Finance and Economics Discussion Series
Divisions of Research & Statistics and Monetary Affairs
Federal Reserve Board, Washington, D.C.

Unintended Consequences of Unemployment Insurance Benefits: The Role of Banks

Yavuz Arslan, Ahmet Degerli, Gazi Kabaş

2021-027

Please cite this paper as:
Arslan, Yavuz, Ahmet Degerli, and Gazi Kabaş (2021). "Unintended Consequences of Unemployment Insurance Benefits: The Role of Banks," Finance and Economics Discussion Series 2021-027. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/FEDS.2021.027.

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

Yavuz Arslan† Ahmet Degerli‡ Gazi Kabaş§

March 2021

Abstract

We use disaggregated U.S. data and a border discontinuity design to show that more generous unemployment insurance (UI) policies lower bank deposits. We test several channels that could explain this decline and find evidence consistent with households lowering their precautionary savings. Since deposits are the largest and most stable source of funding for banks, the decrease in deposits affects bank lending. Banks that raise deposits in states with generous UI policies squeeze their small business lending. Furthermore, counties that are served by these banks experience a higher unemployment rate and lower wage growth.

*We thank Manuel Adelino, Alexander Bechtel, Elizabeth A. Berger, Alon Brav, Sung Je Byun, Charles Calomiris, Stijn Claessens, Anna Cieslak, Dragana Cvijanovic, Mathias Drehmann, Dirk Hackbarth, Leming Lin, David Matsa, Steven Ongena, Manju Puri, Jonathan Reuter, Andre F. Silva, Bent E. Sorensen, Vish Viswanathan, and seminar/conference participants at BIS, European Finance Association 2020, Fuqua Finance Brownbag, MoFiR Workshop on Banking 2019, SFI Research Days 2019, SFS Cavalcade North America 2019, Swiss Winter Conference on Financial Intermediation 2019, Western Economic Association International Conference 2019, Western Finance Association 2019, and the University of Zurich for their helpful comments. The views expressed here are those of the authors and not necessarily those of the Bank for International Settlements and the Federal Reserve Board. Gazi Kabaş gratefully acknowledges financial support from the European Research Council (ERC).

†Bank for International Settlements. Email: yavuz.arslan@bis.org

‡Federal Reserve Board. Email: ahmet.degerli@frb.gov

§Swiss Finance Institute, University of Zurich. Email: gazi.kabas@uzh.ch

1 Introduction

Unemployment insurance (UI) policies are common in both advanced and emerging market economies with a wide range of (sometimes unintended) consequences. UI policies' primary intended effect is to smooth household consumption during unemployment spells.[1] At the same time, they can stabilize macroeconomic fluctuations as they redistribute income to the households in need.[2] On the unintended side, however, higher UI generosity may reduce employment by lowering both the job search intensity of the unemployed as well as firms' job creation.[3] In this paper, we uncover a novel mechanism with several unintended consequences and contribute to the earlier discussion by showing that UI policies might distort the economy through the banking sector as well.

We characterize the mechanism and document its effects in three steps. First, we use county- and branch-level deposit data and a border discontinuity design to causally show that more generous UI benefits lower bank deposits. Second, to evaluate the impact of this reduction in deposits on businesses, we use county-bank-level small business lending data and a within-county comparison to show that a UI-induced decline in deposits lowers bank credit supply to small businesses. Third, we show that the resulting lower credit, in turn, has real effects. In particular, we find that the counties that are served by banks with higher "UI exposure"[4] experience a higher unemployment rate and lower wage growth.

UI policies might lower bank deposits via three main channels. First, to the extent that generous UI benefits lower individual income risk, households' precautionary saving motive weakens. Therefore, generous UI policies may lower bank deposits, households' major saving

[1] Gruber (1997); Ganong and Noel (2019)

[2] See Hsu et al. (2018); Di Maggio and Kermani (2017); McKay and Reis (2016), and U.S. Department of Labor's Unemployment Insurance Directors' Guide

[3] Chodorow-Reich et al. (2018); Hagedorn et al. (2015, 2018)

[4] The phrase "higher UI exposure" refers to banks that raise deposits in states with more generous UI policies.

instrument. Second, generous UI policies might mechanically reduce firm deposits because firms are the ones that end up financing more generous UI benefits by paying more taxes. Third, UI policies could lower bank deposit demand. This might be, for example, because of banks' reduced safe funding demand (i.e., deposits) due to lower household credit risk. With careful identification strategies and robustness checks, we show that households' reduced precautionary saving incentive in response to higher UI benefits is the main driver.

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. UI policies distort bank funding by shifting it away from deposits—the largest and most stable source of funding for banks. The resulting decrease in deposits makes counties suffer from lower access to bank credit and experience worse labor market outcomes.

Second, the mechanism uncovered in this paper suggests an externality that may create further inefficiencies akin to the well-known "paradox of thrift." This externality can be described as follows. In the U.S., each state can choose its own UI generosity. Therefore, a state would prefer to have more generous UI benefits if it considers that 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 would lower total savings and deposits. However, on aggregate, if all states increase their UI benefits, total savings in the country would decline, resulting in lower bank deposits and firm credit. Still, states with more generous UI benefits might look relatively better, as is found in several recent papers (Hsu et al., 2018; Di Maggio and Kermani, 2017) that use cross-sectional identification strategies, yet aggregate welfare could be lower. In our main analysis, we use annual county-level deposit data from Summary of Deposits (SOD) and investigate how changes in UI benefits affect bank deposits. The main identification challenge is that contemporaneous changes in economic conditions that are correlated with UI might bias the results. In particular, in a scenario in which we fail to control relevant economic conditions, we may falsely attribute the changes in deposits to the changes in UI benefits.

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. (2015)). Throughout the paper, we refer to two such counties as a *county-pair* (or simply as a *pair*), and the approach of comparing the deposits of these two counties as *within-county-pair estimation* (or simply as *within-pair estimation*). Since the level of UI benefits is determined at the state level, these neighboring counties 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 their economic conditions. The key identifying assumption in this within-pair estimation 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.

The empirical results show that bank deposits decline substantially when UI generosity rises. In response to an interquartile range increase in state UI benefits, county total deposits decline by 2.3 percent. 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.

Several endogeneity concerns remain about our key identifying assumption. First and most important, state-level economic shocks might affect the level of UI benefits and, at the same time, the level of county deposits. This is not an endogeneity concern in our empirical setting if these shocks affect the other county in the county-pair symmetrically. We show that this is indeed the case. In particular, we use our main border county sample and include state income, state GDP, and state unemployment rate in the regressions as proxies for state economic conditions. Adding these state-level proxies has no significant effect on the coefficient of state UI benefits, and more important, the coefficients of the state-level proxies are insignificant. The latter indicates that state-level economic conditions affect the two counties in the pair symmetrically, and thus their net effect on deposits in the county-pair comparison is zero. Although these results are consistent with our identifying assumption, if the state-level variables that we use in the regressions are irrelevant, then the test has no power. To justify the use of these state-level proxies, we construct a randomly scrambled sample. Instead of matching two neighboring border counties located in different states, we randomly match two non-neighboring counties located in different states. When we estimate our main model with this scrambled sample, we see that state income and state GDP, which are expected to affect deposits positively, have positive and significant coefficients, and that state unemployment, which is expected to affect deposits negatively, has a negative and significant coefficient. These results ensure that the test we provide for the main endogeneity concern has power.

The second concern is that there may be a potentially high degree of heterogeneity in the characteristics of counties in a county pair. This may make the counties in the pair react to state-level shocks asymmetrically. Similarly, one can argue that the counties that are located in the same state might be highly correlated with each other because they are subject to the same set of rules and regulations. If this is the case, the economic conditions in a state are more relevant to a same-state border county than they are to an across-state border county. To address these concerns, we first compare the major economic characteristics of counties and show that border counties are more similar to each other than they are to the rest of the counties in their own states. Next, we run our benchmark regressions for a subset of border counties. In particular, we confine our sample to the county pairs in which counties (i) are geographically closer (less than or equal to 25 miles), (ii) have a similar industrial composition, (iii) have a similar level of local banking competition, (iv) are in the same core-based statistical area, and (v) have a low correlation with their own states. Our results are robust to all of these refinements.

Why do generous UI benefits lower deposits? One possibility is that banks reduce their deposit demand. Banks adjust the composition of their liability side based on their asset side or vice versa (Berlin and Mester, 1999; Drechsler et al., 2017a). Therefore, in response to a decline in the riskiness of their asset side due to lower household credit risk induced by generous UI policies (Hsu et al., 2018), banks' need for safe funding (i.e., deposits) may decrease. Two sets of evidence, however, suggest that the results are not driven by bank deposit demand. First, we compare 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 is that a bank's deposit demand is determined at the bank level, not at the branch level (Gilje et al., 2016; Drechsler et al., 2017b). By comparing the deposits across the same bank's branches, we control for bank deposit demand. Therefore, any effect that we find should be due to either household or firm deposit supply (or both). Indeed, we find that higher UI benefits lower the branch-level deposits within the same bank, which shows the importance of the deposit supply channel. Next, we analyze the effect of UI changes on the deposit rate. If the results are driven by bank deposit demand, then the deposit rate and deposit amount would move in the same direction; on the other hand, if the results are supply driven, they would move in the opposite direction. We show that bank deposit rates rise as UI increases, which supports the deposit supply mechanism. Taken together, both sets of results suggest that the bank deposit demand channel is not the driver of our results.

The other possibility is that more generous UI benefits might reduce deposit supply, from either the firm or household side. First, we provide evidence that firm deposits are not driving our results either. UI benefits could lower deposits through firm behavior, since U.S. states finance their UI payments to households via taxes on firms. Therefore, firms are the ones that end up financing more generous UI benefits by paying more taxes, which may lower their deposit holdings. As the SOD data do not provide the composition of deposit holdings at bank branches, we cannot directly isolate the impact of UI policies on the deposit holdings of households from their impact on those of firms. Instead, we perform two exercises to control for the firm-deposit channel. First, we explicitly include firms' UI tax contributions to state UI funds in our regressions. We find that the coefficient of firms' UI tax contributions is negative but insignificant. More important, the coefficient of UI benefits stays unchanged. Next, we exclude large bank branches from our sample. These are the branches that firms are more likely to work with (Homanen, 2018). Our benchmark results do not change. These two exercises suggest that the impact of UI benefits on deposits is not coming from firms' deposits.

To provide additional support for the household saving mechanism, we exploit the heterogeneity of counties in their exposure to unemployment risk. If the precautionary motive is effective, then we should see stronger results for the subset of counties in which workers face higher unemployment risk as the changes in UI benefits should be more relevant for such counties. As a proxy for unemployment risk, we use extended mass layoff statistics from the U.S. Bureau of Labor Statistics (BLS)[5] and calculate the county-level layoff ratio as the ratio of workers who experience extended mass layoffs to total county employment. We find that the effect of UI benefits on deposits is stronger for counties with a high layoff ratio (i.e., above its median value), consistent with our prediction.

One question to ask for the validity of the household saving mechanism is whether households have enough deposit holdings at banks to drive our results. We use Panel Study of Income Dynamics (PSID) and Survey of Consumer Finances data and show that

[5]BLS "Mass Layoff Statistics" are from https://www.bls.gov/mls/.

households, on average, hold close to USD 28,000 deposits. However, a more relevant statistic is the deposit holdings of households that face unemployment risk, since households with little unemployment risk would not react to changes in UI benefits. If households with high unemployment risk do not hold any deposits, then we would not see any effects of changes in UI benefits on deposits. That said, we show that households that face unemployment spells, which we consider as a measure of higher unemployment risk, keep holding more than USD 15,000 in deposits. The size of the coefficient that we document also implies an estimate consistent with the early literature on UI policies and household savings, supporting the interpretation of our findings as a household saving mechanism. A back-of-the-envelope calculation indicates that 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.

Several additional analyses and robustness checks lend support to our interpretation of the results. First, the results do not change when we control for other state-level social welfare policies that might be correlated with state UI policies. Second, we do not observe that households switch from holding deposits to holding riskier assets, such as bonds and stocks. Finally, by using Google Trends data, we show that households increase their searches for "Unemployment Benefits" as UI benefits change, which suggests that households are aware of the changes in UI policies.

To evaluate the impact of the reduction in deposits on the economy, we first test whether banks that raise deposits in UI-generous states reduce their 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. In particular, we take the weighted average of the UI benefits of states where a bank raises deposits 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 to as bank UI exposure throughout the paper.

The common identification challenge in uncovering the effect on loan supply is to keep loan demand constant. If 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. To control for loan demand, we follow Khwaja and Mian (2008) and implement a within-county estimation strategy using annual county-bank-level small business data from the Community Reinvestment Act (CRA). In particular, we use county-year fixed effects and compare the loan amounts to the same county in the same year by banks with different levels of UI exposure. This within-county estimation holds county loan demand fixed and hence enables us to uncover the effect of bank UI exposure on bank loan supply.

We find that banks that collect deposits in states with generous UI benefits originate less new lending compared to other banks. The effect is economically significant, with an 8.7 percent decrease in new lending in response to an interquartile range increase in bank UI exposure. 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 are expected to experience more reduction in their deposits in response to an increase in UI benefits, and (ii) banks that have a lower equity ratio as, due to agency problems, these banks might have more difficulty in replacing the lost deposits with other sources of funding. These findings further support our causal interpretation.

Finally, to understand whether a UI-induced decrease in small business lending has an impact on local economic activity, we investigate how a county's exposure to UI through its banking sector is related to the county's labor market outcomes. In particular, we consider that lower credit might affect firms' labor demand in two ways. First, firms might use less labor, which may cause an increase in unemployment. Second, firms might lower their wage offers. Since the mechanism builds on bank lending, we expect the results to be particularly strong and significant for the counties that feature a large dependence on external finance (DEF).[6] To test these predictions, we first compute the UI exposure of counties through their lenders. Specifically, we calculate the weighted average of the UI exposure of banks that serve the county in small business lending. We include state-year fixed effects to control for the direct effects of UI benefits on labor markets. With these controls, we compare the counties that face the same level of state UI benefits but have different levels of UI exposure through their lenders. Our results show that when a county's level of UI exposure increases by an interquartile range, its unemployment rate increases by 0.3 percent and wage growth decreases by 0.5 percentage points. Consistent with our prediction, we find that the effects are larger and significant for counties with high DEF while insignificant for counties with low DEF.

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 the results on small business lending, Section 6 presents the results on county-level labor market outcomes, and Section 7 concludes.

2 Related literature

Our paper contributes to the recent literature that studies the stabilizing role of UI policies through their impact on household financial conditions. Hsu et al. (2018) show that UI benefits prevent the unemployed from defaulting on their mortgage and hence insulate

[6] Rajan and Zingales (1996)

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. 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, it is 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 likely to support firms through commercial lending (Ivashina and Scharfstein, 2010; Cornett et al., 2011).

We also contribute to the literature that studies the distortionary effects of UI benefits on the labor market. 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 et al., 2018; Hagedorn et al., 2015, 2018). Our paper provides an additional mechanism that may explain the slow recovery of the U.S. labor market. Our results imply that higher UI benefits during the crisis might have reduced firms' access to bank credit, which in turn hampered their recovery.

Our paper is related to the literature that studies the effect of income risk on household precautionary savings. Engen and Gruber (2001); Carroll and Kimball (2008); Fuchs-Schündeln and Schündeln (2005); Mody et al. (2012); Gourinchas and Parker (2002); Cagetti (2003) find significant effects of the precautionary motive on household savings. 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 (households' main saving instrument) and by analyzing its effect on bank lending and labor market outcomes.

Our paper is also related to the role of deposits in the banking industry and internal capital markets within the banks. The literature offers evidence that both bank fundamentals and panics may lead to deposit outflows (Iyer and Puri, 2012; Iyer et al., 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 generosity of UI benefits. However, consistent with the findings in the literature on the importance of deposits for bank funding, the decline in deposits still leads to a reduction in bank loan supply to non-financial firms. The literature also documents that economic shocks can be transmitted through banks' internal capital markets (Gilje et al., 2016; Cortés and Strahan, 2017; Doerr et al., 2020). Our findings show that the impact of UI in one state is channeled to other states via the banking system.

3 Data and Institutional Background

The analyses in this paper rely on numerous data sources that cover the period from 1995 to 2010. For ease of exposition, we partition the descriptions of these data sources and institutional details into three subsections following the structure of the paper.

3.1 Deposit Analysis

In this subsection, we detail the data sources and variables that play the central role in our deposit analysis. We start with describing the unemployment insurance (UI) policies in the U.S.[7] UI policies provide income to eligible workers who involuntarily become unemployed. While the basic framework and features of the UI system in the U.S. are set by federal law,

[7]The U.S. Department of Labor issues "Significant Provisions of State UI Laws," which provides information on UI policies implemented after 1938. We use the data obtained and provided by Hsu et al. (2018) and Chetty (2008).

most of the details are left to the individual states. The states impose two main limits on UI benefit payments that an unemployed individual can receive. The first restriction is the "benefit duration," which limits the number of weeks that the unemployed individual can receive benefits. The other limit on UI benefits is the "dollar cap." Each state annually sets a limit on the weekly benefits so that benefit payments cannot exceed a certain dollar amount. The unemployed individual obtains the weekly benefits determined by the dollar cap for the benefit duration. In our analyses throughout the paper, we follow the literature and use the product of dollar cap and benefit duration as the main independent variable and refer to it as the "state UI benefit." This variable represents the maximum total UI payment an unemployed individual can receive during his unemployment spell and reflects the UI generosity of the state where the individual resides.

Each state in the U.S. uses its own UI trust funds to make benefit payments to unemployed individuals. The funds are financed mainly by raising taxes on firms. States use an experience-based tax system, meaning that firms with more unemployment insurance claims in the past pay more taxes. Depending on the local economic activity and unemployment rates, states may exhaust their UI trust funds, in which case they may request additional financial support from the federal government.

During times of high unemployment (e.g., the global financial crisis (GFC) of 2007-2008, the COVID-19 crisis), the federal government might extend the duration and increase the amount of benefit payments. For instance, 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 because the benefit extensions are triggered by the economic conditions (i.e., unemployment rate) of states. 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. As a result, the results presented in the paper speak only to the effects of regular UI benefit payments on the economy.

The other main data set that we use is from the Summary of Deposits (SOD) survey issued by the Federal Deposit Insurance Corporation (FDIC). This data set includes the amount of deposits of U.S. bank branches at an annual frequency as well as branch characteristics such as location and parent bank.

In our deposit analysis, we investigate how the changes in state unemployment insurance affect bank deposit holdings. This deposit analysis is based on comparing two border counties located at state borders. Therefore, we aggregate the SOD's branch-level deposits into the county level and supplement the data with annual state UI benefit payments, county-level income, and the unemployment rate.[8,9]

Panel A of Table 1 presents the summary statistics at the county level for the sample of border counties that we use in our deposit analysis. The average weekly UI benefit payment in a county is 330 USD for a period of 26 weeks. The product of the two is our key independent variable (i.e., state UI benefit), and its average is 8,510 USD. The variable shows significant variation that mainly comes from weekly payments as the duration of payments is almost uniform across states and over time. The states also show variation in their frequency of changing their benefit payments (Figure 1). While the states in the West and Midwest change their UI benefits more frequently, the ones in the Southeast region make less frequent changes. The median county in the sample has 311 million USD deposits and 625 million USD total income with an unemployment rate of 5.23%.

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

[9] We do the same analysis at the branch level without aggregating the deposit data at the county level, in which case we compare two branches of the same bank located in different counties at state borders. For a more detailed description and discussion of the empirical design for the deposit analysis, see Section 4.1

3.2 Lending Analysis

In our lending analysis, we study how the reduction in deposits triggered by generous UI policies affects the small business lending of banks. The analysis is based on the Community Reinvestment Act's (CRA) annual bank-county-level small business loan data.[10] We use the total amount of new loans originated at small businesses with gross annual revenues of less than 1 million USD. To gauge the UI-induced decline in bank lending through the deposit channel, we need to measure the exposure of banks to UI through their deposit collection activity. To do this, we take the weighted average of the UI level of the states where a bank raises deposits using the deposits of the bank in those states as weights. We refer to this variable as "bank UI exposure" throughout the paper. This variable reflects the average level of UI benefits the bank faces through deposit markets and is different from the level of UI benefits of the state where the bank's lending activity takes place. After supplementing the small business lending of banks with their UI exposure, we merge the data with bank balance sheet information from Call Reports to control for lender characteristics that may affect loan outcomes. In the Call Reports data, commercial banks report their top-holder bank holding company, enabling us to aggregate bank-level variables into the bank holding company level.

Panel B of Table 1 reports the summary statistics for the variables that we use in our lending analysis. The average (median) amount of new small business lending in a given county is 376,000 (58,000) USD originated by a bank with an asset size of 4.8 (0.7) billion USD. The asset share of deposits for an average bank is 80 percent. This share indicates a high dependence on deposit funding for the sample banks, implying that a decrease in deposits has the potential to affect their lending behavior. The average value of bank UI exposure (9,050 USD), our main independent variable in the lending analysis, is slightly

[10] Small business loan data from https://www.ffiec.gov/cra/craproducts.htm

higher than that of state UI benefits (8,510 USD). This means that the deposit collection activity of sample banks is higher in states with more generous UI benefits, which is not surprising given that states with more generous UI benefits are larger.

3.3 Real Effects Analysis

Finally, to understand whether a UI-induced decrease in small business lending has an impact on local economic activity, we investigate how a county's exposure to UI through its banking sector is related to the county's labor market outcomes. As labor market outcomes, we use the county-level unemployment rate and average wage growth rates. The main independent variable of this exercise, the county's exposure to UI through its banking system, is similar to bank UI exposure. Specifically, county UI exposure is a weighted average of the UI exposures of banks that serve the county in small business lending.[11] The average value of this variable is 9,270 USD, which is slightly larger than bank UI exposure (Panel C of Table 1). We complement these data with the county's DEF à la Rajan and Zingales (1996). Namely, we define DEF as capital expenditures minus cash flow from operations divided by capital expenditures. We use Compustat firms to calculate each industry's external finance dependence at two-digit SIC codes and aggregate this measure up to the county level using the employment shares of industries in the county from the County Business Patterns data.

4 Deposit analysis

A large number of countries implement unemployment insurance (UI) policies to reduce individuals' income risk and to moderate fluctuations in the economy. The distortionary effects of UI policies on labor market outcomes are well documented in the literature.

[11]The market share of banks in county small business lending is used as weights.

However, the effect of these policies on the economy through the banking sector has not yet been studied.

In this section, we use county-level total deposits and state-level unemployment insurance benefits and test whether an increase in UI benefits reduces the amount of deposits held at banks. However, the results of a model in which we simply regress county deposits on state UI benefits would be biased by endogeneity. State UI generosity may depend on state political factors (e.g., election concerns, party preferences), state economic conditions (e.g., labor market conditions, state budget surplus/deficit), and the interaction between the two (Blaustein et al., 1993). Table 2 shows the association between state UI benefits and several proxies for local economic conditions. More specifically, the level of state UI benefits tends to increase during times of high economic growth and low unemployment, suggesting that state governments face fewer budget constraints during such periods. Important to our empirical framework, the economic conditions in a state are by construction correlated with the economic activity in its counties, and hence potentially with county total deposits. Therefore, to the extent that we omit relevant state economic conditions in our regressions, the coefficient of state UI benefits would 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 that are located in that state. The estimated coefficient would erroneously attribute the effect of this economic shock on county deposits to state UI benefits. To establish the causality between state UI benefits and county deposits, we therefore need to control for state economic conditions.

4.1 Identification strategy and main results

We address this identification challenge with a border county design by which we exploit the discontinuous changes 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. For instance, Figure 2a 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*), and the approach of comparing the deposits of these two counties as *within-county-pair estimation* (or simply as *within-pair estimation*). Figure 2b provides a slightly different case of county-pair formation. Northampton County (NC) (in light red) shares the state border with three counties in VA: Southampton, Greensville, and Brunswick. This generates three different county pairs for Northampton in our empirical analysis: Northampton-Southampton, Northampton-Greensville, and Northampton-Brunswick.[12] In our sample, the average number of county pairs a border county belongs to is 2.06, bringing the total number of observations in our deposit analysis to 36,596 out of 17,802 unique county-year observations (Table 1).[13] Figure 3 displays the location of all border counties used in our county-pair comparison analysis.

Why is this within-county-pair estimation useful for our purposes? The two counties within a county pair share the same geography and climate, have access to the same transportation routes, and, more important, are open to similar spillover effects of economic changes. These characteristics suggest that a state-level economic shock is expected to affect the two counties within a county pair symmetrically, since the economic conditions are

[12]For ease of discussion, throughout the paper, we discuss and explain our empirical strategy, identification challenges, and the ways we address them by using the type of county-pair formation shown in Figure 2a; that is, a county at a state border has only one neighbor county across the border. However, our empirical strategy uses both types of county-pair formations.

[13]Using a county-year observation more than once creates a mechanical correlation between county pairs. We provide a detailed discussion of how we address this correlation in our empirical strategy after we introduce our regression specification in this section.

continuous in the sense that state borders do not affect the movement of the economic shocks (Dube et al., 2010; Heider and Ljungqvist, 2015; Hagedorn et al., 2018; Brown and Matsa, 2019). Therefore, comparing the two counties within a county pair controls for economic shocks that are expected to affect both state UI benefits and county deposit levels. The two counties in a county pair, on the other hand, are subject to different levels of UI benefits since the generosity of UI policies is determined by state governments. This discontinuous variation in UI policies allows us to measure the effect of UI benefits on deposits.

One point is worth noting. The necessary identifying assumption for the validity of 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 4.2, we provide robustness checks and tests to support this identifying assumption.

We estimate the following regression model for our within-county-pair estimation:

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

where the dependent variable is the log change in the total deposits of county c from year $y-1$ to y, $\Delta log(UI_{s(c),y})$ is the log change in the UI benefits of the state where county c is located[14], $\delta_{p(c),y}$ 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 the county unemployment rate up to its third-degree polynomial.

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 with each other. Under our identifying

[14]The level of UI benefits that applies to year y is usually announced by the state government during the summer of year $y-1$. This means that we estimate the impact of UI changes that are announced in year $y-1$ on the amount of deposits in year y.

assumption that state-level economic shocks affect the two counties in a pair symmetrically, using pair-year fixed effects cancels out the effect of state shocks on the deposits of the two counties within the pair. This allows us to identify the effect of state UI benefits on deposits. County fixed effects control for the unobserved time-invariant differences. Given the association of UI benefits with economic growth and the unemployment rate (Table 2), we further include county income and unemployment rates to absorb time-varying differences 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 may neighbor multiple counties on the other side of the border,[15] the border county may be placed in more than one county pair in our empirical setting, which generates a mechanical correlation across county pairs. To account for this correlation, we follow Dube et al. (2010), and double-cluster standard errors at the state and border segment level.[16]

Table 3 presents the main results for our deposit analysis. The analysis in each column is at the county level and uses only the counties located at state borders. Each specification 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 reports a negative and significant coefficient for state UI benefits. We add additional controls to the regression in the remaining columns. To control time-invariant differences between the two counties in the pair, column (2) uses county fixed effects. However, the total amount of county deposits is likely to be a function of time-varying county economic conditions; hence, we control for the county-level

[15] See Figure 2b for an example.

[16] "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))

income in column (3) as a proxy for county economic conditions.[17] We further control for county labor market conditions that may be correlated with state-level economic conditions, and hence with state UI benefits, by using the 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. The economic meaning of the coefficient in the last column is that total county deposits decrease by 2.3 percent in response to an interquartile range increase in the level of state UI benefits.[18]

4.2 Endogeneity concerns

In this section, we discuss potential concerns regarding the use of border county design as an identification strategy and ways to mitigate these concerns. State-level economic shocks 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 in our empirical setting if these shocks affect the other county in the county pair symmetrically. This is because making a within-county-pair comparison cancels out the impact of state shocks on county deposits. Therefore, our main identifying assumption is that state-level economic shocks that are correlated with UI changes must affect the two counties in a county pair symmetrically. If this symmetry assumption does not hold, the coefficient of UI benefits would also reflect state economic conditions that are not controlled for in the regressions. To support the use of border county design, we provide two sets of evidence.

First, we show direct evidence for the validity of the identifying assumption. Specifically, we test whether state-level economic conditions affect the two counties in a pair symmetrically. We do so by including *relevant* proxies for state-level economic conditions in our main regression. If the two counties in the pair are affected symmetrically, then, in a

[17] We also use county-level wage income as a control instead of total income and obtain similar results.
[18] ((\$10.04 – \$6.66) / \$8.14) * 0.056 = 2.3%

regression where there are pair×year fixed effects, we should have a zero coefficient for the proxies of 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 in the regressions as proxies for state economic conditions, respectively. Our results show that adding the state-level proxies has no significant effect on the coefficient of state UI benefits, which mitigates the concern that state-level economic conditions may drive our results. More important, in each specification, the coefficients of the state-level proxies are insignificant. This indicates that state-level economic conditions affect the two counties in the pair symmetrically, and thus their net effect on deposits 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 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, we therefore construct a random scrambled sample; that is, 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 an NC border county and a VA border county that share a common border, we match the NC border county with a border county in California (CA). In this constructed border county sample, there should be a 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 should 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 this. 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.[19]

Our second set of tests are more indirect in nature in the sense that they mitigate concerns regarding the use of border county design. One can argue that although the two counties in a county pair are neighbors and share the same geography, climate, and transportation routes, there is potentially some degree of heterogeneity in terms of their characteristics (e.g., income per capita, industrial composition, banking competition, education, age). These heterogeneities may make these counties react to state-level shocks asymmetrically. Therefore, this line of reasoning suggests that the border county design is a better laboratory if the two counties within a pair are more similar to each other. Similarly, one can argue that the counties that are located in the same state are highly correlated with each other because they are subject to the same set of rules and regulations. If this is the case, the economic conditions in a state are more relevant to a same-state border county than they are to an across-state border county. This exacerbates the main endogeneity concern that state-level economic shocks may affect both UI benefits and county economic conditions. Therefore, the border county design is a better fit for our purpose if a border county is less similar to the rest of the counties in the same state. In the remaining of this section, we analyze the similarity of a border county to the neighboring county across the border and to the rest of the counties in its own state, and restrict our sample based on these similarities as a robustness check.

We start by displaying the results of two comparisons in Table 5. In the first three columns, we compare the characteristics of two border counties within a county pair. Although they differ from each other in terms of population (28%), their characteristics

[19]Another observation in columns (4) through (6) is that the coefficient of UI benefits is insignificant. This implies that when we do not use border county design (i.e., when the economic conditions are not properly controlled for), our coefficient of interest is biased upward. Thus, the remaining correlation, if any, between UI benefits and the error term due to economic conditions in the main specification should create bias against our results.

remain close to each other. For instance, the difference between their average income per capita is 4%, and they are similar to each other in terms of their demographic characteristics (i.e., rurality, education, race and age composition). In the next three columns, we compare the characteristics of a border county with the rest of the counties in the same state. 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 their own state. Almost all variables have negative values. This mitigates the concern that state-level economic conditions in a state affect the same-state border county but not the across-state border county.

In line with the comparison we discussed, we test whether our results survive when we restrict the sample. In Table 6, we restrict the sample based on the similarity of the two counties within a county pair. Each column 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 a pair is used as a criterion for county similarity. We use the county pairs only if the distance is less than or equal to 25 miles (Figure 4a).[20] The intuition is that if the two counties have close proximity to each other, they are more likely to be similar to each other and hence more likely to respond to state economic shocks symmetrically. Columns (2) and (3) classify the two counties in the pair based on their industrial composition[21] and local banking competition[22], respectively, and include in our sample only the most similar counties (Figure 4b and Figure 4c, respectively). The counties with similar industrial composition or banking competition are more likely to react symmetrically to an economic shock. In column

[20] The first tercile value of the distance distribution

[21] 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 BEA. Next, we construct the Euclidian distance between the two counties in a pair. The low value of Euclidian distance (i.e., county pairs with an industry distance of less than the first tercile value) indicates more similarity in industrial composition.

[22] We calculate the deposit market Herfindahl-Hirschman Index (HHI) of the counties

(4), we use the core-based statistical area (CBSA) definition of the Office of Management and Budget; that is, the counties are in the same statistical area if they are similar and integrated with each other socioeconomically. In this column, we include the county pairs only if the counties in the pair are also in the same statistical area (Figure 4d). Therefore, the economic conditions in these two counties are arguably similar to each other by construction. The coefficients across four columns are all negative and significant despite the notable decline in sample size.

Finally, in Table 7, we restrict our sample by excluding the border counties that are highly correlated 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. Second, we exclude counties from the sample if they are large relative to their states (counties with 2 percent or more of the state employment level). 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. The results of these two exercises confirm a negative and significant effect.

4.3 Underlying mechanism

Why do we observe lower amounts of deposits when UI benefits are more generous? The decline in deposits might be driven by banks' lower deposit demand. Alternatively, it might be supply driven; that is, firms or households (or both) might reduce their deposit holdings at banks. In this section, we study the underlying mechanism for the decrease in deposits and conclude that our findings are more consistent with a decrease in households' precautionary savings.

In our within-county-pair comparison, we aggregate branch-level deposits into the county level and compare the total deposits of the two border counties within a county pair. We find that generous UI policies reduce county deposits. One potential explanation for the decline in deposits is the lower deposit demand of banks in the county. Generous UI policies may reduce the credit risk of households located in the county (Hsu et al., 2018) and hence the credit risk exposure level of banks that originate loans in the county. This may in turn reduce banks' need or incentive to raise safe and stable funding (i.e., deposit funding) (Berlin and Mester, 1999; Drechsler et al., 2017a).

We rule out this demand-driven explanation by making a branch-level analysis in which we use total branch-level deposits as our dependent variable. In this analysis, instead of using pair×year fixed effects, we use pair×bank×year fixed effects, which means we compare 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. This within-bank estimation allows us to control for bank deposit demand with the assumption 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 to exploit 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 (Gilje et al., 2016; Drechsler et al., 2017b). Therefore, the bank demand for deposits stays constant across its branches, which allows us to measure the impact of UI benefits on deposit supply by households or firms (or both). To make this within-bank estimation, we use only the sample of banks with branches in both counties in a pair and exclude all others since the coefficient is not identified for single-county banks. Table 8 shows the results. In column (1), we have a negative coefficient, which confirms our previous county-level deposit results. In column (2), we further refine the specification by including county×bank fixed effects to absorb time-invariant branch-level brand effects. In the remaining columns, we use additional county-level time-varying variables. The results remain the same.

The effect of UI changes on deposit rates further rules out the demand-driven mechanism. If the results are demand driven, then the price (deposit rate) and quantity (deposit amount) should move in same direction; on the other hand, if the results are supply driven, they should move in the opposite direction. We can investigate this in a bank-level analysis by using Call Report data that give us both bank-level total deposits and the deposit rate.[23] We supplement this data set with a bank-level UI exposure variable that captures the average level of UI benefits a bank faces through its deposit collection activity.[24] In a fashion similar to that when we compare two neighbor counties across a state border, we compare the deposit amount and deposit rate of two comparable banks with different levels of UI exposure by employing propensity score matching.[25] Similar to border counties forming a county pair, each treated bank and its matched control bank constitute a bank pair in the bank-level analysis. The first four columns of Table 9 display the results for the deposit amount, with different controls. The coefficient of bank UI exposure is negative and statistically significant and quite similar to what we find in our county-level analysis. The last four columns report the results for the deposit rate. Consistent with the supply-driven story, the coefficient of bank UI exposure is positive and significant, indicating that banks pay more interest on their deposits when their UI exposure increases.

Having established that the decline in deposits is supply driven, we now turn to the question of whether firms or households are responsible for this decline. On the one hand, generous state UI benefits may reduce the amount of deposits firms hold at banks because firms are the ones who end up financing more generous UI benefits by paying more taxes. On the other hand, generous UI policies lower individuals' income risk and moderate economic fluctuations, which may reduce household precautionary savings and hence bank deposits–

[23]We obtain the deposit rate from Call Reports by dividing the end-of-year total deposit interest expenses to lagged total deposits.

[24]See Section 3.2 for the calculation of the bank UI exposure variable.

[25]See Table A1 for the balance table of the matching exercise.

households' main saving instrument. As the SOD data do not provide the composition of deposit holdings at bank branches, we cannot directly isolate the impact of UI policies on the deposit holdings of households from their impact on those of firms. However, we rule out the firm-driven explanation by performing several exercises. In column (1) of Table 10, we exclude the bank branches that firms are more likely to work with. Specifically, we exclude the largest branches (i.e., top 1 percent) from the sample and calculate county total deposits by aggregating the deposits of the remaining branches because firms are expected to hold their deposits in large branches (Homanen, 2018).[26] Our results remain unchanged. In the remaining columns, we explicitly control for firms' UI tax contributions to state UI funds and the wage base these contributions are based on. The coefficient of firms' UI tax contributions is negative as expected but insignificant. More important, the coefficient of UI benefits stays unchanged. Overall, the results in Table 10 suggest that the decline in deposits is not driven by firms but rather by households.

To further support the household lower precautionary motive mechanism, we exploit the heterogeneity of counties in their exposure to unemployment risk. If the precautionary motive is the underlying mechanism, then we should see stronger results for the subset of counties in which the changes in UI benefits are more relevant. For instance, workers in the manufacturing industry experience more extended mass layoffs and hence have a higher level of unemployment risk.[27] Therefore, changes in the level of UI benefits should have a stronger impact on the savings 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. Based on this reasoning, we classify counties based on their layoff ratios, that is, the ratio of workers who experience extended mass layoffs to total county employment. Table 11 shows

[26]This may be because large branches have more officers and hence are able to provide better services to firms, which may encourage firms to work with these branches. Alternatively, firms may make some branches large by depositing their money into those branches. We do not take a stance on the exact mechanism.

[27]BLS "Mass Layoff Statistics" from https://www.bls.gov/mls/

that the effect of UI benefits on deposits is negative and highly significant for counties with high layoff ratios, whereas it is not significant for counties with low layoff ratios, consistent with our prediction.

One question to ask for the validity of the household saving mechanism is whether households with unemployment risk have enough deposit holdings at banks. It is possible that deposit holdings can be large for individuals with a low level of unemployment risk and low or non-existent for the ones with a high level of unemployment risk. Reassuringly, the PSID data set suggests that this is not the case (Table 12). Households with past unemployment experience hold, on average, significant deposits (more than USD 15,000).[28, 29]

The size of the coefficient that we document also implies an estimate consistent with the early literature on UI policies and household savings, supporting the interpretation of our findings as a household saving mechanism. A back-of-the-envelope calculation indicates that 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. This estimate suggests an economically significant role for UI policies in household savings, which qualitatively confirms the findings of the earlier literature. In particular, our estimates are very close to the findings of Engen and Gruber (2001) but smaller than the findings of Bird and Hagstrom (1999). One concern may be that our results are not perfectly comparable to the earlier papers as the saving measures are different: while we explore the effects of UI benefits on deposits, earlier papers analyzed their effects on a broader measure of savings. That said, bank deposits is the most common savings instrument for most of the households, and for most of them it is the only one. According to the Survey of Consumer Finances

[28]Note that this number is a conservative estimate for deposit holdings of households with unemployment risk since some of these households may not have actual unemployment experience.

[29]As is the case with other assets, deposit holdings are skewed, which makes the medians smaller than the means. For the whole sample, the deposit holdings median is USD 3,556. For the households with a high level of unemployment risk, the median is USD 1,052.

(SCF), while more than 90 percent of families have transaction accounts with a median value of 4,000 USD, only 20 percent of families directly hold stocks or bonds or both.[30] Furthermore, stocks and bond holdings are concentrated mainly among the highest-income people.[31]

4.4 Robustness Checks

In this section, we summarize the results of several robustness tests. One concern in our empirical strategy is picking up the effect of other state-level policies. For instance, the generosity of state-level social welfare programs might be correlated with that of unemployment insurance policies. To alleviate such concerns, in Table A2, we control for other state policies. Namely, we include changes in minimum wage, health insurance payments, union coverage, and total non-UI transfers as additional controls. Including these controls either individually or altogether does not change the magnitude and significance of the coefficient of UI generosity.

The SCF data show that the majority of households hold bank deposits as their main financial assets. However, UI may also have an impact on other types of financial assets (i.e., bonds, stocks). On the one hand, an increase in UI benefits may weaken the household precautionary motive and hence lower households' bond and stock holdings. On the other hand, households may want to increase their holdings of these assets as their level of income risk becomes lower. This portfolio adjustment may have important implications for the financing policy of firms. For instance, if UI increases the bond holdings of households, then firms can replace the decrease in bank finance with bond issuance. We perform two exercises to understand whether these mechanisms are at play by using IRS data. The

[30]These values are for 2004.

[31]Moreover, the findings that we report in Table A3 and Table A4 suggest that UI has no significant effect on stock and bond holdings. As a result, we believe that our results capture a big part of the changes in precautionary savings in response to the changes in UI benefits.

IRS's Statistics of Income (SOI) database provides county-level interest and dividend income statistics. Under the assumption that counties in the same pair have similar bond and stock portfolios, differences in incomes generated by these assets imply different levels of these asset holdings.[32] We replicate our main specification by replacing county deposits with interest earnings on bonds and dividend income on stocks. We find no effect of UI on bonds (Table A3) and on stock holdings (Table A4). These findings may be explained either by the two opposing effects discussed above or by the low level of unemployment risk of stock and bond holders.

Finally, a rise in UI generosity would influence the savings of employed people only if they are aware of the changes in the policies. We provide supporting evidence that this is indeed the case. By using Google Trends data, we show that households increase their "Unemployment Benefits" searches as UI benefits change (Table A5). Moreover, the relationship between the Internet search activity and the changes in UI benefits stays significant even when we control for state-level income, GDP state fixed effects, and, more important, unemployment. Overall, these findings suggest that people are aware of the changes in UI benefits.

5 Lending analysis

So far, we have established that generous UI policies reduce bank deposits. In this section, we test whether banks that raise deposits in UI-generous states (i.e., banks with a high level of UI exposure) reduce their commercial lending. Since banks rely heavily on deposits for their funding, and since they cannot perfectly replace deposits with other funding sources, we expect banks to squeeze their loan supply in response to an increase in their level of UI

[32] We calculate the interest income on bonds by subtracting the interest income on deposits from total interest income.

exposure. The main identification challenge in testing this prediction on loan supply is to control for loan demand. If a borrower'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.

To address this identification challenge and to establish the causality between bank UI exposure and commercial lending, we implement a within-county estimation using annual county-bank-level small business lending data from the CRA. In particular, we use county×year fixed effects and compare the loan amounts to the same county in the same year by banks with different levels of UI exposure. Assuming that a county's loan demand is symmetric across different banks, our empirical strategy holds loan demand fixed and hence enables us to uncover the effect of banks' UI exposure on their loan supply (a là Khwaja and Mian 2008).[33]

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

$$log(new\ lending)_{c,b,y} = \beta \Delta log(UI\ Exposure)_{b,y} + \gamma \Delta Bank\ Controls_{b,y-1} + \delta_{c,y} + \alpha_b + \epsilon_{c,b,y} \tag{2}$$

where the dependent variable is the log of the loan amount originated by bank b to county c in year y, $\Delta log(UI\ Exposure)_{b,y}$ is the log change in the UI exposure of bank b, $\delta_{c,y}$ is county×year fixed effects for county c, and α_b are fixed effects for bank b. Across different specifications, we saturate the model with county×bank fixed effects, bank-level controls, and banks' exposure to the economic conditions/policy environment of the counties/states where they raise deposits. We double-cluster standard errors at the bank and county level. From our sample, we exclude a bank-county observation if the bank raises deposits in the county. This means that we study the lending activity of a bank only in counties that do

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

not contribute to the calculation of its UI exposure. This ensures that the bank UI exposure variable is not correlated with the economic conditions of the county where the lending takes place.

Table 13 presents our main results. Each specification in the table includes county×year and bank fixed effects. Column (1) is our baseline specification with no control variables other than the county×year and bank fixed effects and shows a negative and significant coefficient for bank UI exposure. The economic meaning of this coefficient is that an interquartile range increase in bank UI exposure decreases the loan supply by 8.7 percent.[34] One concern with our baseline specification could be endogenous matching between counties and banks. Banks with different levels of exposure might prefer to extend their loan supply to particular counties, and this behavior can create a selection bias in our estimations. To address this concern, in column (2), we include county×bank fixed effects in our model. Remarkably, the coefficient stays the same despite a big increase in R^2, which mitigates the concerns about endogenous borrower-lender matching (Altonji et al., 2005; Oster, 2017). In column (3), we saturate the model with bank control variables that are commonly used in the bank lending literature. In column (4), we also control for the exposure of banks to the economic conditions/policy environment of the counties/states where they raise deposits. The coefficients in these two columns stay unchanged in terms of both their magnitude and statistical significance.

The heterogeneity tests in Table 14 further support our interpretation of the results. First, we use the heterogeneity of banks in their ability to replace the decrease in deposits. In particular, we consider that banks with lower equity ratios are more likely to suffer from agency problems (Holmstrom and Tirole, 1997) and might have more difficulty in substituting

[34]When comparing the magnitude of the decrease in deposits and small business loans, it is important to keep in mind that the deposit variable is a stock variable, whereas the small business lending variable is a flow variable. Furthermore, the share of deposits in bank balance sheets is much higher than that of small business lending.

the decrease in deposits with external wholesale funding. Therefore, we expect that these banks squeeze their lending supply more. In columns (1) and (2), we split the banks into two subsamples based on their equity ratios. In line with our expectation, we find that the banks with low equity ratios decrease their lending more, whereas the effect is insignificant for banks with high equity ratios.

Second, we exploit the implications of the results in Section 4.3, where we find that household behavior is the main driver of the negative relationship between UI and deposits. Given that the amount of deposits an average household holds is expected to be small, changes in UI should have more of an effect on banks that have a greater reliance on small deposits. Indeed, this is the case. In columns (3) and (4), we divide the sample into two subsamples based on the share of small deposits on bank balance sheets. We find that the negative impact of UI exposure on lending is stronger for banks that have a higher share of small deposits.

6 Real Effects

After demonstrating the negative impact of a UI-induced decline in deposits on small business lending, we conclude our analysis by testing whether the mechanism that we have identified so far has any real effects. Specifically, we test whether counties that are served by banks with a high level of UI exposure (i.e., counties with a higher level of UI exposure) face any negative real consequences. As is more common in the UI literature, we focus on two labor market outcomes: the unemployment rate and the change in average wages. Since decreased access to bank credit may constrain firms' labor demand, we expect to find that counties with higher levels of UI exposure experience a higher unemployment rate and lower average wage growth.

In studying the effect of county UI exposure on local labor market outcomes, it is important to control for the effect of the UI policies of the state where the county is located. In other words, we need to distinguish between the effect coming from the county's UI exposure through its banking sector and the effect coming from the state's UI benefits. This is because state UI policies can also alter labor market outcomes directly, for instance, by lowering household job search intensity, firm job creation, or both.[35] We control for the direct effect of UI benefits by including state×year fixed effects. This means that we compare the counties that face the same level of state UI benefits but have different levels of UI exposure through their lenders. Using state×year fixed effects also controls for time-varying state economic shocks.

We estimate the following regression model:

$$y_{c,y} = \beta \Delta log(UI\ Exposure)_{c,y-1} + \kappa\, County\ Controls_{c,y-1} + \delta_c + \lambda_{state,y} + \epsilon_{c,y} \tag{3}$$

where $\Delta log(UI\ Exposure)_{c,y-1}$ is county c's exposure to UI benefits through its lenders, and δ_c and $\lambda_{state,y}$ are county and state×year fixed effects, respectively. We include the county's exposure to bank-level characteristics as control variables.[36] The dependent variables are either the log of the unemployment rate in percentage points or the log change in the average wage. We expect our coefficient of interest, β, to be positive for the unemployment rate and negative for the average wage. We cluster standard errors at the state level.

The first three columns of Table 15 present the results for the unemployment rate. In column (1), we use the all-county sample and find a positive and significant coefficient. In

[35]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 a higher equilibrium wage is called the macro effect. More recently, these effects are also discussed in Hagedorn et al. (2018).

[36]These variables are the county's exposure to bank assets, equity ratio, liquidity ratio, wholesale funding ratio, share of loans in total assets, net income ratio, and interest expense ratio.

columns (2) and (3), we divide our county sample into two subsamples with respect to their DEF. We expect that counties with a higher DEF would be more affected by a UI-induced decline in bank lending. Consistent with our prediction, the coefficient is significant only for counties with a high DEF. The economic meaning of this coefficient is that as the county's UI exposure increases by an interquartile range, the county's unemployment rate increases by 0.3 percent. In the last three columns, we investigate the relationship between the county's average wages and its exposure to UI benefits. Column (4) shows that counties with an increase in UI exposure experience a decline in their average wage growth rate. Consistent with our conjecture, when we split the sample into two subsamples based on their DEF, we find that the result holds only for counties with a high DEF. The economic meaning of the coefficient in the last column is that as the county's UI exposure increases by an interquartile range, its average wage growth declines by 0.5 percentage points.

Overall, the combination of a decline in wages and an increase in the unemployment rate lends support to our argument that counties with a high level of exposure to UI benefits through their banking system experience a decline in labor demand. This mechanism is in line with the bank lending channel of UI benefits that we document in the previous sections.

7 Discussion and Conclusion

UI policies have many benefits. Most important, they smooth household consumption during unemployment spells. However, UI policies also have unintended consequences, particularly in the labor market. In this paper, we uncovered a novel mechanism through which UI policies distort credit markets.

Our study yields three sets of results. First, we use both county- and branch-level data and show that more generous UI benefits reduce bank deposits. Second, we use bank-county-level small business lending data from the CRA and show that banks that raise deposits from states with more generous UI benefits originate less credit to firms. Third, we show that counties that are served by these banks experience a higher unemployment rate and lower wage growth. All of our results indicate both statistically and economically significant effects. Collectively, our findings provide a strong set of evidence that UI benefits distort bank funding and commercial lending.

The effects that we found in this paper are likely to be stronger for European countries. The reason is that our findings rely on U.S. data, where 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-U.S. firms are much more bank dependent than their U.S. counterparts, the real effects of bank UI exposure on firm outcomes may be even stronger.

UI benefits certainly affect employed and unemployed people differently. For example, recent evidence by Hsu et al. (2018) suggests that UI benefits reduce the 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; that is, UI benefits lower the 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 on the unemployed, which forms on average about 5-6 percent of the 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 relative to their counterparts as changes are made in the UI benefits that they face. By construction, this kind of methodology cannot say anything about the effects of the mean UI benefits on the macro economy. For that analysis, 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 ongoing research.

References

Altonji, J. G., T. E. Elder, and C. R. Taber (2005). Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools. *Journal of political economy 113*(1), 151–184.

Amiti, M. and D. E. Weinstein (2018). How much do idiosyncratic bank shocks affect investment? evidence from matched bank-firm loan data. *Journal of Political Economy 126*(2), 525–587.

Berlin, M. and L. J. Mester (1999). Deposits and relationship lending. *The Review of Financial Studies 12*(3), 579–607.

Bird, E. J. and P. A. Hagstrom (1999). The wealth effects of income insurance. *Review of Income and Wealth 45*(3), 339–352.

Blaustein, S. J., W. J. Cohen, and W. Haber (1993). Unemployment insurance in the united states: the first half century.

Brown, J. and D. A. Matsa (2019). Locked in by leverage: Job search during the housing crisis. *Journal of Financial Economics*.

Cagetti, M. (2003). Wealth accumulation over the life cycle and precautionary savings. *Journal of Business & Economic Statistics 21*(3), 339–353.

Calomiris, C. W. and J. R. Mason (1997). Contagion and bank failures during the great depression: The june 1932 chicago banking panic. Technical report.

Calomiris, C. W. and J. R. Mason (2003). Fundamentals, panics, and bank distress during the depression. *American Economic Review 93*(5), 1615–1647.

Carroll, C. D. and M. S. Kimball (2008). *Precautionary saving and precautionary wealth.* Springer.

Chetty, R. (2008). Moral hazard versus liquidity and optimal unemployment insurance. *Journal of political Economy 116*(2), 173–234.

Chodorow-Reich, G., J. Coglianese, and L. Karabarbounis (2018). The macro effects of unemployment benefit extensions: A measurement error approach. *Quarterly Journal of Economics*.

Cornett, M. M., J. J. McNutt, P. E. Strahan, and H. Tehranian (2011). Liquidity risk management and credit supply in the financial crisis. *Journal of financial economics 101*(2), 297–312.

Cortés, K. R. and P. E. Strahan (2017). Tracing out capital flows: How financially integrated banks respond to natural disasters. *Journal of Financial Economics 125*(1), 182–199.

Di Maggio, M. and A. Kermani (2017). Unemployment insurance as an automatic stabilizer: The financial channel.

Diamond, P. A. (1982). Wage determination and efficiency in search equilibrium. *The Review of Economic Studies 49*(2), 217–227.

Doerr, S., G. Kabas, and S. Ongena (2020). Population aging and bank risk-taking.

Drechsler, I., A. Savov, and P. Schnabl (2017a). Banking on deposits: Maturity transformation without interest rate risk.

Drechsler, I., A. Savov, and P. Schnabl (2017b). The deposits channel of monetary policy. *The Quarterly Journal of Economics 132*(4), 1819–1876.

Dube, A., T. W. Lester, and M. Reich (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The review of economics and statistics 92*(4), 945–964.

Engen, E. M. and J. Gruber (2001). Unemployment insurance and precautionary saving. *Journal of monetary Economics 47*(3), 545–579.

Fuchs-Schündeln, N. and M. Schündeln (2005). Precautionary savings and self-selection: Evidence from the german reunification "experiment". *The Quarterly Journal of Economics 120*(3), 1085–1120.

Ganong, P. and P. Noel (2019). Consumer spending during unemployment: Positive and normative implications. *American economic review 109*(7), 2383–2424.

Gilje, E. P., E. Loutskin, and P. E. Strahan (2016). Exporting liquidity: Branch banking and financial integration. *The Journal of Finance 71*(3), 1159–1184.

Gourinchas, P.-O. and J. A. Parker (2002). Consumption over the life cycle. *Econometrica 70*(1), 47–89.

Gruber, J. (1997). The consumption smoothing benefits of unemployment insurance. *American Economic Review*.

Hagedorn, M., F. Karahan, I. Manovskii, and K. Mitman (2018). Unemployment benefits and unemployment in the great recession: the role of macro effects.

Hagedorn, M., I. Manovskii, and K. Mitman (2015). The impact of unemployment benefit extensions on employment: The 2014 employment miracle? Technical report, National Bureau of Economic Research.

Heider, F. and A. Ljungqvist (2015). As certain as debt and taxes: Estimating the tax sensitivity of leverage from state tax changes. *Journal of Financial Economics 118*(3), 684–712.

Holmstrom, B. and J. Tirole (1997). Financial intermediation, loanable funds, and the real sector. *the Quarterly Journal of economics 112*(3), 663–691.

Homanen, M. (2018). Depositors disciplining banks: The impact of scandals. *Available at SSRN*.

Hsu, J. W., D. A. Matsa, and B. T. Melzer (2018). Unemployment insurance as a housing market stabilizer. *American Economic Review 108*(1), 49–81.

Ivashina, V. and D. Scharfstein (2010). Bank lending during the financial crisis of 2008. *Journal of Financial economics 97*(3), 319–338.

Iyer, R. and M. Puri (2012). Understanding bank runs: The importance of depositor-bank relationships and networks. *American Economic Review 102*(4), 1414–45.

Iyer, R., M. Puri, and N. Ryan (2016). A tale of two runs: Depositor responses to bank solvency risk. *The Journal of Finance 71*(6), 2687–2726.

Jiménez, G., S. Ongena, J.-L. Peydró, and J. Saurina (2014). Hazardous times for monetary policy: What do twenty-three million bank loans say about the effects of monetary policy on credit risk-taking? *Econometrica 82*(2), 463–505.

Khwaja, A. I. and A. Mian (2008). Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review 98*(4), 1413–42.

McKay, A. and R. Reis (2016). The role of automatic stabilizers in the us business cycle. *Econometrica 84*(1), 141–194.

Mody, A., F. Ohnsorge, and D. Sandri (2012). Precautionary savings in the great recession. *IMF Economic Review 60*(1), 114–138.

Mortensen, D. T. and C. A. Pissarides (1994). Job creation and job destruction in the theory of unemployment. *The review of economic studies 61*(3), 397–415.

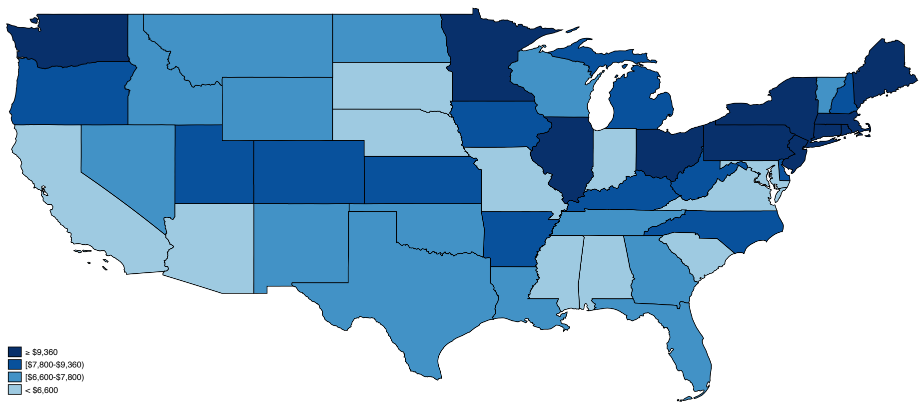
Oster, E. (2017). Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics*, 1–18.

Rajan, R. G. and L. Zingales (1996). Financial dependence and growth. Technical report, National bureau of economic research.

Figure 1
Unemployment Insurance (UI) Benefits in the U.S.

This figure shows the UI benefit distribution across states and its dynamics over time. Panel a shows the level of UI benefits across states for a particular year (2000). The states with darker blue have higher level of UI benefits. Panel b shows the frequency of changes in state UI benefits during our sample period (1995-2010). The states with darker blue change the level of their UI benefits more frequently.

(a)



(b)

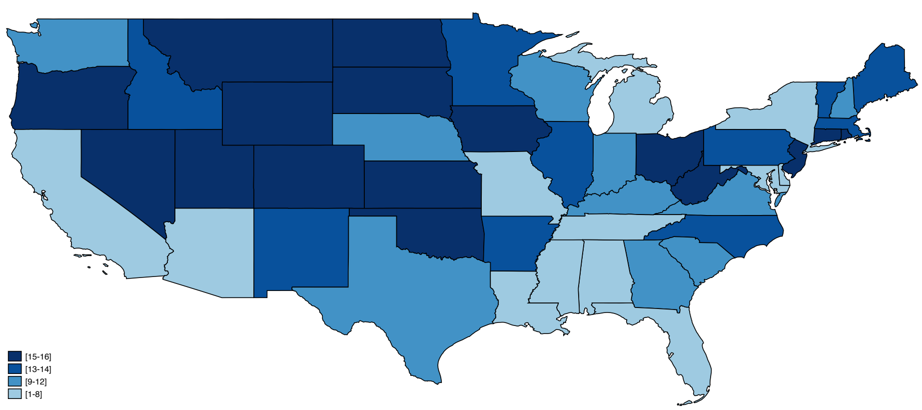
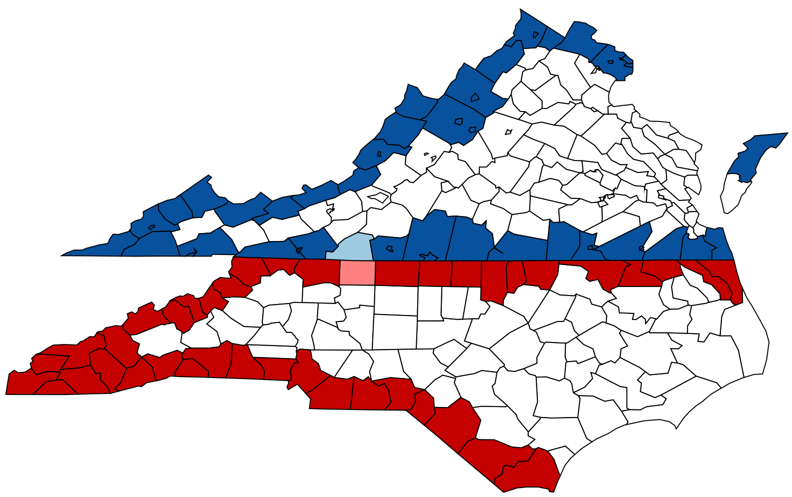


Figure 2
NC and VA County-Level Map: County-Pair Formation

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 and VA border counties are depicted in red and blue, respectively. In Figure 2a, 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 is included only in one county-pair: Stokes-Patrick. In Figure 2b, 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 separate county-pairs for Northampton: Northampton-Southampton, Northampton-Greensville, and Northampton-Brunswick.

(a)



(b)

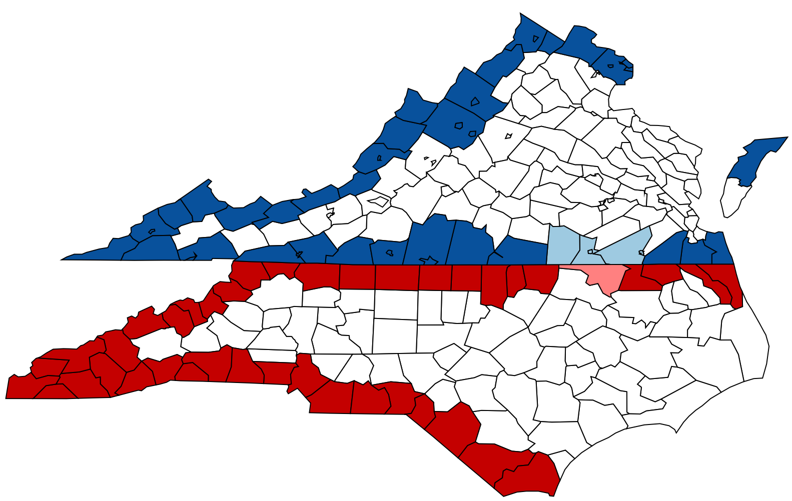


Figure 3
Border Counties

This figure shows the location of all U.S. border counties used in our county-pair comparison analysis. The colored counties are used in our analysis whereas the white counties are non-border counties and excluded from our sample.

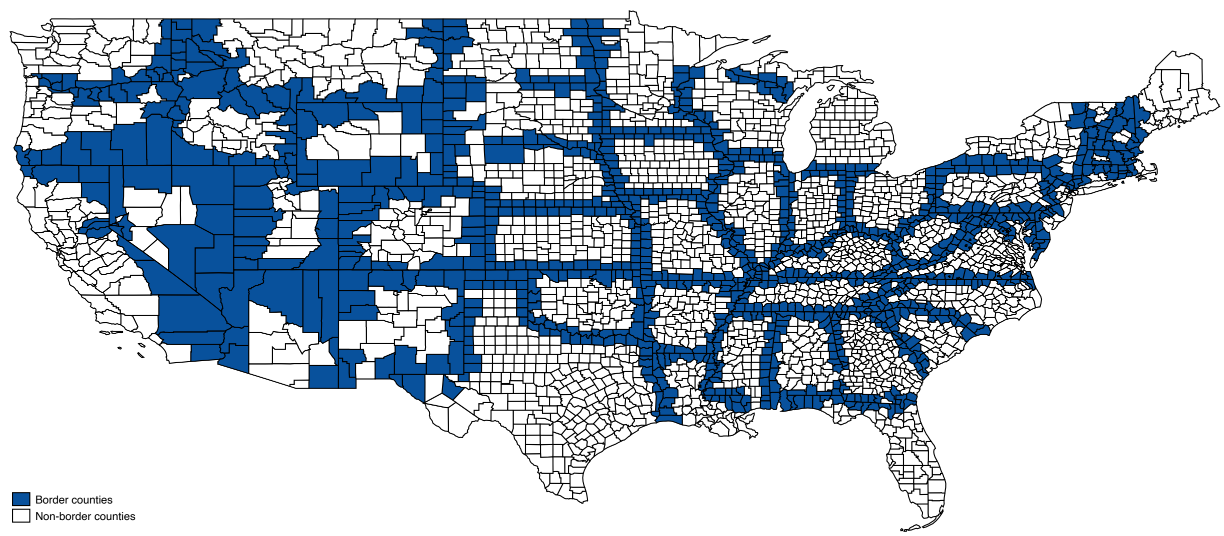
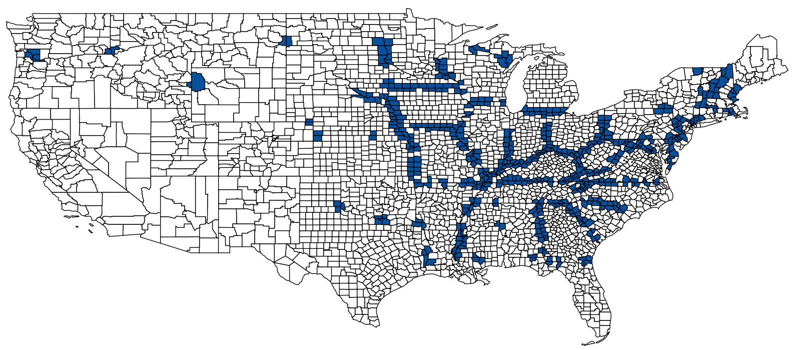


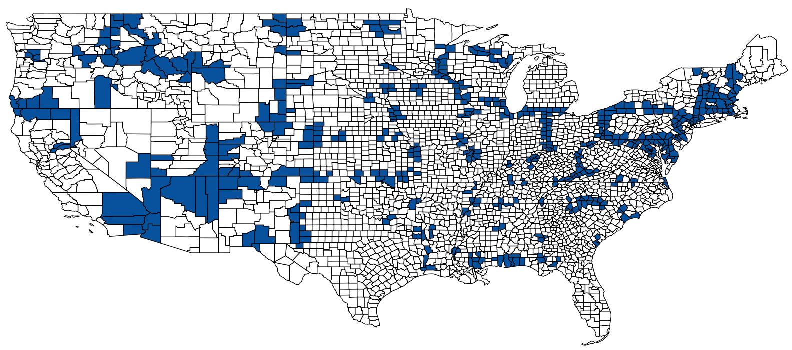
Figure 4
Border County Subsamples

This figure shows the location of all U.S. border counties used in Table 6. Figure 4a shows the county-pairs for which the distance between the centers of two counties within the pair is less than or equal to 25 miles. Figure 4b shows the county-pairs for which the Euclidian distance of industrial compositions of two counties within the pair is less than or equal to the sample tercile value. Figure 4c shows the county-pairs where the two counties in a pair have similar deposit market concentration (i.e., similar county deposit market HHI). Figure 4c shows the county-pairs for which two counties in the pair are also in the same core-based statistical area.

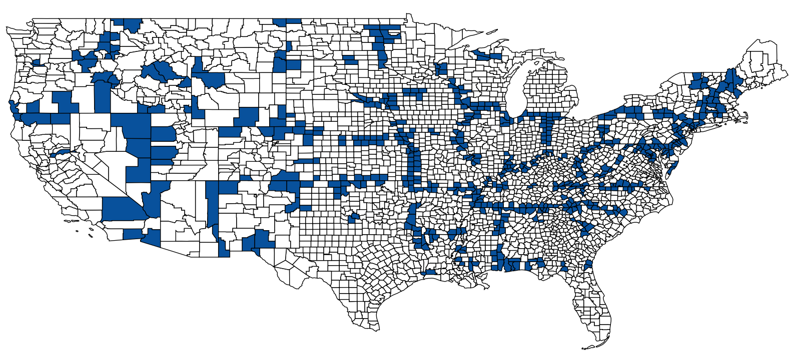
(a)



(b)



(c)



(d)

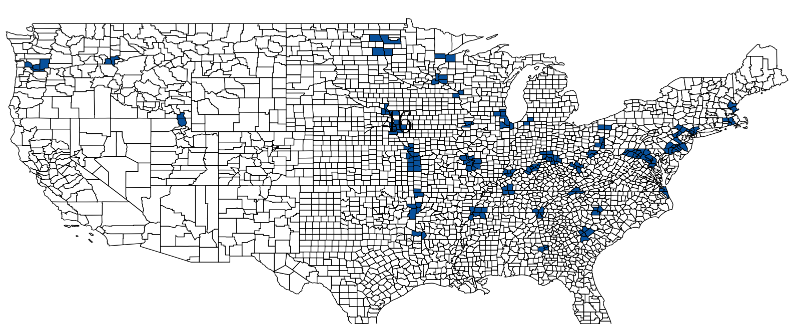


Table 1
Summary Statistics

This table provides summary statistics for the period between 1995 and 2010. Panel A presents the summary statistics at the county-year level for the sample of border counties used in our deposit analysis. Panel B.1 presents the bank-county-year-level statistics for newly originated small businesses loans (CRA) that we use in our lending analysis, and Panel B.2 reports the characteristics of Call Report banks that are used in this analysis. Panel C presents the county-year-level statistics for the sample of counties used in our real effects analysis.

	Mean	SD	25^{th} perc.	Median	75^{th} perc.
Panel A- Deposit Analysis					
(*Border county characteristics*)					
Weekly UI benefit (tho. $)	0.33	0.10	0.26	0.31	0.38
UI benefit duration (weeks)	26.07	0.51	26.00	26.00	26.00
State UI benefit (tho. $)	8.51	2.61	6.66	8.14	10.04
State UI benefit (growth, %)	3.38	3.94	0.00	3.20	4.51
Deposit (mil. $)	1,752	11,942	130	311	769
Deposits (growth, %)	3.57	5.81	0.46	3.28	6.32
Income (mil. $)	2,889	10,090	255	625	1,711
Income (growth, %)	4.34	4.75	2.15	4.44	6.62
Unemployment rate (%)	5.87	2.75	3.95	5.23	7.13
HHI, county	0.31	0.19	0.18	0.25	0.38
Rurality	5.25	2.69	3.00	6.00	7.00
Bachelor's degree (%)	16.20	7.50	11.13	14.30	18.80
Hispanic (%)	4.95	9.49	0.89	1.73	4.06
White (%)	88.01	15.74	85.86	95.12	97.78
Age-65 (%)	14.96	3.97	12.43	14.70	17.22
# of county-pairs	2.06	0.95	1.00	2.00	2.00
Obs. (county × year)	17,802				
Panel B- Lending Analysis					
B.1- Small Business Lending (CRA)					
New Lending (tho. $)	376	1,659	3	58	287
New Lending (log)	3.69	2.52	1.39	4.08	5.66
Obs. (bank × county × year)	364,643				
B.2- Bank Characteristics					
Bank UI exposure (tho. $)	9.05	2.85	7.13	8.63	10.58
Size (mill. $)	4,783	17,758	401	717	1,723
Liquidity (%)	28.26	11.82	19.91	26.91	35.05
Loans (%)	65.21	11.93	58.46	66.49	73.32
Deposits (%)	80.02	9.04	75.59	82.02	86.74
Wholesale fund. (%)	8.95	7.60	2.72	7.33	13.51
Equity (%)	9.28	2.19	7.87	8.87	10.17
Profitability (%)	1.14	0.89	0.93	1.23	1.52
Obs. (bank × year)	12,267				
Panel C- Real Effects Analysis					
(*County characteristics*)					
County UI exposure (tho. $)	9.27	1.72	7.93	9.34	10.54
Average wage (tho. $)	0.55	0.15	0.45	0.52	0.61
Average wage (growth, %)	3.15	4.24	1.38	3.13	4.88
Unemployment rate (%)	5.98	2.67	4.13	5.38	7.16
External finance dependence	-0.17	0.23	-0.27	-0.20	-0.12
Obs. (county × year)	35,764				

Table 2
UI Benefits and State Economic Conditions

This table estimates the correlation between state economic conditions and state UI benefits. Each column uses state-year-level data for the period between 1983 and 2010 and provides the results of a regression model in which the dependent variable is the log change in state UI benefits and the independent variables are several state economic condition proxy variables (lagged one period). Each column includes state and year fixed effects. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(UI Benefit)$					
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta log(GDP)$	0.236***					0.126**
	(0.047)					(0.058)
$\Delta log(Average\ wage)$		0.570***				0.186
		(0.132)				(0.150)
$Unemployment\ rate$			-0.691***			-0.497***
			(0.115)			(0.132)
$\Delta log(UI)\ Reserves$				0.001		0.001
				(0.001)		(0.001)
$Negative\ UIReserves$					-0.009	0.000
					(0.006)	(0.006)
Fixed Effects:						
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Obs.	1,212	1,212	1,212	1,212	1,212	1,212
R^2	0.141	0.138	0.145	0.122	0.123	0.156

Table 3
Deposits and UI Benefits: Within-Pair Estimation

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variable is the log change in the UI benefits of the state where the county is located. The sample includes all U.S. border counties depicted in Figure 3. 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 (i.e., the set of all counties on both sides of a border between two states) and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(County Deposit)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$,	-0.053***	-0.054***	-0.055***	-0.055***	-0.056***
State	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
$\Delta log(Income)$,			0.036**	0.035**	0.037**
County			(0.014)	(0.014)	(0.014)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair × Year FE	Y	Y	Y	Y	Y
County FE	N	Y	Y	Y	Y
Obs.	36,596	36,596	36,596	36,596	36,596
R^2	0.557	0.601	0.601	0.601	0.601

Table 4
Within-Pair Estimation: Continuous Economic Conditions

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variable is the log change in the UI benefits of the state where the county is located. Columns (1) through (3) use the main county-pair sample and use a specification comparable to column (5) of Table 3, 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 instead 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. Standard errors are two-way clustered at state and border segment level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(County Deposit)$					
	Main Sample			Scrambled Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta log(UIBenefit)$,	-0.056***	-0.056***	-0.057***	-0.010	-0.006	-0.010
State	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
$\Delta log(Income)$,	0.036***	0.036**	0.037**	0.062***	0.078***	0.091***
County	(0.013)	(0.014)	(0.014)	(0.017)	(0.017)	(0.017)
$\Delta log(Income)$,	0.014			0.268***		
State	(0.045)			(0.046)		
$\Delta log(GDP)$,		0.018			0.158***	
State		(0.035)			(0.031)	
$Unemp.rate$,			-0.184			-0.501***
State			(0.136)			(0.106)
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.	36,596	36,596	36,596	36,602	36,602	36,602
R^2	0.601	0.601	0.601	0.566	0.566	0.566

Table 5
County Comparisons: Pair County vs. State Counties

This table provides the summary statistics of two comparisons. In the first three columns (under the heading of |Pair-County|), we compare the characteristics of two neighboring border counties in a count pair. In the second three columns (under the heading of |Rest-County|), we compare the characteristics of a border county with the rest of the counties in its own state. 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 between the means of the two comparisons. A negative value in the last column indicates that neighboring 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(population)	0.28	0.27	0.17	0.76	0.62	0.45	-0.49***
log(deposit per capita)	0.13	0.10	0.10	0.24	0.16	0.23	-0.11***
log(income per capita)	0.04	0.03	0.04	0.17	0.16	0.06	-0.13***
log(ave. wage)	0.05	0.05	0.04	0.24	0.25	0.10	-0.19***
Unemployment rate (%)	0.42	0.33	0.30	0.70	0.42	0.57	-0.28***
Manufacturing share (%)	3.20	2.63	2.57	1.70	1.50	1.19	1.50***
Herfindahl-Hirschman ind.	0.05	0.05	0.04	0.03	0.03	0.02	0.02***
Rurality	0.48	0.35	0.41	2.19	2.07	0.95	-1.71***
Bachelor's degree (%)	1.39	0.77	1.42	6.74	6.26	2.86	-5.34***
Hispanic (%)	0.99	0.64	1.06	2.23	1.05	3.13	-1.25***
White (%)	2.43	1.39	2.69	5.31	5.24	3.50	-2.88***
Age-65 (%)	0.91	0.72	0.74	2.53	2.15	1.67	-1.63***
Observations	1,092			1,092			2,184

Table 6
Within-Pair Estimation: County Characteristics

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variable is the log change in the UI benefits of the state where the county is located. 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 for which the distance between the centers of two counties within the pair is less than or equal to 25 miles. Column (2) uses only the county pairs for which the Euclidian distance of the industrial compositions of two counties within the 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 a similar deposit market concentration (i.e., similar county deposit market HHI). Column (4) uses only the county pairs for which two counties in the 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(CountyDeposit)$			
	(1) Distance	(2) Industry	(3) Banking	(4) CBSA
$\Delta log(UIBenefit)$,	-0.060**	-0.071***	-0.064***	-0.106***
State	(0.025)	(0.019)	(0.019)	(0.037)
$\Delta log(Income)$,	0.025	0.005	0.032	-0.124
County	(0.026)	(0.030)	(0.025)	(0.083)
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.	12,224	12,218	12,224	4,704
R^2	0.585	0.609	0.598	0.596

Table 7
Within-Pair Estimation: Excluding Correlated Counties

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variable is the log change in the UI benefits of the state where the county is located. 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 in the regression of county income growth on state income growth). Column (2) excludes the counties that have 2 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(County Deposit)$	
	(1)	(2)
	Low income beta	Low employment share
$\Delta log(UIBenefit)$,	-0.075***	-0.044**
State	(0.026)	(0.021)
$\Delta log(Income)$,	0.036	0.019
County	(0.024)	(0.023)
Controls & Fixed Eff:		
Unemp.	Y	Y
cubic(Unemp.)	Y	Y
Pair × Year FE	Y	Y
County FE	Y	Y
Obs.	10,528	10,628
R^2	0.578	0.596

Table 8
Deposits and UI Benefits: Within-Bank Estimation

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-bank- (i.e., branch-) level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in branch total deposits and the main independent variable is the log change in the UI benefits of the state where the branch is located. 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 state and border segment level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(Branch Deposit)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$,	-0.092***	-0.076**	-0.079**	-0.080**	-0.079**
State	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
$\Delta log(Income)$,			0.095*	0.092*	0.083*
County			(0.048)	(0.049)	(0.048)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair × Year × Bank FE	N	Y	Y	Y	Y
County FE	Y	N	N	N	N
County × Bank FE	N	Y	Y	Y	Y
Pair × Year FE	Y	N	N	N	N
Obs.	38,616	38,616	38,616	38,616	38,616
R^2	0.281	0.679	0.679	0.679	0.680

Table 9
Deposits and UI Benefits: Matching Exercise-Bank Level

This table estimates the effect of bank UI exposure on bank deposits and deposit interest rate. Each column uses bank-year-level data from Call Reports for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is either the log change in bank total deposits (in columns (1)-(3)) or the change in bank deposit rates (in columns (3)-(6)), and the main independent variable is the log change in UI exposure of the bank. Each pair consists of one treated and one control bank. A bank is treated if its UI exposure is above the median value in a given year and in the control group if its UI exposure is below the median value in a given year. The sample excludes the banks with an estimated propensity score above 0.8 or below 0.2. The sample also excludes the bank pairs if the difference between the estimated propensity scores is above 0.034, which is one-fourth of the standard deviation of the estimated propensity score in the sample. Matching is done with replacement. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and year level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	$\Delta log(Deposits)$, *Bank*			$\Delta(Int.\ Exp./Deposits)$, *Bank*		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta log(UI\ Exposure)$, *Bank*	-0.109*	-0.109*	-0.092*	0.003*	0.003*	0.003*
	(0.057)	(0.056)	(0.047)	(0.002)	(0.002)	(0.002)
$\Delta log(Inc.\ Exposure)$, *Bank*	0.120***	0.121***	0.113***	-0.001*	-0.001*	-0.001*
	(0.010)	(0.010)	(0.009)	(0.001)	(0.001)	(0.001)
Controls & Fixed Eff:						
Unemp. Exp.	No	Yes	Yes	No	Yes	Yes
Bank Controls	No	No	Yes	No	No	Yes
Bank Pair x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	96,618	96,602	94,906	81,088	81,072	81,072
R^2	0.509	0.509	0.533	0.720	0.720	0.724

Table 10
Deposits and UI Benefits: Controlling for Firm Deposit Holdings

This table estimates the effect of state UI benefits on bank deposits. All columns use county-level data for the period between 1995 and 2010 and provide the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variable is the log change in the UI benefits of the state where the county is located. The sample includes all U.S. border counties. To calculate county total deposits in column (1), we exclude the branches that are in the top 1st percentile size distribution. 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(County\,Deposit)$			
	(1)	(2)	(3)	(4)
$\Delta log(UIBenefit)$,	-0.042***	-0.055***	-0.058***	-0.058***
State	(0.015)	(0.014)	(0.015)	(0.015)
$\Delta log(Income)$,	0.048***	0.037**	0.036**	0.036**
County	(0.014)	(0.014)	(0.014)	(0.014)
$\Delta log(wage\,base)$,		0.005		0.006
State		(0.008)		(0.008)
$\Delta log(Firm\,UI\,Contr.)$,			-0.004	-0.005
State			(0.004)	(0.004)
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.	36,596	36,596	36,596	36,596
R^2	0.599	0.601	0.601	0.601

Table 11
Deposits and UI Benefits: County Layoff Ratio

This table estimates the effect of state UI benefits on bank deposits. Each column uses county-level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variable is the log change in the UI benefits of the state where the county is located. Columns (1) and (2) split the sample into two subsamples based on the median value of the county layoff ratio. The county layoff ratio is measured as the ratio of workers who experience extended mass layoffs to total county employment (BLS, Mass Layoff Statistics). 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Var: $\Delta log(CountyDeposit)$	
	(1) County Layoff Rate, Low	(2) County Layoff Rate, High
$\Delta log(UIBenefit)$,	-0.019	-0.051***
State	(0.026)	(0.017)
$\Delta log(Income)$,	0.040*	0.006
County	(0.020)	(0.027)
Controls & Fixed Eff:		
Unemp.	Y	Y
cubic(Unemp.)	Y	Y
Pair × Year FE	Y	Y
County FE	Y	Y
Obs.	11,572	11,552
R^2	0.603	0.590

Table 12
Household Deposit Holdings

This table provides summary statistics for household deposit holdings (i.e., transaction accounts). The data are from the PSID for the period between 1994 and 2009. Rows (1) through (5) report the deposit holdings for the following households: (1) all households, (2) households in which the head of household has at least one unemployment spell, (3) households in which the head of household has at least one unemployment spell and is currently employed, (4) households in which the head of household or spouse has at least one unemployment spell, (5) households in which the head of household or spouse has at least one unemployment spell and is currently employed. The statistics are weighted by using the family weights provided in the PSID.

	Mean	SD	25^{th} perc.	Median	75^{th} perc.	Obs.
Households						
(1) All	27,822	118,612	262	3,556	16,748	54,007
Households						
(2) Head with unemp. exp.	15,462	94,906	0	1,052	7,115	19,509
Households						
(3) Head with unemp. exp., currently emp.	16,491	100,705	0	1,231	8,131	16,318
Households						
(4) Head/Spouse with unemp. exp.	18,807	98,602	0	1,525	10,164	25,072
Households						
(5) Head/Spouse with unemp. exp., currently emp.	20,120	104,427	11	1,964	10,668	21,139

Source: University of Michigan, Institute for Social Research, Panel Study of Income Dynamics Sensitive Data Files. Some of the data used in this analysis are derived from Sensitive Data Files of the Panel Study of Income Dynamics, obtained under special contractual arrangements designed to protect the anonymity of respondents. These data are not available from the authors. Persons interested in obtaining PSID Sensitive Data Files should contact through the Internet at PSIDHelp@isr.umich.edu.

Table 13
Small Business Lending and Bank UI Exposure: Within-County Estimation

This table estimates the effect of bank UI exposure on bank small business lending. Each column uses county-bank-year-level data from the CRA data for the period between 1996 and 2010 and provides the results of a regression model in which the dependent variable is the log of new small business lending originated by a bank in a county and the main independent variable is bank UI exposure. Bank UI exposure is the weighted average of the UI level of the states where the bank raises deposits using the deposits of the bank in those states as weights. We exclude the bank-county observations from the sample if the bank raises deposits in the county. Bank controls are size, equity ratio, liquidity ratio, wholesale funding ratio, share of loans in total assets, net income ratio, and interest expense ratio. Bank exposure variables are the economic conditions and the policy environment of the state where the bank raises deposits: exposure to deposit/loan market concentration, exposure to income, unemployment rate, and state-level policy variables (i.e., minimum wage, health insurance payments, union coverage, non-UI transfers). Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and county level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $log(new\,lending)$			
	(1)	(2)	(3)	(4)
$\Delta log(UI\,Exposure)$, Bank	-0.022**	-0.023**	-0.026**	-0.024***
	(0.010)	(0.010)	(0.010)	(0.009)
Controls & Fixed Eff:				
Bank controls	N	N	Y	Y
Bank exposures	N	N	N	Y
Bank FE	Y	N	N	N
County × Year FE	Y	Y	Y	Y
County × Bank FE	N	Y	Y	Y
Obs.	364,643	364,643	364,643	364,643
R^2	0.396	0.645	0.650	0.654

Table 14
Small Business Lending and Bank UI Exposure: Heterogeneity

This table estimates the effect of bank UI exposure on bank small business lending for a specific subsample. Each column uses county-bank-year-level data from the CRA for the period between 1996 and 2010 and provides the results of a regression model in which the dependent variable is the log of new small business lending originated by a bank in a county and the main independent variable is bank UI exposure. Bank UI exposure is a weighted average of the UI level of the states where the bank raises deposits using the deposits of the bank in those states as weights. We exclude the bank-county observations from the sample if the bank raises deposits in the county. Columns (1) and (2) split the sample of banks into two subsamples based on their equity ratios. Columns (3) and (4) split the sample of banks into two subsamples based on their small deposit ratio. Bank controls: size, equity ratio, liquidity ratio, wholesale funding ratio, share of loans in total assets, net income ratio, and interest expense ratio. Bank exposure variables are the economic conditions and the policy environment of the state where the bank raises deposits: exposure to deposit/loan market concentration, exposure to income, unemployment rate, and state-level policy variables (i.e., minimum wage, health insurance payments, union coverage, non-UI transfers). Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at bank and county level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $log(new\,lending)$			
	Equity Ratio		Small Deposit Ratio	
	(1) Low	(2) High	(3) Low	(4) High
$\Delta log(UI\,Exposure)$, Bank	-0.039***	-0.000	0.011	-0.029*
	(0.011)	(0.007)	(0.007)	(0.017)
Controls & Fixed Eff:				
Bank controls	Y	Y	Y	Y
Bank exposures	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y
County × Bank FE	Y	Y	Y	Y
Obs.	166,735	157,170	95,712	97,241
R^2	0.745	0.704	0.729	0.817

Table 15
Real Effects and County UI Exposure

This table lays out the relationship between a county's exposure to UI benefits through its banking system and two of its labor market outcomes: the unemployment rate and the average wage. Each column uses county-year-level data for the period between 1996 and 2010. The independent variable is the log change in a county's exposure to UI benefits. This variable is calculated by taking the weighted average of UI exposures of banks that serve the county in small business lending. In columns (1) and (4), the sample of all counties is used, while in columns (2)-(3) and (5)-(6), the sample is divided into two subsamples based on the county's DEF. Columns (1)-(3) use the log of the county unemployment rate in percentage points as the dependent variable. Columns (4)-(6) use the log change in the county average wage as the dependent variable. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	$log(unemployment\,rate)$			$\Delta log(average\,wage)$		
	(1) All	(2) County DEF Low	(3) County DEF High	(4) All	(5) County DEF Low	(6) County DEF High
$\Delta log(UI\,Exposure)$, County	0.038**	0.025	0.055**	-0.007*	-0.002	-0.012*
	(0.014)	(0.017)	(0.021)	(0.004)	(0.006)	(0.007)
Controls & Fixed Eff:						
State × Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
County bank exposures	Y	Y	Y	Y	Y	Y
County controls	Y	Y	Y	Y	Y	Y
Obs.	35,764	17,966	17,743	35,764	17,966	17,743
R^2	0.921	0.926	0.918	0.164	0.155	0.197

Appendix

Table A1
Deposits and UI Benefits: Matching Exercise-Balance Table

This table presents the summary statistics of the variables that are used in the matching exercise for the treated, control, and full samples. The variables are log(assets), equity ratio, liquidity ratio, bank-level deposit market HHI, cash ratio, banks' exposure to county-level log(income), unemployment rate, and log(wage) where these exposures are weighted averages of these variables weighted by banks' deposit amounts in these counties. $Diff.$ is calculated as the variable's mean value for the $Treated$ sample minus the variable's mean value for the $Full$ ($Control$) sample. $Norm.$ $Diff.$ stands for normalized difference, which is calculated as the differences between the mean values of two groups divided by the square root of the average variances of the two groups. The last column is the percentage change in normalized differences that the matching procedure yields. A negative value means improvement. A bank is treated if its UI exposure is above the median value in a given year. A bank is in the control group if its UI exposure is below the median value in a given year. The sample excludes the banks with an estimated propensity score above 0.8 or below 0.2. The sample excludes the bank pairs if the difference between the estimated propensity scores is above 0.034, which is one-fourth of a standard deviation of the estimated propensity score in the sample. Matching is done with replacement.

	Treated		Full			Norm.	Control			Norm.	% Change in
	Mean	SD	Mean	SD	Diff.	Diff.	Mean	SD	Diff.	Diff.	Norm. Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
log(Assets)	11.59	1.34	11.56	1.35	0.03	0.02	11.61	1.30	-0.02	-0.01	-49.08
Equity(%)	10.71	4.77	10.75	5.54	-0.04	-0.01	10.76	4.95	-0.05	-0.01	19.65
Liquidity(%)	30.64	14.65	31.41	15.58	-0.77	-0.05	29.99	14.75	0.65	0.04	-15.95
HHI, Bank	0.20	0.12	0.23	0.14	-0.03	-0.27	0.22	0.13	-0.02	-0.16	-64.63
Cash(%)	5.44	4.98	5.85	5.59	-0.41	-0.08	5.58	5.04	-0.14	-0.03	-176.35
log(Income), county	14.70	1.91	14.49	1.97	0.21	0.11	14.59	1.91	0.12	0.06	-79.65
Unemp. Rate, county	5.37	2.19	5.59	2.51	-0.22	-0.09	5.47	2.33	-0.09	-0.04	-123.66
log(wage), county	20.51	2.21	20.21	2.31	0.30	0.13	20.30	2.25	0.21	0.09	-43.38
N	52949		57255				25920				

Table A2
Deposits and UI Benefits: Controlling for Other Policies

This table estimates the effect of state-level policies on bank deposits. Each column uses county-level data for the period between 1995 and 2010 and provides the results of a regression model in which the dependent variable is the log change in county total deposits and the main independent variables are the log change in UI benefits, log change in minimum wage, log change in health insurance payments, change in union coverage, and log change in aggregate non-UI transfer payments. These variables are for the state where the county is located. The sample includes all U.S. border counties. 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable: $\Delta log(County\,Deposit)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$,	-0.057***	-0.056***	-0.057***	-0.056***	-0.058***
State	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
$\Delta log(Min.Wage)$,	0.008				0.008
State	(0.007)				(0.007)
$\Delta log(Health\,Ins.)$,		0.010			0.008
State		(0.007)			(0.007)
$\Delta Union\,Coverage$,			-0.055		-0.048
State			(0.057)		(0.057)
$\Delta log(non-UI\,Transfers)$				-0.013	-0.012
State				(0.012)	(0.012)
Controls & Fixed Eff:					
County controls	Y	Y	Y	Y	Y
Pair × Year FE	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y
Obs.	36,596	36,596	36,596	36,596	36,596
R^2	0.601	0.601	0.601	0.601	0.601

Table A3
Other Financial Assets and UI Benefits: Bonds

This table estimates the effect of state UI benefits on county interest income. Interest income does not include interest payments from deposits. All columns use county-level data for the period between 1995 and 2010 and provide the results of a regression model in which the dependent variable is the log change in county interest income and the main independent variable is the log change in the UI benefits of the state where the county is located. The sample includes all U.S. border counties. To calculate deposit interest payments, the interest expense of each bank is calculated using Call Reports data. Then, for each county-bank-year, the calculated interest expense is multiplied by the deposit amount. Finally, estimated interest income from deposits is subtracted from total interest income. The data source is the IRS SOI. 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	$\Delta log(County Interest Income)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$,	-0.005	-0.011	-0.023	-0.023	-0.024
State	(0.064)	(0.069)	(0.069)	(0.069)	(0.069)
$\Delta log(Income)$,			0.369***	0.363***	0.376***
County			(0.082)	(0.081)	(0.082)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair × Year FE	Y	Y	Y	Y	Y
County FE	N	Y	Y	Y	Y
Obs.	35,020	35,020	35,020	35,020	35,020
R^2	0.650	0.658	0.659	0.659	0.659

Table A4
Other Financial Assets and UI Benefits: Stocks

This table estimates the effect of state UI benefits on county dividend income. Interest income does not include interest payments from deposits. All columns use county-year-level data for the period between 1994 and 2010 and provide the results of a regression model in which the dependent variable is the log change in county dividend income 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 data source is the IRS SOI. 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 and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	$\Delta log(County\,Dividends)$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$, State	0.035	0.046	0.037	0.037	0.037
	(0.042)	(0.045)	(0.045)	(0.045)	(0.045)
$\Delta log(Income)$, County			0.251***	0.252***	0.252***
			(0.072)	(0.072)	(0.072)
Controls & Fixed Eff:					
Unemp.	N	N	N	Y	Y
cubic(Unemp.)	N	N	N	N	Y
Pair × Year FE	Y	Y	Y	Y	Y
County FE	N	Y	Y	Y	Y
Obs.	35,776	35,776	35,776	35,776	35,776
R^2	0.754	0.759	0.760	0.760	0.760

Table A5
Household Awareness, Google Trends

This table documents the relationship between Internet Search and UI. Internet search information is taken from Google Trends. The query that is used in this table is "Unemployment Benefits." Google Trends provides a trend index of search queries at the state level. The index value is between 0 and 100 where the highest value is normalized to 100. Each column uses state-level data for the period between 2004 and 2010 and provides the results of a regression model in which the dependent variable is the change of Google trends and main independent variable is the state-level contemporaneous log change of UI benefits. The control variables are contemporaneous log change of nominal income, change of unemployment rate, log change of real GDP at the state level, and state fixed effects. Standard errors are clustered at state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	$\Delta Web\ Search$				
	(1)	(2)	(3)	(4)	(5)
$\Delta log(UIBenefit)$,	4.901**	8.468***	9.443***	5.081***	4.505**
State	(2.247)	(2.825)	(2.115)	(1.834)	(1.769)
$\Delta log\ (Income)$,			-26.589***	4.646	8.305*
State			(1.957)	(3.787)	(4.188)
$\Delta(Unemp.\ Rate)$,				106.413***	96.874***
State				(11.543)	(10.560)
$\Delta log(GDP, Real)$,					-12.412***
State					(3.374)
State FE	N	Y	Y	Y	Y
Obs.	294	294	294	294	294
R^2	0.009	0.042	0.337	0.566	0.581