and loan maturity. Second, by using the outstanding loan amount between the firm and its lenders, we take the weighted average of the UI exposures of its lenders. We refer to this new constructed firm-year level variable as firm UI exposure throughout the paper.

3.2 Summary statistics

Table 1 reports the summary statistics in three panels. Panel A presents the summary statistics at the county level for the sample of border counties that we use in our deposit analysis. The weekly UI benefit payment in an average county is 330 USD for a period of 26 weeks. The product of weekly payments and the duration of the payments is the maximum total UI benefit payment an unemployed person can obtain during his unemployment spell. This variable is our main independent variable, which we refer to as *UI benefit*, in the deposit analysis, and its average is 8,540 USD. The variable shows significant variation that mainly comes from weekly payments as the duration of payments is almost uniform across time and states. The median county in the sample has 313 million USD deposits and 624 million USD total income.

Panel B reports summary statistics at the firm-bank level for our commercial lending analysis. Since we implement a within-firm estimation, we keep only the sample of firms that have lending relationships with multiple lenders. The relevant variable for this analysis is bank-level UI exposure, which is obtained by taking the weighted average of UI benefits of states where a bank operates. It reflects the average level of UI benefits the bank faces. Since most of the banks in our sample operate in more than one state, averaging state UI benefits to obtain bank UI exposure decreases the variation in the variable. Furthermore, note that the average bank UI exposure is higher than the average state UI benefits, implying that our sample is biased toward banks that operate in states with high UI benefits. The typical bank in the sample is large, with an asset size of 475 billion USD and a Dealscan loan size of 567 million USD. The asset share of core deposits for an average bank is 45 percent, significantly lower than that for an average Call Report bank. This implies that we underestimate the effect of bank UI exposure on loan supply because the banks in our sample are less dependent on deposits and hence less affected by the decline in deposits.

Panel C presents firm-level summary statistics for the investment analysis. The typical firm in the sample is large. This is mainly because Dealscan data is biased toward reporting loan contract agreements of large banks and firms. Furthermore, the probability of matching large Dealscan borrowers with Compustat firms is higher since the information on those firms is more complete.

4 Deposit analysis

In this section, using county total deposits and state UI benefits, we show that the increase in unemployment insurance (UI) benefits reduces household deposit holdings by lowering their incentive to save. The results of a model in which we simply regress county deposits on state UI benefits are contaminated by endogeneity. State UI generosity depends on state political factors (election concerns, party preferences), state economic factors (labor market conditions, state budget surplus/deficit), and the interaction of the two. State economic conditions are also expected to affect economic activity in the county, and hence total county deposits. This implies that to the extent that we omit relevant state economic conditions in our regression, the coefficient of state UI benefits will be biased. For instance, when an economic shock hits a state, the shock can trigger a change in state UI benefits, along with a change in the deposit levels of the counties located in that state. Therefore, the estimated coefficient would erroneously attribute the effect of the economic shock on county deposits to the state UI benefits. To establish the causality from state UI to county deposits, therefore, we must control for state economic conditions.

4.1 Identification strategy

We address the identification challenge by exploiting the discontinuous change in UI benefits at state borders. Instead of simply comparing the deposits of any counties with different levels of UI benefits, we compare the deposits of two contiguous counties that neighbor each other at state borders, one of them in one state and the other in the neighboring state (Dube et al., 2010; Hagedorn et al., 2018; Heider and Ljungqvist, 2015). For instance, Figure 1a shows county-level maps of the state of North Carolina (NC) in red, and the state of Virginia (VA) in blue. The light-red county at the NC border is Stokes County. Since the only county located in VA that shares the same border with Stokes County is Patrick County (in light blue), we compare the deposits of these two counties. Throughout the paper, we refer to two such counties as a *county-pair* (or simply as a *pair*, interchangeably). Figure 1b provides a slightly different case of county-pair formation. Light red-painted Northampton County of NC shares the state border with three counties in VA: Southampton, Greensville, and Brunswick. This generates three different county-pairs in our empirical analysis: Northampton-Southampton, Northampton-Greensville, and Northampton-Brunswick. Figure 2 displays the location of all border counties used in our county-pair comparison analysis.

The two counties within a county-pair arguably have similar characteristics. They share the same geography and climate; have access to the same transportation routes; and more importantly are open to similar spillover effects of economic changes. Having similar characteristics and being neighbors to each other implies that a state-level economic shock is expected to affect the two counties within a county-pair symmetrically, since the economic conditions are continuous in the sense that state borders do not affect the movement of the economic shocks. Therefore, comparing the two counties within a county-pair controls for economic shocks that are expected to affect both state UI benefits and deposit levels. However, the two counties in a county-pair are subject to different levels of UI benefits since the UI policies are determined by state governments. This discontinuous variation in UI policies allows us to measure the effect of UI benefit on deposit levels. We refer to this approach as *within county-pair estimation*.

It is important to emphasize that the necessary identifying assumption behind within county-pair estimation is not that the two counties in a county-pair are similar, but that state-level economic shocks that may be correlated with state-level UI benefit changes do not stop at the state border, and affect the two counties within a county-pair symmetrically. In Section 4.3, we provide robustness checks and tests to support this argument.

For our within county-pair estimation, we estimate the following regression model:

$$\begin{aligned} \Delta log(deposit_{c,y}) = \beta \Delta log(UI_{s(c),y}) + \gamma_1 \Delta log(income_{c,y}) + \theta f(unemp.rate_{c,y}) \\ + \delta_{p(c),y} + + \eta_c + \epsilon_{c,y} \end{aligned} \tag{1}$$

where the dependent variable is the change in the log of the total deposits of county c from year $y-1$ to y, $\Delta log(UI_{s(c),y})$ is the contemporaneous log change in the UI benefits of the state where county c is located, $\delta_{p(c),y}$ are pair-year fixed effects for county-pair p where county c is located, and η_c are fixed effects for county c. Across different specifications, we also control for county income, and county unemployment rate up to its third-degree polynomial. The coefficient of interest is β, with an expectation of negative sign.

The pair-year fixed effects, $\delta_{p(c),y}$, are key to the within county-pair estimation, and allow different county-pairs to have time-varying differences from each other. Under our identifying assumption that state-level economic shocks affect the two counties in the pair symmetrically, using these fixed effects cancels out the effect of state shocks on the deposits of the two counties within the pair, and hence we identify the effect of state UI benefits on deposits. Including county fixed effects further controls for the unobserved time-invariant differences, while county income and unemployment rates control for observed time-varying differences (e.g., county-level economic activity, and labor market conditions) across counties within a county-pair.

Clustering standard errors needs special consideration. First, since the level of UI benefits is determined at the state level, the variable of interest is constant across counties within a state. This creates downward bias in standard errors. Second, since a border county in one state may have a common border with more than one county on the other side of the border, the same county may be in more than one county-pair, which generates a mechanical correlation across county-pairs. To account for these correlations in standard errors, we follow Dube et al. (2010), and double-cluster standard errors at the state and border segment level.[10]

4.2 Within county-pair estimation

Table 2 presents the main results for the deposit analysis. The analysis in columns (1) to (5) is at county level, and uses only the counties located at state borders. Each specification in these columns includes pair-year fixed effects, which means we are comparing the total deposits of the two border counties within a county-pair. Column (1) is our baseline specification with no control variables other than the pair-year fixed effects, and shows a negative and significant coefficient for state UI benefits. The economic meaning of this coefficient is that total county deposits decrease by 2.2 percent in response to a one standard deviation increase in the level of state UI benefits. In dollar terms, an individual in a median U.S. county decreases his deposit holdings by 82 USD when the state pays an additional 1,000 USD of unemployment insurance benefits.

We add additional controls to the regression in columns (2) through (5). To control time-invariant differences between the two counties in the pair, column (2) uses county fixed effects.

[10]"A border segment is defined as the set of all counties on both sides of a border between two states".(footnote 17, Dube et al. (2010))

Arguably, the total amount of county deposits is a function of county economic conditions; hence, we control for the county-level income[11] in column (3) as a proxy for county economic conditions. We further control for county labor market conditions that may be correlated with state-level economic conditions and hence with state UI by using county unemployment rate and its third-degree polynomial in columns (4) and (5), respectively. The coefficients across these columns are similar to that in column (1), and still highly significant.[12]

A potential concern in county-level results of columns (1) through (5), and an alternative explanation to our findings, is the heterogeneity of banks across state borders. The branch-opening decision of banks is not random. Banks endogenously choose which counties to locate their branches in based on the interplay of their own bank characteristics and the economic prospects of counties. This raises the concern that the characteristics of the banks that operate in one county may be different from those of the banks that operate in the other county within the pair; hence the banks in these two counties may differ in their incentives to raise deposits. For instance, these banks may differ in their country-wide lending opportunities, which creates heterogeneity among them in their incentives to raise deposits since deposits are the main funding source for bank lending. If these incentives are time-varying and correlated with the changes in economic conditions that drive changes in UI benefits, then our coefficient is biased. This discussion, in fact, relates to the discussion of whether the effect that we measure is demand-driven or supply-driven. The mechanism that we propose in this paper is supply-driven, meaning households save less and hence hold

[11]We also use county level wage income as a control instead of total income, and obtain similar results. When we use both variables in the regressions as controls, county income stays significant whereas county wage income becomes insignificant, which is consistent with our expectation as the two variables are highly positively correlated. Since county income is more relevant for deposits, we use county income as a proxy for county economic conditions.

[12]One additional concern is that changes in UI benefits may have coincided with changes in other state-level policies and these policies may be driving the deposits, not the UI benefits. To alleviate this concern, in an un-tabulated analysis, we include controls for changes in state level union coverage, health expenditures, non-UI transfer payments and minimum wages. The results suggest that other policies do not have significant effects on deposits while the effect of UI benefits stays relatively constant.

smaller deposits at banks when state UI becomes more generous. However, if the banks are heterogeneous across the state border, then their demand for deposits may respond heterogeneously to economic shocks, which may also explain our results.

To control bank-specific deposit demand across the two counties within a pair, in columns (6)-(7) of Table 2, we do branch-level analysis and use total branch-level deposits as our dependent variable. In these specifications, instead of using pair-year fixed effects, we use pair-bank-year fixed effects, which means we are comparing the deposits of the two branches of the same bank, one of them located in one county and the other one in the other county in the pair. The identifying assumption for this within-bank estimation is that the deposit demand of a bank is determined at the bank level, not at the branch level. The economic rationale behind this assumption is that banks can allocate deposits that they collect in one branch to another branch for lending to exploit the lending opportunities as much as possible. This implies that there is no reason for a bank to decrease its deposit demand in one branch, but increase it in another branch. Empirical evidence in the banking literature also supports this intuition (Gilje et al., 2016; Drechsler et al., 2017). Therefore, the bank demand for deposits stays constant across its branches, which allows us to measure the impact of UI benefits on household deposit supply. To make this within-bank estimation, in columns (6) and (7) we use only the sample of banks with branches in both counties in the pair, and exclude all others since the coefficient is not identified for single-county banks. Column (6) confirms our previous county-level deposit results. In column (7), we further refine the specification by including county-bank fixed effects. Absorbing time-invariant branch-level brand effects, we document a similar result. Therefore, both the within county-pair and the within bank analysis of Table 2 support our interpretation of the results as a supply-driven mechanism. To further rule out demand-driven explanations, we provide additional aggregate bank-level evidence in Section 4.4.

Showing that the branch-level and county-level analyses provide consistent results, we continue our analysis by using county-level data, as the branch-level analysis excludes more than three-quarters of the observations.[13]

To mitigate the concern that a possible pre-trend in county deposit growth may drive the deposit outcomes, we include the two lags and lead of the state UI growth into our model along with concurrent growth rate. Figure 3 presents the point estimates and 90 percent confidence intervals around the coefficients, and indicates that the lags and the lead of unemployment insurance are not statistically different from zero, and the coefficient of interest is still negative and significant. Having a significant effect for the coefficient of the contemporaneous UI benefits growth without any lag, however, raises the question of why we see an immediate effect of UI policies on deposit holdings of households. There are three points to note. First, we use annual SOD deposits data, meaning that we measure the cumulative effect of the change in UI policies on household deposit holdings throughout the whole year. Second, there are potentially two different mechanisms through which the changes in UI benefit may affect household saving behavior. The first one is that when the level of UI benefits increases, households may start to save less out of the income that they earn after the policy change. The second one is that households may start to spend their existing savings after the increase in UI benefits as they have less incentive to keep these savings. The impact on household savings of this latter mechanism is more immediate. Third, in the regressions, we use the variables in growth, not level. Therefore, an insignificant coefficient for the lead variable means that the bank deposits level does not come back to its previous level, implying a persistent effect on deposits.

[13] The robustness checks that we provide in the next two sections at the county level also hold at the branch level.

4.3 Endogeneity concerns

In this section, we discuss possible endogeneity concerns, and the robustness checks to mitigate these concerns. The robustness checks, and also the county-level and bank-level heterogeneity tests in the next section (Section 4.4) overall support our identification strategy and provide consistent results with the underlying economic mechanism that drives our results.

The first endogeneity concern is related to the similarity of two counties within a county-pair. Although the two counties are neighbors to each other and share the same geography, climate, and transportation routes, there is a potentially high degree of heterogeneity in terms of their features. For instance, the county-level U.S. map (Figure 2) shows that the distance between the centers of two counties within a pair is greater in the western part of the country than it in the eastern part, which implies that the border counties in the west are expected to be less similar to each other. If the counties within a pair are different along a dimension that is, for some reason, correlated with state UI benefits, this would create bias in our coefficient to the extent that we do not control for this dimension in our regression. To mitigate this concern, we make our within county-pair comparison for a subset of counties that are more similar to each other along several dimensions.

Each column in Table 3 uses a different criterion for county comparison, and excludes county-pairs from the sample if the counties in the pair are less similar to each other along that criterion. In column (1), for instance, the distance between the centers of two counties in the pair is used as a criterion for county similarity. We only use the county-pairs if the distance is less than or equal to 20 miles (i.e., in the first tercile of the distance distribution). The intuition is that we expect the economic conditions of these two counties to be more similar to each other as the distance between the counties decreases. The coefficient is similar to the one in the full sample.

Column (2) classifies the two counties in the pair as more similar based on their industrial composition. To make this classification, first we calculate the employment share of each industry in the counties by using the Regional Economic Information System of the Bureau of Economic Analysis (BEA). Next, we construct the Euclidian distance between the two counties in the pair, and include in our sample only the most similar counties (i.e., county-pairs with industry distance of less than the first tercile value). The idea is that the counties with similar industrial composition probably experience similar economic shocks. We obtain a similar coefficient with the one in our full sample.

In column (3), we analyze the banking sector of the two counties in the pair. Drechsler et al. (2017) show that when the Fed funds rate rises, banks that operate in more concentrated deposit markets experience larger deposit outflows. This would bias our coefficient if one of the counties in the pair has a more concentrated deposit market and experiences a higher increase in UI benefits relative to the other county in the pair during Fed funds rate hikes.[14] In this case, the decrease in deposit levels in the concentrated county may result from either the change in state UI benefits (household deposit supply) or the change in the Fed funds rate (bank deposit demand). Therefore, different banking sector competition (i.e., concentration) in the two counties in the pair may drive our deposit results. To mitigate this concern, we calculate the deposit market HHI of the counties, and restrict our sample to county-pairs where the two counties have similar HHIs. We find a similar coefficient.

In column (4), we use the core based statistical area (CBSA) definition of Office of Management and Budget, according to which the counties are in the same statistical area if they are similar and integrated to each other socioeconomically. Column (4) includes only the county-pairs if the counties in the pair are also in the same statistical area. Therefore, the economic conditions in these two counties are arguably similar to each other by construction.

[14]In the data, the level of regular UI benefits is positively correlated with the economic activity indicators. This may be because the improved state budget balances during economic expansions give state governments more room to increase UI benefits.

The coefficient is still negative and significant despite the dramatic decrease in the sample size.

The second endogeneity concern relates to the core of our identifying assumption. As discussed in the identification strategy section (Section 4.1), our main identifying assumption is that state-level economic shocks that are correlated with UI changes must affect the two counties in the county-pair symmetrically.[15] Since the level of UI benefits is determined at the state level, state-level economic conditions have the potential to affect the level of UI benefits and at the same time the level of county deposits. This is not an endogeneity concern only if these state-level economic conditions affect the other county in the county-pair symmetrically, in which case making a within county-pair comparison cancels out the impact of the state shock on county deposits. To empirically test whether state-level economic conditions affect the two counties in the pair symmetrically, we include relevant proxies for state-level economic conditions into our main regression. If the counties in the pair are affected symmetrically, then we should have a zero coefficient for the proxies for the state economic conditions (Hagedorn et al., 2018).

In columns (1) through (3) of Table 4, we use our main border county sample, and include state income, state GDP, and state unemployment rate into the regressions as proxies for state economic conditions, respectively. First, adding these state-level proxies has no significant effect on the coefficient of state UI benefits, mitigating the concern that state-level economic conditions may drive our results. Second, in each specification, the coefficients of the state-level proxy variables are insignificant. This means that state-level economic conditions affect each county in the pair symmetrically, and thus the net effect in the county-pair comparison is zero. Although these results are consistent with our identifying assumption, the remaining question is whether the state-level economic proxies that we

[15]The state level economic shocks may be driven either by the changes in economic conditions of the state itself, or by the heterogeneous responses of states to changes in nationwide aggregate macroeconomic conditions. Our identification strategy and robustness check appeal to both types of shocks.

use in columns (1) through (3) are relevant variables for the county deposits. If we use irrelevant state-level variables in the regressions, then the test has no power. To justify the use of these state-level proxies, therefore, we construct a random scrambled sample. Instead of matching two neighboring border counties located in different states, we match two non-neighboring counties located in different states. For instance, instead of pairing a North Carolina border county and a Virginia border county that share a common border, we match NC border county with a border county in California (CA). In this constructed border county sample, there should be discontinuity of economic conditions across the two counties in the pair by construction. Therefore, with the constructed sample, comparing the counties in the same pair does not cancel out the effect of state-level economic shocks on the deposits. This means that the proxies of state-level economic conditions should have statistically significant coefficients with the expected signs. The results in columns (4) through (6) confirm our identifying assumption. Namely, state income and state GDP, which are expected to affect deposits positively, have positive and significant coefficients, and state unemployment, which is expected to affect deposits negatively, has a negative and significant coefficient. These results ensure that the test we have in the first three columns has power. The other observation in columns (4) through (6) is that the coefficient of UI benefits is insignificant. This implies that when the economic conditions are not properly controlled for, our coefficient of interest is biased upward. Thus, the remaining correlation, if any, between UI benefit and the error term due to economic conditions in the main specification creates bias against our results. The findings of the robustness exercises in Table 3 are also are in line with this implication. As we constrain the sample to county-pairs with more similar counties, our estimates become more negative.

A similar but a slightly different form of the previous endogeneity concern is that the correlation between the two counties in the same pair might be lower than the correlation among the counties in the same state. This is a legitimate concern since the counties in the same state are subject to the same set of rules and regulations. If this concern is true, then the state-level economic conditions in the state are applicable to the same-state border county, but not to the across-state border county, which violates our identifying assumption. To mitigate this concern, we provide two exercises. First, we show that the counties in the pair are more similar to each other than they are to the rest of the counties in their own states. Second, we estimate our main specification with a sample in which the counties that are highly correlated with their own states are excluded, and find that the results do not change.

For the first exercise, Table 5 displays the results of two comparisons. The first one compares the characteristics of the neighbor border counties in the pair. The first three columns show the descriptive statistics of this comparison. In a similar way, we compare the characteristics of the border counties with the rest of the counties in their own states. The second three columns show the descriptive statistics of this comparison. In the last column, we calculate the difference between the two comparisons. A negative value in this column indicates that the border counties are more similar to each other than they are to the rest of the counties in the state. Almost all variables have a negative value, mitigating the concern that the counties are similar to each other within a state.

Table 6 presents the results of the second exercise, in which we exclude from the sample the counties that have a high correlation with their own states. For this exercise, we follow two different methodologies. First, we estimate the county income beta with respect to state income by regressing county income on state income, and exclude the border counties with high betas from the sample. Column (1) presents the result for this exercise, and confirms that the coefficient is still negative and significant. Second, we exclude counties from the sample if they are large relative to their states. If a county is large, then the change in county economic conditions is more influential on the changes in overall state-level economic conditions, which implies a high correlation between county and state economic conditions by definition. To exclude these counties, in column (2), we restrict our sample to the counties that have two percent or less of the state employment. The result confirms the negative and significant effect.

4.4 Heterogeneity

In this section, we exploit the heterogeneity at the county level and aggregate bank level to provide additional evidence that supports our conclusion: the change in the level of UI benefits is the driving force behind the decrease in deposits.

At the county level, we exploit the heterogeneity of counties in their sensitivities to the changes in UI benefits. If our economic mechanism is true then we should see stronger results for the subset of counties in which the changes in UI benefits are more relevant/binding. One possible way to test this is to classify counties based on their characteristics. For instance, since unemployment risk is higher for workers in the manufacturing industry,[16] the change in the level of UI benefits is expected to have a stronger impact on the saving behavior of workers in this industry, suggesting that our results should be stronger for counties where the employment share of the manufacturing industry is high. However, the U.S. UI system poses a challenge to using the share of manufacturing in the heterogeneity test. The challenge emerges in the following way: according to the UI system, changes in the level of UI benefits are not binding for low-income employees due to percentage caps the UI benefit schedules impose. For instance, the average percentage cap in our sample is 50 percent, and indicates that an unemployed worker is able to obtain UI benefits of up to 50 percent of his previous wage income. If the previous wage of this worker is too low, then the percentage cap will be binding for him, and he would not be able to take advantage of the increases in the level of UI

[16]See Table A1 in Agrawal and Matsa (2013) for average layoff separation rates of U.S. industries based on BLS "Mass Layoff Statistics"

benefits (i.e. the level of UI benefits are not binding for him). This implies that, according to our proposed economic mechanism, this worker must not change his saving behavior, and by extension his deposit holdings at banks. This means that the percentage cap the UI benefit schedules impose is binding for low-income workers in the manufacturing industry. Therefore, the changes in dollar cap (i.e. UI benefits – our variable of interest) are not binding for them. As a result, the design of the UI system in the U.S. creates non-linearity in the effect of industrial composition on the strength of the link between UI benefits and household saving. This makes it harder to conjecture on which industry workers the changes in the level of UI benefits have a stronger effect.

To overcome this challenge, instead of exploiting the heterogeneity of county characteristics, we focus on the realized UI payments of counties. More specifically, we obtain the UI beta for each county in the sample by regressing the county-level realized UI payments on state UI benefits. High UI beta for the county implies that the changes in state UI benefits are more binding for the workers in the county, while low UI beta implies they are less binding. Table 7 shows that the effect of UI benefits is stronger for high beta counties, while it is not significant for low beta ones, consistent with our prediction.

At the bank level, we turn to annual Call Reports data and exploit heterogeneity of deposit accounts. The driving force behind the mechanism that we propose for the deposit outcome is the change in the precautionary saving motives of households who face unemployment risk. Since these households have relatively low incomes, they are not expected to have large deposits in their bank accounts. Therefore, we expect to find that the effect of state UI benefits on small deposits is strong and significant, but insignificant on large deposits. From 2006 onwards, banks have started to report the amount of small deposits and large deposits in their Call Reports. To confirm our county-level deposits results, first, in column (1) of Table 8, we regress total bank deposits on bank UI exposure.[17] The result

[17]Note that the independent variable in this analysis is slightly different from the one we used in county-

in the first column verifies our county level analysis. In column (2) and column (3), we regress small deposits and large deposits on bank UI exposure, respectively, and show that the change in bank UI exposure has an impact only on small deposits.

Further bank-level evidence comes from deposit rates that banks pay to their depositors and the heterogeneity of banks in terms of the share of small deposits in their balance sheets. As we discuss in Section 4.2, the mechanism in this paper is supply-driven, which is supported by our branch-level analysis. To further support our interpretation of the results, we analyze the effect of UI changes on deposit rates. If the results are supply driven, then the price (deposit rate) and quantity (deposit amount) should move in opposite directions; on the other hand, if the results are demand-driven they should move in the same direction. We obtain the deposit rate from Call Reports by dividing the end of year total deposit interest expenses to lagged total deposits. Column (1) of Table 9 reports that banks pay more interest on their deposits when their UI exposure increases, supporting the supply mechanism. Furthermore, in column (2) and column (3), we split the banks into two subsamples based on the share of small deposits in their balance sheets, and show that deposit rate increase is stronger for banks with a higher small deposit ratio.[18] The small deposits are more critical in the funding structure for these banks; hence, they are more eager to pay higher interest rates (10 bps) to their depositors in order to prevent the decrease in deposits.

5 Lending analysis

In this section, we study the impact of UI generosity on bank commercial lending. Since banks heavily rely on deposits for their funding, and since they cannot perfectly replace

level analysis. Instead of using state UI benefit, we use a bank level UI exposure variable that shows the average UI level a bank faces, based on the states where the bank operates. See Section 3.1 for the constructions of this variable.

[18]The sample size decreases considerably as the information on small and large deposits is available only for the period after 2006.

deposits with other sources of funding, they are expected to squeeze their loan supply in response to an increase in their unemployment insurance exposure. The main identification challenge to test this prediction on loan supply is to control firm loan demand. If a firm's loan demand decreases as the UI exposure of its lenders increases, then the decline in the equilibrium amount of loans would be erroneously attributed to the increase in bank UI exposure.

5.1 Identification strategy

To address this identification challenge and to establish the causality from bank UI exposure to commercial lending, we follow Khwaja and Mian (2008), and implement within-firm estimation using annual firm-bank level Dealscan data on commercial loans by banks. In particular, we use firm-year fixed effects, and compare the loan amounts to the same firm in the same year by banks with different UI exposure. To make this within-firm estimation, we use only the sample of firms that work with at least two banks in a given year, and exclude all others since the coefficient is not identified for single-bank firms. Assuming that firm loan demand is symmetric across different banks, our empirical strategy holds loan demand fixed, hence enables us to uncover the effect of bank UI exposure on their loan supply.

For our within-firm estimation, we estimate the following regression model (a là Khwaja and Mian 2008):[19]

$$\Delta log(loan_{f,b,y}) = \beta \Delta log(UI^{bank}_{b,y-1}) + \gamma \Delta BankControls_{b,y-1} + \delta_{f,y} + \alpha_b + \epsilon_{f,b,y} \quad (2)$$

where the dependent variable is the change in log of the outstanding loan amount granted by bank b to firm f in year y, $\Delta log(UI^{bank}_{b,y-1})$ is the lagged change in log of the bank UI

[19]Examples of the Khwaja and Mian (2008) strategy include Jiménez et al. 2014 and, Amiti and Weinstein 2018.

exposure of bank b, $\delta_{f,y}$ are firm-year fixed effects for firm f, and α_b are bank fixed effects for bank b. Across different specifications, we also control for bank size, equity ratio, average deposit rate, net interest income ratio, and Herfindahl-Hirschman index (HHI) of the deposit markets where the banks operate. The coefficient of interest is β, with an expectation of negative sign. We double-cluster standard errors at the bank and firm level.

The key control in this within-firm comparison is firm-year fixed effects, which allows time-varying differences among firms. Under the assumption that a firm's loan demand across its lenders is symmetric, using firm-year fixed effects controls for the firm's loan demand and enables us to measure the effect of bank UI exposure on loan supply. Including bank fixed effects further control unobserved time-invariant differences among banks (e.g., bank management).

Note that, instead of using the estimated change in deposits caused by the change in UI benefits, we use bank UI exposure itself as our variable of interest. This allows us to measure the effect on loan supply of the UI policy itself, instead of the changes in deposits. To better understand this, consider a bank whose UI exposure increases takes an action to slow down the decrease in its deposits. Then, although the UI exposure of the bank increases, the bank does not experience a decrease in its deposits; hence, there should be no effect on its loan supply. If other banks in our sample also take similar actions, then the coefficient of bank UI exposure in our model would be insignificant. A model that uses the estimated decrease in deposits, on the other hand, can mask this ineffectiveness of the UI policies on bank lending, since it recovers the treatment effect on the treated banks. In other words, that model is designed to measure the decrease in loan supply only for the banks whose deposits decline due to an increase in UI benefits. However, in our model, we allow banks to take actions to prevent the decline in their deposits, and hence measure the effect of bank UI exposure on bank loan supply. Therefore, our model provides more insights about the true policy effects on bank loan supply.[20]

5.2 Within-firm estimation

Table 10 presents the results for commercial lending analysis. Each specification in the table includes firm-year and bank fixed effects, meaning we compare the lending of different banks to the same firm in the same year, and control for time-invariant bank characteristics. Column (1) is our baseline specification with no control variables other than the firm-year and bank fixed effects, and shows a negative and significant coefficient for bank UI exposure. The economic meaning of this coefficient is that a one standard deviation increase in bank UI exposure decreases the loan supply by 2.6 percent at the mean value. In column (2), we saturate the model with bank control variables that are used in the bank lending literature: bank size, equity ratio, liquidity ratio, profitability, deposit rate, and average HHI of deposit markets where the bank operates. The magnitude of the coefficient is similar to column (1), but its statistical precision increases.

In columns (3) through (6), we provide two additional pieces of evidence that support our interpretation of the results. First, we exploit the heterogeneity of banks in their ability to replace the decrease in deposits. Banks with lower equity ratios are expected to suffer more from agency problems (Holmstrom and Tirole, 1997), and thus have more difficulty in substituting the decrease in deposits with external wholesale funding. Therefore, we expect that these banks squeeze their lending supply more. In columns (3) and (4), we split the banks into two subsamples based on their equity ratio. In line with our expectation, the banks with low equity ratios decrease their lending more, whereas the effect is insignificant for the banks with high equity ratios.

[20] Our model is comparable to the intention-to-treat effect estimator. For more discussion about the intent-to-treat and treatment-on-treated see *Mostly Harmless Econometrics* (Angrist and Pischke, 2008), and Dupas et al. (2018).

Second, in columns (5) and (6), we report the results by dividing the sample into two based on the share of small deposits in bank balance sheets, and find that the effect is stronger for banks that have high small deposit ratios. This exercise serves two purposes. First, the exercise is parallel to the one that we do on deposit analysis, in which we show that the decrease in deposits is especially strong for banks with high small deposit ratios, suggesting that the lending effect should also be stronger for these banks. Second, this exercise helps us to mitigate concerns about omitted variable bias. Namely, if there were an unobserved bank-level variable correlated with bank UI exposure, our results could be driven by this variable. Yet, the findings in columns (5) and (6) depict that the concerns about the omitted variable bias are valid only if the unobserved bank-level variable is correlated with the bank small deposit ratio as well as bank UI exposure, which is highly unlikely. Therefore, finding stronger results for commercial lending for the banks with a high small deposit ratio further supports our mechanism.

6 Investment analysis

Lastly, in this section, we test whether firms that borrow from banks with high unemployment insurance exposure experience a reduction in their investment. Consistent with the literature, we find that the decrease in firms' access to bank lending adversely affects their investment. The effect is especially strong for financially constrained firms, which implies that these firms cannot substitute bank lending with other sources of external funding (e.g., bond issuance).

6.1 Identification strategy

For our investment analysis, we estimate the following regression model:

$$\begin{aligned} investment_{f,y} = \beta UI^{firm}_{f,y-1} + \gamma BankControls_{b,y-1} + \kappa FirmControls_{f,y-1} \\ + \delta_f + \alpha_b + \eta_{ind,y} + \lambda_{loc,y} + \epsilon_{f,y} \end{aligned} \tag{3}$$

where the dependent variable is firm f's investment ratio (capital expenditure divided by lagged assets) in year y; $UI^{firm}_{f,y-1}$ is firm f's lagged UI benefit exposure; δ_f, α_b, $\eta_{ind,y}$, $\lambda_{loc,y}$ are firm, bank, firm's industry-year, and firm's location-year fixed effects, respectively. We saturate our model with bank controls, and firm controls. The coefficient of interest is β, with an expectation of a negative sign. We cluster standard errors at the firm level.

A major concern about this exercise is that a negative coefficient on β might be a consequence of the effects of UI benefits on labor markets rather than the bank lending channel that we aim to identify. For instance, if higher UI benefit decreases job search intensity and increases the equilibrium wage, then a decline in firm investment might be due to lower firm employment creation induced by the higher UI benefit in the firm's location.[21] It is also possible that an economic shock could lead to higher UI benefits and lower firm investment demand, which would create a spurious correlation between UI benefits and firm investment.

To tackle these concerns, in our specification we include the firm's location-year fixed effects, which means we compare the firms that face the same level of state UI benefits but

[21] The labor search literature discusses two types of effects: micro and macro (Diamond, 1982; Mortensen and Pissarides, 1994).The negative effect of UI benefits on the job search intensity of individuals is called the micro effect, and the negative effect of UI benefits on the job creation of firms due to higher equilibrium wage is called the macro effect. More recently, these effects are also discussed in (Agrawal and Matsa, 2013), and Hagedorn et al. (2018). On the one hand, the results in Hagedorn et al. (2018) imply that firm investment decreases if UI benefits increase due to a higher equilibrium wage. On the other hand, since the compensating wage premium that employees ask for decreases as UI benefits increase, Agrawal and Matsa (2013) suggest that the firm can pass the freed cash flow on the investment leading to a higher firm investment ratio.

have different UI exposure through their lenders. Moreover, we include industry-year fixed effects to control for industry-specific shocks. These controls warrant that we measure only the bank channel of UI on firm outcomes.

6.2 Investment results

Table 11 presents the results for our investment analysis. Each column includes firm, state-year, and industry-year fixed effects, meaning we are controlling time-invariant firm-level covariates, state-level economic shocks, and industry-level economic shocks. Column (1) of Table 11 is our baseline specification with no firm and bank controls, and shows that as firm UI exposure increases by one standard deviation, firm investment ratio decreases by 23 basis points. This magnitude implies that the firm investment level decreases by 3 percent at its mean value. In column (2), we include firm controls (Tobin's Q, leverage ratio, size, Z-score) and bank controls (size, equity ratio, and liquidity ratio).[22] Adding firm and bank control variables does not change the magnitude of the coefficient. These results show that the decrease in bank lending induced by the decline in deposits in response to more generous UI benefits has real consequences on firm investment.

In columns (3) and (4), we split the sample of firms into two groups based on their financial constraints. We follow the literature and use firm size as a proxy for firm financial constraints. Small firms suffer more from agency problems and hence have more limited access to external funding sources other than bank lending. Therefore, for our investment analysis, we should find stronger results for small firms. The results confirm our intuition. The effect is larger for small firms, whereas the coefficient for the sample of large firms is insignificant.

Columns (5) and (6) divide the sample of firms into two based on the share of small

[22] If a firm is served by more than one bank, the bank variables are the weighted average of individual bank variables by using the outstanding loan amount between the firm and its lenders as weights.

deposits in their lenders' balance sheets, and produce consistent results with the exercises that we previously did for deposits and commercial lending analysis. In those previous exercises, we find that the decrease in total deposits is mainly due to decreases in small deposits, and that the banks that have a higher reliance on small deposits squeeze their loan supply more. This implies that the decline in firm investment should be stronger if the firm's lender heavily relies on small deposits for its funding. The results are in line with our hypothesis, and support our interpretation of the results: the observed decline in firm investment is due to a decrease in loan supply.

In Table 12, we focus only on the sample of small firms. As in the previous table, each specification includes firm and bank control variables as well as firm and industry-year fixed effects. In column (1), we refine the spatial control by including county-year fixed effects instead of state-year fixed effects. With industry-year fixed effects, we compare the investment levels of different firms located in the same county while controlling for industry-specific investment demand. Even in this tight specification, the coefficient is still statistically significant and the magnitude does not change.[23] In column (2), we introduce bank fixed effects to control for time-invariant bank characteristics that may affect their lending behavior. The coefficient is still negative and statistically significant. Note that if a firm is working with more than one bank, we use the largest bank as the lender of the firm to define bank fixed effects. Since this assumption is not perfect, we introduce an attenuation bias, which explains the drop in the magnitude of the coefficient. Furthermore, including the bank fixed effects reduces the variation in bank UI exposure, and hence the variation in firm UI exposure, which may also partly explain the drop in the size of coefficient. In columns (3) and (4), to further investigate the bank-level external funding frictions, we divide our sample into two based on the equity ratio of the banks. Consistent with the loan outcome results, the effect is stronger for firms that borrow from the low equity banks. The effect is

[23]Note that the comparable specification is reported in column (4) of Table 11.

still negative for the other half of the sample, but the coefficient is not significant.

7 Discussion and Conclusion

It has been well documented that, both theoretically and empirically, lower income risk reduces precautionary savings for households. As UI benefits reduce the left tail of income risk, a rare but disastrous state for an individual, it also has similar, likely larger, effects. What has been missing is an analysis of the natural link between unemployment insurance, household savings and bank deposits, and bank lending. We aimed to fill this gap with this paper.

We have three sets of results. First, using both county- and bank-level data we show that more generous UI benefits reduce bank deposits. Second, we use matched bank-firm data from Dealscan and show that banks that collect deposits from counties that have more generous UI benefits originate less credit to firms. Third, we use Compustat data and find that firms that work primarily with banks that raise deposits in regions with higher UI benefits have lower investment. All of our results indicate both statistically and economically significant effects. Taken all together, our findings provide a strong set of evidence that UI benefits distort bank funding and commercial lending, and hence have an adverse impact on firm investment.

Our findings rely on U.S. data. In the U.S., social welfare programs are relatively less generous and firms finance themselves primarily from financial markets rather than from banks. Therefore, we suspect that the mechanisms highlighted in our paper may be even stronger in countries where both UI coverage ratios are larger and the duration of UI payments is longer, such as in European countries. Besides, since non-US firms are much more bank-dependent than their US counterparts, the real effects of bank UI exposure on firm outcomes may be stronger for these firms.

UI benefits certainly affect employed and unemployed people differently. For example, recent evidence by Hsu et al. (2018) suggests that UI benefits reduce default probability of the unemployed. Similarly, UI benefits are found to lower job search intensity and increase reservation wages for the unemployed. Different from this literature, our results are unconditional, i.e., UI benefits lower precautionary motive of every individual in the economy irrespective of the employment state. Therefore, the macroeconomic effects are likely to be stronger compared to the studies that base their analysis only to the unemployed, which forms on average about 5-6 percent of population.

Similar to many papers, we use cross-sectional data to identify the causal mechanism. As a result, our findings compare how different counties, banks and firms behave from their counterparts as the UI benefits that they face changes. By construction, this kind of methodology cannot say anything about the effects of the mean UI benefits on the macro economy. For that, one needs to have a general equilibrium model with an explicit treatment of income and unemployment risk, precautionary savings and bank lending. This is a topic of an ongoing research.

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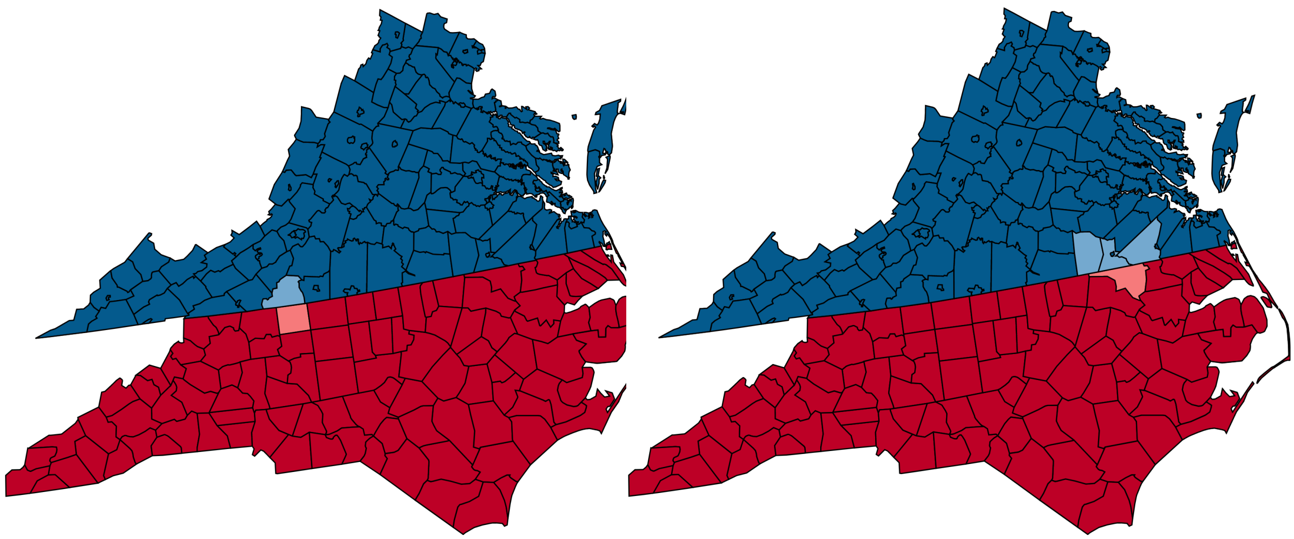
Figure 1
NC and VA County-Level Map: County-Pair Formation

This figure is the county-level map of the state of North Carolina (NC) and the state of Virginia (VA), and provides two examples that show how we form our county-pairs. NC is depicted in red and VA is depicted in blue. In Panel a, the light-red county at NC border is Stokes County, and the light-blue county at VA border is Patrick County. Since the only county located in VA that shares the same border with Stokes County is Patrick County, Stokes County has only one county to compare. Therefore, Stokes (NC) and Patrick (VA) form a county-pair. In Panel b, the light-red county is Northampton county (NC). Northampton shares the state border with three counties in VA: Southampton, Greensville, and Brunswick. This generates three different county-pairs: Northampton-Southampton, Northampton-Greensville, and Northampton-Brunswick.



(a) **(b)**

Figure 2
Border Counties

This figure shows the location of all U.S. border counties used in our county-pair comparison analysis. The counties in dark blue are used in our analysis, and the counties in light blue are excluded from the sample

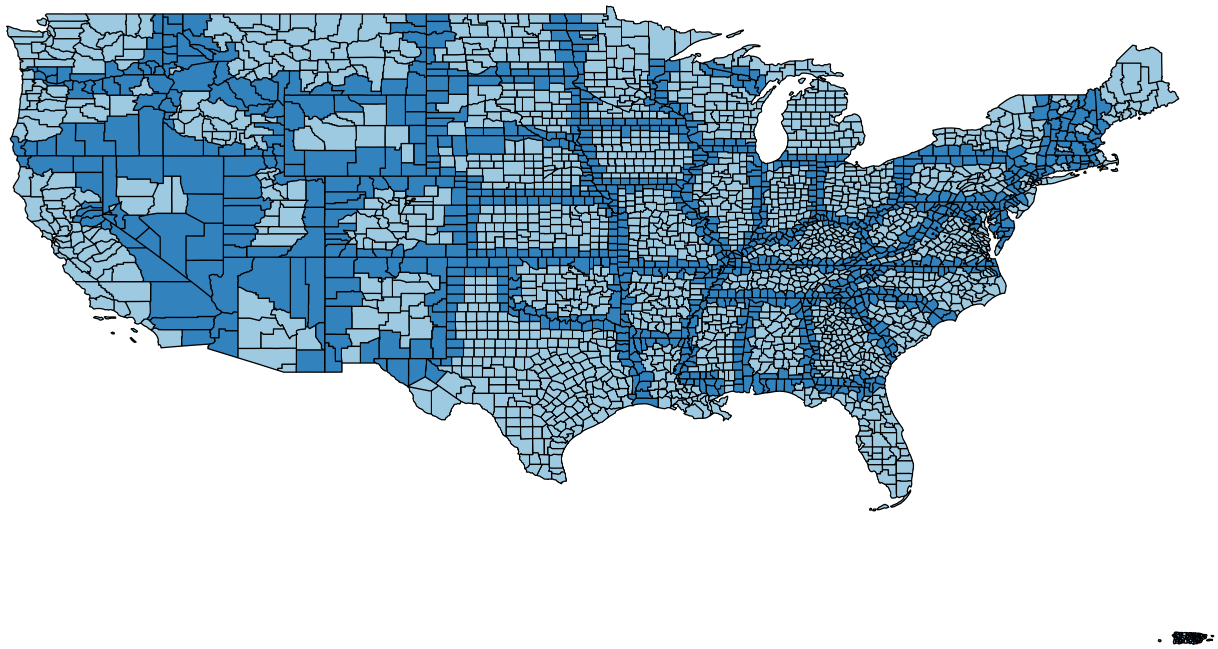


Figure 3
Dynamic Effects

This figure shows the dynamic effects of UI benefits growth on deposits growth. We include the two lags and lead of state UI growth into our main model along with concurrent growth rate. The figure presents the point estimates and 90 percent confidence intervals around the coefficients of lags, concurrent and lead of UI benefits growth.

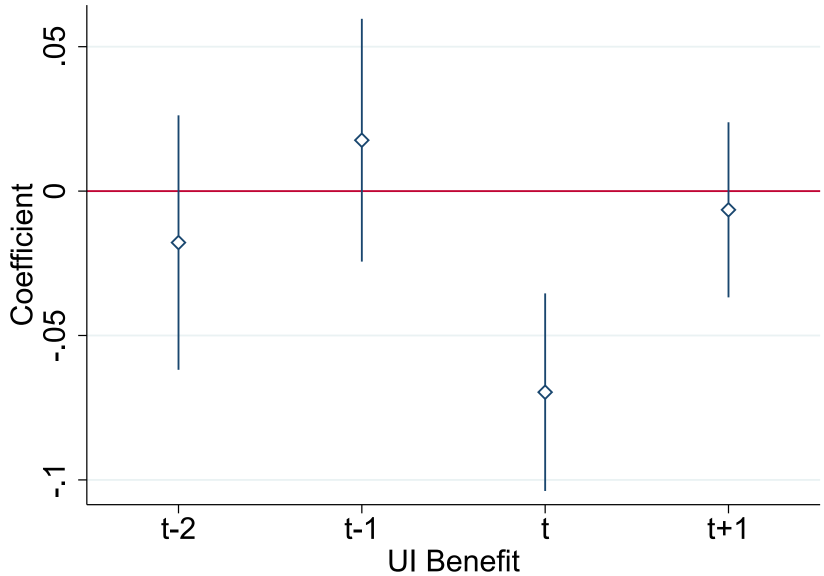


Table 1
Summary Statistics

This table provides summary statistics at the county, bank, and firm levels for the period between 1994 and 2010. Panel A presents the summary statistics at the county level for the sample of border counties used in the deposit analysis. Panel B reports summary statistics at the firm-bank level for the commercial lending analysis. Since a within-firm estimation is implemented, the sample includes only the sample of firms that have lending relationships with multiple lenders. Panel C presents firm-level summary statistics for the investment analysis.

	Mean	SD	25^{th} perc.	Median	75^{th} perc.
A- County Characteristics					
UI Benefit, weekly (tho. $)	0.33	0.10	0.26	0.31	0.39
UI Benefit, duration (weeks)	26.08	0.54	26.00	26.00	26.00
UI Benefit (tho. $)	8.54	2.69	6.66	8.11	10.06
UI Benefit, growth (%)	3.40	3.65	0.00	3.20	4.51
Deposit (mil. $)	1921	12338	130	313	768
Deposits, growth (%)	3.59	5.94	0.45	3.30	6.34
Income (mil. $)	2933	9252	254	624	1729
Income, growth (%)	4.31	4.93	1.92	4.42	6.74
HHI, county	0.31	0.19	0.18	0.25	0.39
Obs. (county × year)	36,874				
B- Bank Characteristics					
Bank UI exposure (tho. $)	10.00	2.21	8.46	10.08	10.95
Loan amount (mill. $)	566	1243	56	208	578
Loan amount, growth (%)	4.72	66.43	0.00	0.00	0.00
Size (bill. $)	475	532	80	212	655
Size, growth (%)	10.02	17.59	0.50	5.91	13.14
Equity (%)	9.03	1.66	8.07	9.06	10.12
Securities (%)	15.11	7.00	10.01	14.17	18.72
Core deposits (%)	44.54	15.39	33.05	48.39	55.23
Profitability (%)	1.23	0.80	0.94	1.24	1.64
HHI, Bank	0.18	0.04	0.16	0.18	0.20
Obs. (firm × bank × year)	174,151				
C- Firm Characteristics					
Firm UI exposure (tho. $)	9.67	1.74	8.35	9.99	10.82
Investment rate (%)	7.11	7.93	2.44	4.64	8.47
Size (bill. $)	5.82	18.05	0.29	0.99	3.67
Tobin's q	1.69	2.28	1.05	1.34	1.86
Current ratio (%)	2.00	14.97	1.11	1.60	2.29
Leverage (%)	25.87	25.78	10.17	22.86	35.93
Fixed coverage	21.09	394.69	1.17	2.61	6.03
Altman's z-score	3.05	12.99	1.43	2.57	4.04
Obs. (firm × year)	29,685				

Table 2
Deposits and UI Benefits: Within-Pair & Within-Bank Estimation

This table estimates the effect of state UI benefits on bank deposits. Columns (1) through (5) use county-year level data for the period between 1994 and 2010, and provide the results of a regression model where the dependent variable is the log change in county total deposits, and the main independent variable is the contemporaneous log change in the UI benefits of the state where the county is located. The sample includes all U.S. border counties. The key fixed effect in these columns are pair-year fixed effects. Columns (6) and (7) use county-bank-year level data for the period between 1994 and 2010, and provide the results of a regression model where the dependent variable is the log change in branch total deposits, and the main independent variable is the contemporaneous log change in the UI benefits of the state where the branch is located. The key fixed effects in these columns are pair-year-bank fixed effects. Only the sample of banks with branches in both counties in a pair is used, since the coefficient is not identified for single-county banks. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at the level of state and border segment (i.e., the set of all counties on both sides of a border between two states).

	$\Delta log(County Deposit)$					$\Delta log(Branch Deposit)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta log(UIBenefit)$, State	-0.054***	-0.059***	-0.061***	-0.061***	-0.061***	-0.106***	-0.085**
	(0.015)	(0.016)	(0.016)	(0.016)	(0.015)	(0.038)	(0.039)
$\Delta log(Income)$, County			0.043***	0.043***	0.045***	0.116***	0.089**
			(0.015)	(0.015)	(0.015)	(0.041)	(0.039)
Controls & Fixed Eff:							
Unemp.	N	N	N	Y	Y	Y	Y
cubic(Unemp.)	N	N	N	N	Y	Y	Y
Pair–Year FE	Y	Y	Y	Y	Y	N	N
County FE	N	Y	Y	Y	Y	Y	N
Pair–Year–Bank FE	N	N	N	N	N	Y	Y
County–Bank FE	N	N	N	N	N	N	Y
Obs.	36,148	36,148	36,148	36,148	36,148	37,012	37,012
R^2	0.553	0.596	0.597	0.597	0.597	0.608	0.678

Standard errors in parentheses
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 3
Within-Pair Estimation: County Characteristics

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-year level data for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the log change in county total deposits, and the main independent variable is the contemporaneous log change in the UI benefits of the state where the county is located. The key fixed effects in each column are pair-year fixed effects. Each column makes a within-pair estimation by using a subset of counties that are more similar to each other along a specific dimension. Column (1) uses only the county-pairs if the distance between the centers of two counties within a pair is less than or equal to 20 miles. Column (2) uses only the county-pairs if the Euclidian distance of industrial compositions of the two counties within a pair is less than or equal to the sample tercile value. Column (3) uses only the county-pairs where the two counties in a pair have similar deposit market concentration (i.e., similar county deposit market HHI). Column (4) uses only the county-pairs if the two counties in a pair are also in the same core-based statistical area.Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at state and border segment level.

	$\Delta log(County\,Deposit)$			
	(1)	(2)	(3)	(4)
	Distance	Industry	Banking	CBSA
$\Delta log(UIBenefit)$,	-0.063**	-0.080***	-0.073***	-0.122***
State	(0.026)	(0.020)	(0.018)	(0.038)
$\Delta log(Income)$,	0.059**	0.046	0.044	0.007
County	(0.024)	(0.030)	(0.027)	(0.067)
Controls & Fixed Eff:				
Unemp.	Y	Y	Y	Y
cubic(Unemp.)	Y	Y	Y	Y
Pair–Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
Obs.	12,086	12,122	11,488	4,576
R^2	0.583	0.607	0.629	0.592

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 4
Within-Pair Estimation: Continuous Economic Conditions

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-year level data for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the log change in county total deposits, and the main independent variable is the contemporaneous log change in the UI benefits of the state where the county is located. The key fixed effects in each column are pair-year fixed effects. Columns (1) through (3) use the main county-pair sample, and use a specification comparable to column (5) of Table 2, with the only difference of having additional state level control variables. Columns (4) through (6) use the same specification and control variables as in columns (1) through (3), but use a randomly constructed scrambled sample. Instead of matching two neighboring border counties located in different states, the scrambled sample matches two non-neighboring border counties located in different states. Control variables and fixed effects are indicated at the bottom of each column, and the coefficients of the additional state-level variables are reported in the relevant columns. Standard errors are two-way clustered at state and border segment level.

	Dependent Variable: $\Delta log(CountyDeposit)$					
	Main Sample			Scrambled Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta log(UIBenefit)$,	-0.061***	-0.061***	-0.062***	-0.008	-0.006	-0.013
State	(0.016)	(0.015)	(0.016)	(0.017)	(0.017)	(0.017)
$\Delta log(Income)$,	0.045***	0.044***	0.045***	0.075***	0.086***	0.099***
County	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)
$\Delta log(Income)$,	-0.001			0.225***		
State	(0.048)			(0.043)		
$\Delta log(GDP)$		0.019			0.144***	
State		(0.036)			(0.027)	
$Unemp.rate$			-0.191			-0.636***
State			(0.144)			(0.108)
Controls & Fixed Eff:						
Unemp. controls	Y	Y	Y	Y	Y	Y
Pair–Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Obs.	36,148	36,148	36,148	35,974	35,974	35,974
R^2	0.597	0.597	0.597	0.569	0.569	0.570

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 5
County Comparisons: Pair County vs. State Counties

This table provides the summary statistics of two comparisons. The first three columns (under the heading of |Pair-County|) show the descriptive statistics for the comparison in which we compare the characteristics of the neighboring border counties in a pair. The second three columns (under the heading of |Rest-County|) show the descriptive statistics of the comparison in which we compare the characteristics of the border counties with the rest of the counties in their own states. Comparison is made by calculating the difference between the relevant characteristics of the counties, and then taking the absolute value of the difference. In the last column, we calculate the difference of the mean of the two comparisons. A negative value in the last column indicates that the border counties are more similar to each other than they are to the rest of the counties in their own state.

	|Pair-County|			|Rest-County|			Diff.
	Mean	Med	SD	Mean	Med	SD	– Diff.
log(deposit)	0.99	0.81	0.80	1.41	1.25	1.01	-0.43***
deposit, growth(%)	1.33	1.07	1.08	2.20	1.68	1.87	-0.87***
log(income)	0.94	0.76	0.75	1.35	1.23	0.89	-0.41***
income, growth(%)	0.89	0.64	0.88	0.96	0.74	0.81	-0.07*
log(ave. wage)	0.16	0.13	0.13	0.28	0.27	0.16	-0.12***
ave. wage, growth(%)	0.55	0.44	0.45	0.48	0.37	0.42	0.07***
log(labor force)	0.90	0.74	0.71	1.25	1.12	0.85	-0.34***
labor force, growth(%)	0.79	0.62	0.70	0.87	0.73	0.69	-0.08**
unemployment rate (%)	1.12	0.83	1.04	1.31	0.91	1.40	-0.20***
manufacturing share (%)	0.09	0.07	0.08	0.08	0.07	0.07	0.01**
HHI, county	0.15	0.11	0.15	0.13	0.10	0.13	0.02***
Observations	997			997			1994

Table 6
Excluding Correlated Counties

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-year level data for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the log change in county total deposits, and the main independent variable is the contemporaneous log change in the UI benefits of the state where the county is located. The key fixed effects in each column are pair-year fixed effects. Column 1 excludes the counties that have a high correlation with their own states. The correlation criterion is county income beta with respect to state income (i.e., the coefficient of the regression of county income growth on state income growth). Column (2) excludes the counties that have two percent or more of the state employment level. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at state and border segment level.

	$\Delta log(County Deposit)$	
	(1) Inc. Beta, Low	(2) Emp. share, Low
$\Delta log(UIBenefit)$, State	-0.066**	-0.046**
	(0.027)	(0.018)
$\Delta log(Income)$, County	0.041*	0.042***
	(0.022)	(0.014)
Controls & Fixed Eff:		
Unemp.	Y	Y
cubic(Unemp.)	Y	Y
Pair–Year FE	Y	Y
County FE	Y	Y
Obs.	9,312	24,076
R^2	0.569	0.582

Standard errors in parentheses
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 7
Heterogeneity: County UI Sensitivity

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-year level data for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the log change in county total deposits, and the main independent variable is the contemporaneous log change in the UI benefits of the state where the county is located. The key fixed effects in each column are pair-year fixed effects. Columns (1) and (2) split the county-pair sample into two subsamples based on their UI beta. UI beta is obtained by regressing the county-level realized UI payments on state UI benefits. High UI beta implies that changes in state UI benefits are more binding for the workers in the county, while low UI beta implies the opposite. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at state and border segment level.

	$\Delta log(County Deposit)$	
	(1) County UI Beta, High	(2) County UI Beta, Low
$\Delta log(UI Benefit)$, State	-0.097***	-0.049
	(0.034)	(0.030)
$\Delta log(Income)$, County	0.020	0.013
	(0.024)	(0.043)
Controls & Fixed Eff:		
Unemp.	Y	Y
cubic(Unemp.)	Y	Y
Pair–Year FE	Y	Y
County FE	Y	Y
Obs.	5,472	5,646
R^2	0.601	0.623

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 8
Bank-Level Analysis: Heterogeneity across Deposit Types

This table estimates the effect of bank UI exposure on bank deposits. Each column uses bank-year level data from Call Reports for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the log of bank deposits, and the main independent variable is the contemporaneous bank UI exposure. The sample excludes the banks with a loan ratio smaller than 20 percent, and a core deposit ratio smaller than 30 percent. As the dependent variable, columns (1), (2), and (3) use bank total deposits, bank small deposits (i.e., deposits smaller than 250k), and bank large deposits (i.e., deposits larger than 250k), respectively. The sample period for columns (2) and (3) is 2006-2010, during which Call Reports provide information on small and large deposits. Bank controls: size, bank-level average HHI. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and year level.

	Bank Deposit		
	(1)	(2)	(3)
	All	<250k	>250k
UI Exposure, Bank	-0.026*	-0.146**	0.017
	(0.015)	(0.039)	(0.030)
Controls & Fixed Eff:			
Bank Controls	Y	Y	Y
Bank FE	Y	Y	Y
Year FE	Y	Y	Y
Obs.	106,556	28,488	28,424
R^2(Adj.)	0.956	0.964	0.954

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 9
Bank-Level Analysis: Deposit Rate

This table estimates the effect of bank UI exposure on bank deposit rate. Each column uses bank-year level data from Call Reports for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the bank deposit rate, and the main independent variable is bank UI exposure. The sample excludes the banks with loan ratio smaller than 20 percent, and core deposit ratio smaller than 30 percent. Deposit rate is calculated by dividing the end of year total deposit interest expenses to lagged total deposits. Columns (2) and (3) split the sample of banks into two based on their small deposits ratio. The sample period for columns (2) and (3) is 2006-2010, during which Call Reports provide information on small and large deposits. Bank controls: size, equity ratio, liquid asset ratio, bank-level average HHI. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and year level.

	Deposit Rate		
	(1)	(2)	(3)
		Small Dep.	Small Dep.
	All	Share, High	Share, Low
UI Exposure, Bank	0.035*	0.101*	0.065
	(0.020)	(0.040)	(0.041)
Controls & Fixed Eff:			
Bank Controls	Y	Y	Y
Bank FE	Y	Y	Y
Year FE	Y	Y	Y
Obs.	94,889	9,206	10,023
R^2(Adj.)	0.821	0.882	0.870

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Table 10
Commercial Loan and Bank UI Exposure: Within-Firm Estimation

This table estimates the effect of bank UI exposure on bank loan supply. Each column uses firm-bank-year level data from Dealscan for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the annual log change in the outstanding loan amount between bank and firm, and the main independent variable is bank UI exposure. The key fixed effects in each column are firm-year fixed effects. The sample, in Columns (1) and (2), consists of only firms that work with at least two banks in a given year, and excludes all others since the coefficient is not identified for single-bank firms due to firm-year fixed effects. Columns (3) and (4) split the sample of banks into two subsamples based on their equity ratio. Columns (5) and (6) split the sample of banks into two subsamples based on the ratio of small deposit to bank total assets. Bank controls: size, equity ratio, asset share of deposits, return on assets, and bank-level average HHI. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and firm level.

	Dependent Variable: $\Delta log(loan)$					
	All		Equity Ratio		Small Deposit Ratio	
	(1) All	(2) All	(3) High	(4) Low	(5) High	(6) Low
$\Delta log(UI\,Exposure)$, Bank	-0.125*	-0.129**	-0.054	-0.316**	-0.332***	-0.098
	(0.071)	(0.052)	(0.088)	(0.137)	(0.094)	(0.091)
Controls & Fixed Eff:						
Bank controls	N	Y	Y	Y	Y	Y
Firm–Year FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Obs.	174,179	174,179	45,377	46,498	34,222	37,140
R^2	0.647	0.648	0.708	0.674	0.723	0.729

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 11
Firm Investment

This table estimates the effect of firm UI exposure on firm investment. Each column uses firm-year level data from Compustat for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the firm investment rate (i.e., end-of-year capital expenditure divided by lagged assets), and the main independent variable is firm UI exposure, which is calculated by taking the weighted average of the UI exposures of firm's lenders using the outstanding loan amount between the firm and its lenders as weights. Columns (3) and (4) split the sample of firms into two subsamples based on their size. Columns (5) and (6) split the sample of firms into two subsamples based on their lenders' small deposit ratio. Firm controls: firm size, marginal Q, leverage, and Altman's Z-score. Bank controls: bank size, equity ratio, liquid assets ratio. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and firm level.

	Dependent Var.: Capital Expenditure/Assets			
	All		Firm Size	
	(1)	(2)	(3)	(4)
	All	All	Large	Small
UI Exposure, Firm	-0.234**	-0.249**	-0.137	-0.448***
	(0.109)	(0.093)	(0.216)	(0.098)
Controls & Fixed Eff:				
Firm Controls	N	Y	Y	Y
Bank Controls	N	Y	Y	Y
Firm FE	Y	Y	Y	Y
State–Year FE	Y	Y	Y	Y
Industry–Year FE	Y	Y	Y	Y
Obs.	25,255	25,255	11,042	12,533
R^2(Adj.)	0.669	0.709	0.755	0.702

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Table 12
Firm Investment, Small Firms

This table estimates the effect of firm UI exposure on firm investment. Each column uses firm-year level data from Compustat for the period between 1994 and 2010, and provides the results of a regression model where the dependent variable is the firm investment rate (i.e., end-of-year capital expenditure divided by lagged assets), and the main independent variable is firm UI exposure, which is calculated by taking the weighted average of the UI exposures of firm's lenders using the outstanding loan amount between the firm and its lenders as weights. The sample includes only small firms (i.e., firms with assets size smaller than the sample median). Columns (3) and (4) split the sample of firms into two subsamples based on their lenders' equity ratio. Firm controls: firm size, marginal Q, leverage, and Altman's Z-score. Bank controls: bank size, equity ratio, liquid assets ratio. For bank fixed effects, we use the biggest lender of the firm in a given year. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and firm level.

	All		Equity ratio	
	(1)	(2)	(3)	(4)
	All	All	High	Low
UI Exposure, Firm	-0.421***	-0.293***	-0.305	-0.378**
	(0.113)	(0.069)	(0.204)	(0.151)
Controls & Fixed Eff:				
Firm Controls	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Bank FE	N	Y	N	N
State–Year FE	N	Y	Y	Y
County–Year FE	Y	N	N	N
Industry–Year FE	Y	Y	Y	Y
Obs.	11,016	13,386	5,516	6,549
R^2(Adj.)	0.706	0.702	0.748	0.691

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

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