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Unintended Consequences of Unemployment Insurance Benefits: The Role of Banks

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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 deposit holdings due to reduced precautionary savings. Because 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 reduce their loan supply to small businesses. Furthermore, counties that are served by these banks experience a higher unemployment rate and lower wage growth.

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Keywords: bank funding • bank lending • precautionary saving • unemployment insurance • deposits

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. At the same time, they can stabilize macroeconomic fluctuations as they redistribute income to the households in need. On the unintended side, however, higher UI generosity may reduce employment by lowering both the job search intensity of the unemployed and firms' job creation.[1] In this paper, we uncover a novel mechanism with several unintended consequences and contribute to the earlier discussion by showing that UI policies might affect 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 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 with show that a UI-induced decline in deposits lowers bank credit supply to small businesses.[2] Third, we show that the resulting lower credit, in turn, has real effects. The counties that are served by banks with higher "UI exposure" experience a higher unemployment rate and lower wage growth.

To investigate how changes in UI benefits affect bank deposits, we use annual county-level deposit data from the Summary of Deposits (SOD) and state-level UI data from the Department of Labor for the period between 1995 and 2010. The main identification challenge is that contemporaneous changes in economic conditions that are correlated with UI and deposits might bias the results. In particular, if we fail to control for relevant economic conditions, we may falsely attribute the changes in deposits to the changes in UI benefits.[3]

To address this identification challenge, we exploit the discontinuous changes in the level of UI benefits at state borders (Dube et al. 2010, Hagedorn et al. 2015). In particular, we make a *within-county-pair estimation* (or simply *within-pair estimation*) by comparing the deposits of two contiguous counties at state borders, one of them in one state and the other in the neighboring state. The level of UI benefits is determined at the state level, hence 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. Therefore, within-pair estimation enables us to control for relevant economic conditions and identify the effect of UI on deposits.

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%. The results do not change when we include additional county-level variables (county income, unemployment rate) to control for county economic conditions, or when we include county fixed effects to control for time-invariant county-level characteristics.

The key identifying assumption in our 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. To test this assumption in our empirical setting, we include state-level variables that may proxy state economic conditions into our regressions.[4] We find that they all have insignificant coefficients that indicate that state-level economic conditions affect the two counties in the pair symmetrically. Thus, their net effect on deposits in the county-pair comparison is zero. To ensure that this is a valid test, we construct a randomly scrambled sample by matching two nonneighboring counties located in different states and conduct our test with this scrambled sample. We find that all state-level economic conditions proxies have significant coefficients with their predicted signs, indicating that these proxies are relevant for county deposits.

Another concern might be that there may be a 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, 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 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, (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 UI benefits can reduce deposits through households by increasing the unemployment rate, therefore, leading these households to use their savings more during their unemployment spells. Alternatively, UI benefits might weaken the households' precautionary saving motive (Hansen and İmrohoroğlu 1992, Engen and Gruber 2001). We provide extensive evidence that the mechanism via unemployment is unlikely to drive our results. We first add unemployment rate (up to its third-degree form) to our regression to control for the unemployment mechanism. We also use" mediation analysis," a method widely used in the policy evaluation literature, to disentangle the role of precautionary motive. Third, we exploit county-level heterogeneity in the correlation between UI benefits and unemployment rate and investigate if our results change based on how sensitive this correlation is. In addition, we use the finding in the literature that UI's effect on unemployment is low when unemployment is high to quantify the role of unemployment on deposits.[5] Separately, we use the Panel Study of Income Dynamics (PSID) data to limit the analysis to the employed. Results from all these tests suggest that the precautionary motive accounts for almost all the decline in deposits.

Another possible mechanism that might drive our results is that UI benefits could lower deposits through firms, since U.S. states finance their UI payments via taxes on firms. Therefore, more generous UI benefits increase firms' tax payments, which may lower their deposit holdings. However, two exercises that we perform suggest that firm deposits may not be the primary driver of our results. First, we explicitly include firms' UI tax contributions to state UI funds in our regressions, and find that the coefficient of firms' UI tax contributions is negative but insignificant. More importantly, 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 results imply that firm deposits cannot explain our results.

The other possibility is that banks might reduce their deposit demand when UI increases. Banks adjust the composition of their liability side based on their asset side or vice versa (Berlin and Mester 1999, Drechsler et al. 2021). 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 suggest that our results are not driven by bank deposit demand. First, relying on the assumption that a bank's deposit demand is determined at the bank level (Gilje et al. 2016, Drechsler et al. 2017), we control for the bank deposit demand by comparing the deposits of the two branches of the same bank, one of them located in one county and the other one in the other county across the border. We find that the branch located in the county with higher UI benefits experiences a lower deposit growth. Next, we show that bank deposit rates and deposit amounts move in the opposite direction as UI benefits change, which implies a supply-side explanation. Taken together, both sets of results suggest that the bank deposit demand is not the driver of the decline in deposits.

Several additional analyses and robustness checks support our interpretation of the results. First, although it is not possible to completely rule out the impact of other state-level policies on our results, it is reassuring to see that 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 show that our results continue to hold with alternative UI-replacement rate definitions, such as benefits relative to state/county income measures. Third, controlling for counties' different levels of sensitivities to national shocks does not change our conclusions. Fourth, 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 test whether banks that raise deposits in UI-generous states reduce their lending. Deposits are unique for banks since they are the largest and most stable funding source that banks rely on (Ivashina and Scharfstein 2010, Hanson et al. 2015, Drechsler et al. 2017). 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 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.

The common identification challenge in uncovering the effect on loan supply is to keep loan demand constant. To do this, we follow Khwaja and Mian (2008) and implement a within-county estimation strategy using annual county-bank-level small business lending 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. We find that banks that collect deposits in states with more generous UI benefits originate less new lending compared with other banks. The effect is economically significant, with an 8.7% decrease in new lending in response to an interquartile range increase in bank UI exposure. The link between bank UI exposure and loan supply is especially strong for banks with a higher reliance on small deposits and banks with a lower equity ratio.

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 conjecture 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. Because 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).

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 county labor markets. With these controls, we compare counties with the same level of state UI benefits but with different levels of UI exposure through their lenders. Our results indicate that counties' UI exposure through their lenders has moderate real effects. In particular, we find that when a county's UI exposure increases by an interquartile range, its unemployment rate increases by 0.3% 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 findings of this paper are important for at least two reasons. First, the results highlight a new and previously unnoticed mechanism relevant to the policy discussions surrounding UI policies. UI policies affect bank funding by reducing deposits—the largest and most stable funding source for banks. The resulting decrease in deposits limits counties' access to bank credit and hence makes them experience worse labor market outcomes. On top of this, the results show that changes in UI generosity can generate spillover effects via banks' internal capital markets. By reallocating deposits across branches, banks carry the effects of a change in UI from one location to other locations.

Second, our findings suggest a mechanism that might mitigate the aggregate stabilizing effects of UI policies. Banks with more deposit funding perform better during downturns, that is, their lending declines less compared with the ones that rely more on market funding. Therefore, more generous UI benefits, although stabilizing aggregate downturn by helping the unemployed to smooth their consumption, might hurt the economy via their weakening effects on bank funding composition.

We 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 employee reservation wages and therefore decreasing firms' job creation incentives (Hagedorn et al. 2013, 2015; Chodorow-Reich et al. 2019). 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 also contributes to the recent literature that studies the stabilizing role of UI policies. Hsu et al. (2018) show that UI benefits lower unemployed's mortgage default rate and hence insulate the housing market from labor market shocks. Di Maggio and Kermani (2017) find that household consumption and delinquencies become less responsive to local shocks when UI benefits are more generous. Our paper provides a mechanism that might work against the stabilizing effects. Specifically, as UI insulates households from negative shocks, it reduces household precautionary savings. We link this effect to bank deposits and analyze its influence on bank lending and labor market outcomes.[6]

Our paper is also related to the literature on the role of deposits in the banking industry. The literature documents that deposits are the largest and most reliable source of funding for banks (Stein 1998, Hanson et al. 2015). Therefore, deposit outflows lead to a reduction in bank loan supply (Ivashina and Scharfstein 2010). In this paper, the driving force behind the decline in deposits is not the deterioration of bank fundamentals or monetary policy (Iyer and Puri 2012, Drechsler et al. 2017), but instead the change in the generosity of UI benefits. The literature also documents that economic shocks that affect deposits can be transmitted through banks' internal capital markets (Gilje et al. 2016, Doerr et al. 2023). Our findings show that the impact of UI on deposits in one state is channeled to other states via the banking system.

2. 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.

2.1. Deposit Analysis

In this section, we detail the data sources and variables that play the central role in our deposit analysis. We start with describing the UI policies in the United States.[7] UI policies provide income to eligible workers who involuntarily become unemployed. Although the basic framework and features of the UI system in the United States are set by federal law, 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 United States uses its own UI trust funds to make UI benefit payments. The funds are financed mainly by raising taxes on firms. States use an experience-based tax system, meaning that firms with more UI 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. 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 annual frequency and branch characteristics such as location and parent bank.

In our deposit analysis, we investigate how the changes in state UI 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]

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 OA1). Although 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%.

2.2. 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 CRA annual bank-county-level small business loan data. 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 banks' small business lending with their UI exposure, we merge the data with bank balance sheet information from Call Reports to

Table 1. Summary Statistics

Variable	Mean	Standard deviation	25th percentile	Median	75th percentile
Panel A: Deposit analysis					
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
No. of county-pairs	2.06	0.95	1.00	2.00	2.00
Observations (county × year)	17,802				
Panel B: Lending analysis					
B.1: Small business lending (CRA)					
New Lending (tho. $)	376	1,659	3	58	287
Observations (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
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
Observations (bank × year)	12,267				
Panel C: Real effects analysis					
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
Observations (county × year)	35,764				

Notes. 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 business 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.

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%. 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 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.

2.3. 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.[9] 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 from Rajan and Zingales (1996). Namely, DEF is defined as capital expenditures minus cash flow from operations divided by capital expenditures. We use Compustat firms to calculate each industry's DEF at two-digit Standard Industrial Classification (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.

3. Deposit Analysis

In this section, we use county-level total deposits and state-level UI 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 due to 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. The positive correlation between UI generosity and economic conditions implies that failing to control for economic conditions should attenuate the negative effect of UI generosity on deposits, not increase it. 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.

3.1. Identification Strategy and Main Results

We address this identification challenge with a border county design in 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 1(a) shows county-level maps of the state of North Carolina (NC), in the south of the horizontal line, and the state of Virginia (VA), in the north of the horizontal line. The dark shaded county at the NC border is Stokes County. Because the only county located in VA that shares the same border with Stokes County is Patrick County (dark shaded county in VA), 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 1(b) 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

Table 2. UI Benefits and State Economic Conditions

	Dependent variable: Δlog(*UI Benefit*)					
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δlog(*GDP*)	0.236***					0.126**
	(0.047)					(0.058)
Δlog(*Average wage*)		0.570***				0.186
		(0.132)				(0.150)
Unemployment rate			−0.691***			−0.497***
			(0.115)			(0.132)
Δlog(*UI*) *Reserves*				0.001		0.001
				(0.001)		(0.001)
Negative UI Reserves					−0.009	0.000
					(0.006)	(0.006)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	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

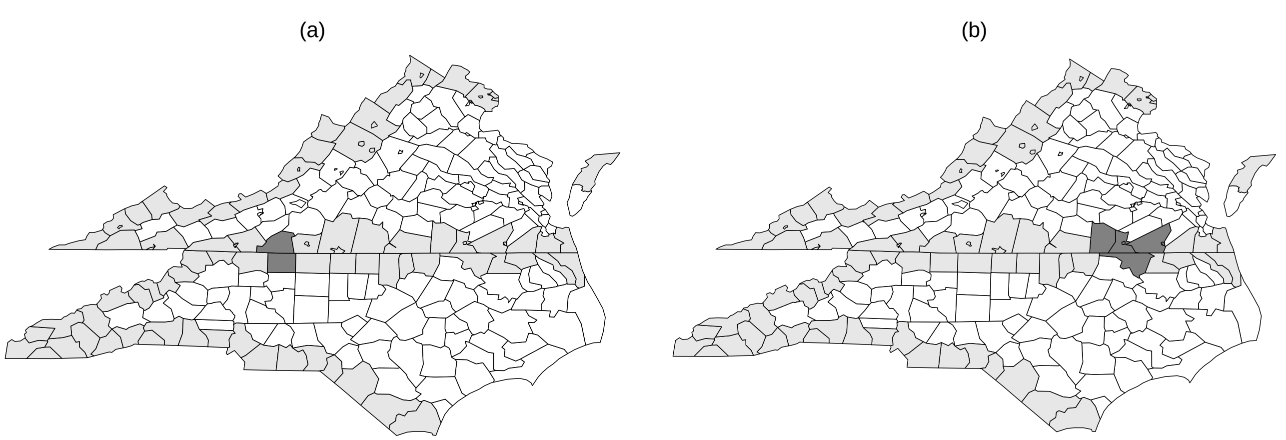
Notes. This table estimates the correlation between state economic conditions and state UI benefits. Each column uses state-year-level data for the period between 1983 and 2010 and provides the results of a regression model in which the dependent variable is the log change in state UI benefits and the 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.

$^*p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

in our empirical analysis: Northampton-Southampton, Northampton-Greensville, and Northampton-Brunswick.[10] 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 of 17,802 unique county-year observations (Table 1).[11] Figure 2 displays the location of all border counties used in our county-pair comparison analysis.

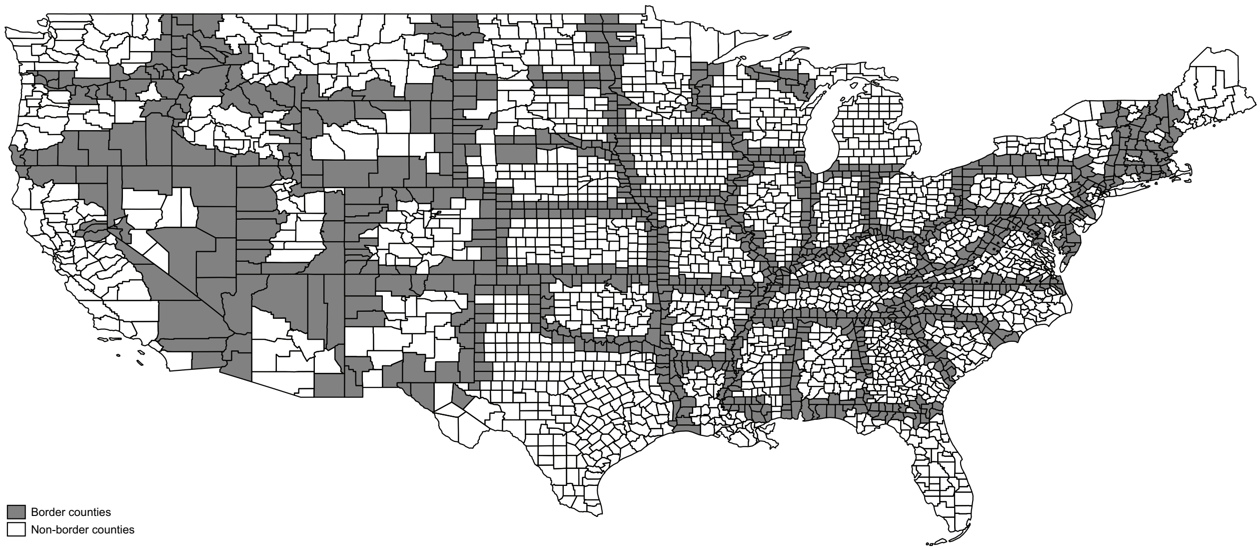
Why is this within-county-pair estimation useful for our purposes? The two counties within a county-pair share the same geography and climate, have access to the same transportation routes, and, more importantly, are open to similar spillover effects of economic changes. These characteristics suggest that a state-level economic shock is expected to affect the two counties within a county-pair symmetrically, since the economic conditions are continuous in the sense that state borders do not affect the movement of the economic shocks (Dube et al. 2010, Hagedorn et al. 2013, Brown and Matsa 2020). 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

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



Notes. This figure is the county-level map of the state of North Carolina (NC), in the south of the horizontal line, and the state of Virginia (VA), in the north of the horizontal line, and provides two examples that show how we form our county-pairs. NC and VA border counties are shaded. In the figure on the left, dark shaded county at NC border is Stokes County and the dark shaded county at VA border is Patrick County. Because 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 the figure on the right, dark shaded county at NC border 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.

Figure 2. Border Counties



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

UI benefits because 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.

It is worth noting that 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 3.2, we provide robustness checks and tests to support this identifying assumption.

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

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

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,[12] $\delta_{p(c),y}$ 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 from each other. Under our identifying 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 a downward bias in standard errors. Second, because a border county may neighbor multiple counties on the other side of the border,[13] 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.[14]

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

Table 3. Deposits and UI Benefits: Within-Pair Estimation

	Dependent variable: Δlog(*County Deposit*)				
Variable	(1)	(2)	(3)	(4)	(5)
Δlog(*UI Benefit*), State	−0.053***	−0.054***	−0.055***	−0.055***	−0.056***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Δlog(*Income*), County			0.036**	0.035**	0.037**
			(0.014)	(0.014)	(0.014)
Unemployment	No	No	No	Yes	Yes
cubic(Unemployment)	No	No	No	No	Yes
Pair × year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	No	Yes	Yes	Yes	Yes
Observations	36,596	36,596	36,596	36,596	36,596
R^2	0.557	0.601	0.601	0.601	0.601

Notes. 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 2. 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.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

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 income in column (3) as a proxy for county economic conditions. We further control for county labor market conditions that may be correlated with state-level economic conditions, and hence with state UI 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 are still highly significant. The economic meaning of the coefficient in the last column is that total county deposits decrease by 2.3% in response to an interquartile range increase in the level of state UI benefits.[15]

3.2. 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 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 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 regression where there are pair × year fixed effects, we should have a zero coefficient for the proxies of state economic conditions (Hagedorn et al. 2013). In columns (1) through (3) of Table 4, we use our main border county sample and include state income, state gross domestic product (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 importantly, 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 nonneighboring 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

Table 4. Within-Pair Estimation: Continuous Economic Conditions

	Dependent variable: Δlog(*County Deposit*)					
	Main sample			Scrambled sample		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δlog(*UIBenefit*), State	−0.056***	−0.056***	−0.057***	−0.010	−0.006	−0.010
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Δlog(*Income*), County	0.036***	0.036**	0.037**	0.062***	0.078***	0.091***
	(0.013)	(0.014)	(0.014)	(0.017)	(0.017)	(0.017)
Δlog(*Income*), State	0.014			0.268***		
	(0.045)			(0.046)		
Δlog(*GDP*), State		0.018			0.158***	
		(0.035)			(0.031)	
Unemployment rate, State			−0.184			−0.501***
			(0.136)			(0.106)
Unemployment	Yes	Yes	Yes	Yes	Yes	Yes
cubic(Unemployment)	Yes	Yes	Yes	Yes	Yes	Yes
Pair × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	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

Notes. 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. 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.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

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.[16]

We perform additional tests to mitigate concerns regarding the use of border county design (the results are in the Online Appendix). First, we address the concern that two counties in a county-pair might still differ from each other, which may make these counties react to state-level shocks asymmetrically. Similarly, 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 characteristics of two border counties within a county-pair (Table OA1). We show that their characteristics remain close to each other. When we compare the characteristics of a border county with the rest of the counties in the same state we find that border counties are more similar to each other than they are to the rest of the counties in their own state. 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.

Second, we restrict our sample based on the degree of similarities of the two counties within a county-pair. Specifically, we estimate our specification for the county-pairs only if (i) the distance between counties is less than or equal to 25 miles, (ii) counties have similar industrial composition, (iii) counties have similar local banking competition, and (iv) counties are in the same statistical area. The coefficients are all negative and significant despite the notable decline in sample size (Table OA2).

Finally, in Table OA3, 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 excluding 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% 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.

3.3. Underlying Mechanism

Why do we observe a lower amount 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' deposit holdings due to reduced precautionary savings. We begin with the household deposit supply mechanism. After that, we provide evidence that alternative mechanisms are unlikely to drive our results.

3.3.1. Household Deposit Supply. There are two channels through which UI can reduce household deposit holdings. The first channel works through households' precautionary savings. The idea is that to the extent that UI protects households from adverse shocks, a more generous UI allows households to reduce their precautionary savings. Given that bank deposits are the main saving tool for households, this reduction in precautionary savings, in turn, may reduce the deposits. Alternatively, by providing income during unemployment, UI can lengthen the unemployment rate and its duration (e.g., by lowering job search efforts or increasing reservation wages). Because households are likely to drain their savings when unemployed, UI can reduce deposits via this channel. That being said, higher UI also implies higher income for the unemployed, which lowers the need to tap into their own deposits. Therefore, the net effect of UI on deposits via unemployment might be small. We perform several tests to show that the mechanism via UI unemployment is not strong enough to derive our results and hence conclude that the precautionary motive is more likely to drive our results.

To begin with, regarding the impact of UI on unemployment, the empirical studies document small or insignificant effects (Chodorow-Reich et al. 2019, Boone et al. 2021).[17] In line with these findings, we also find that the unemployment rate and UI are positively related in our county sample, but the effect is small. A one-standard-deviation increase in UI increases the change in the unemployment rate by 3.9 basis points.[18]

Given the small economic effect of UI on the unemployment rate in our sample, the effect of UI on deposits through unemployment is not expected to be large. Nevertheless, to disentangle the impact of UI on deposits through a precautionary saving mechanism we start by including the county-level unemployment rate and its third-degree polynomial as control variables in our benchmark model (column (5) in Table 3). If the decline in deposits is mainly driven by the higher unemployment rate and/or longer unemployment duration, including the unemployment rate or its third-degree polynomial as control variables should reduce the estimated impact of UI. However, Table 3 documents that the magnitude of UI's coefficient remains mostly unchanged.

To provide direct evidence of the impact of UI benefits on household savings we use household-level data from the PSID. The main advantage of the PSID data are that we can observe the employment status of the household head, which enables us to selectively focus on the employed and hence disentangle the impact of the unemployment channel on our results. The main disadvantage is that we cannot use the border discontinuity design as we can only observe the state, not the county, of residence. Thus, the results of this analysis should be interpreted with care.

We use the same household-level specification and control variables as in Engen and Gruber (2001) and find that household liquid savings (i.e., transaction deposits) decline with higher UI. Column (1) of Table 5 is the baseline specification with relevant demographic controls and indicates a negative association between household deposits and UI generosity. To control for nonlinearities in household saving decisions, in the next two columns, we include higher orders of age and wage variables. Doing so does not change the results. Next, we focus on a sample where we include only the employed. Specifically, in column (4), we exclude from the sample the households in which the household head is unemployed during the current interview wave. We find that our results continue to hold, and the size of the coefficient remains mostly unchanged. This finding underlines the importance of the precautionary savings channel for our results since the channel via UI-unemployment relation should not influence the employed by construction. In column (5), to avoid the possibility that recent unemployment spells (not only the current one) might influence households' current savings, we exclude from the sample the households in which the household head has experienced any unemployment spell during the last three interview waves. Our results still hold with similar-sized coefficients. These results suggest that the effect via the UI-unemployment mechanism is likely to be small in our empirical setting.

We report the results of three more tests that support the precautionary mechanism in the Online Appendix. First, we use mediation analysis, which enables us to hold the effect through unemployment constant while estimating the effect through precautionary savings by splitting the total effect into direct and indirect effects.

Table 5. Household Liquid Savings and UI Benefits: PSID Sample

	log(*Deposit to Income ratio*)				
	All sample			Employed	
Variable	(1)	(2)	(3)	(4)	(5)
log(*UI Benefit*), *State*	−0.015**	−0.016**	−0.016**	−0.015**	−0.016**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Household demographics	Yes	Yes	Yes	Yes	Yes
Quadratic(age & wage)	No	Yes	Yes	Yes	Yes
Quartic(wage)	No	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	38,660	38,660	38,660	36,247	33,632
R^2	0.100	0.103	0.105	0.102	0.099

Notes. This table estimates the effect of state UI benefits on household deposit holdings (i.e., transaction accounts) using the PSID data between 1994 and 2009 (1994, 1999, 2001, 2003, 2005, 2007, and 2009 waves), following the specifications in Engen and Gruber (2001). Each column uses household-level data and provides the results of a regression in which the dependent variable is the household deposit holdings normalized by household income and the main independent variable is the log of UI benefits of the state where the household resides. Columns (1) through (3) include all households; column (4) excludes households in which the household head is unemployed; column (5) excludes households in which the household head experienced any unemployment spell during the last three interview waves. Household control variables and fixed effects are indicated at the bottom of each column. Household demographics include head age, sex, marital status, race, education, wage, spouse education, and family size. Standard errors are clustered at the state-year level and reported in parentheses.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

The total effect is the overall effect of UI benefits on deposits while the indirect effect is the effect of UI benefits on deposits through its impact on the unemployment rate. Therefore, the direct effect is the difference between the total and the indirect effects, which is the precautionary savings channel in our setting. Table OA4 reports the direct effect of UI benefits on deposits while fixing the level of the unemployment rate to each county's sample mean values. In all columns, the magnitude of the direct effect is only slightly smaller than our benchmark results, suggesting that the unemployment channel does not drive our results.

Second, we exploit county-level heterogeneity in the correlation between UI benefits and unemployment rate to test whether the unemployment channel is at work for particular counties. Specifically, we might see an effect of UI on deposits through unemployment in the counties where the unemployment rate and UI are correlated. For this test, first, we estimate county-level betas, β_{UI}, by regressing county-level change in the unemployment rate on UI benefits and a linear time trend for each county separately. Second, relying on these county-level betas, we test if the impact of UI benefits on deposits shows heterogeneity across counties. Our results reported in Table OA5 show that counties do not show heterogeneity in the impact of UI on deposits based on how their unemployment rate and UI benefits are correlated. We also find that UI has a positive and significant coefficient when β_{UI} is insignificant (i.e., the unemployment channel is ineffective), supporting the precautionary savings mechanism. We estimate a slightly larger effect when β_{UI} is significant. Yet, the increase is statistically insignificant and small in magnitude.

In the third test, we leverage the literature's finding that the effect of UI on unemployment rate and unemployment duration decreases during recessions–periods with high unemployment risk (Schmieder et al. 2012).[19] We exploit these differential effects in a heterogeneity test. If the channel through unemployment rate and duration is more important, the decline in deposits should be higher in counties with low unemployment risk. On the contrary, if the precautionary savings channel is more important, the reduction in deposits should be higher in counties with high unemployment risks. We use mass layoff statistics as a proxy for unemployment risk and evaluate how the decline in deposits interacts with this risk in Table OA6. This heterogeneity test reveals that the negative effect of UI benefits on deposits is stronger for counties with high layoff ratios, suggesting that the precautionary savings mechanism is likely to be more important for the decline in deposits.

Do households with unemployment risk have enough deposit holdings at banks? This is crucial for the validity of the results. It is possible that deposit holdings can be large for individuals with a low level of unemployment risk and low or nonexistent for those with a high level of unemployment risk. The PSID data suggest that this is not the case (Table OA7). Households with past unemployment experience hold, on average, significant deposits (more than 15,000 USD).[20]

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 deposit 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 UI benefits.[21] 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). 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 are the most common savings instrument for most of households, and for most of them, it is the only one. According to the Survey of Consumer Finances (SCF), whereas more than 90% of families have transaction accounts, only 20% of families directly hold stocks or bonds or both.[22] Furthermore, stocks and bond holdings are concentrated mainly among the highest-income people.[23]

3.3.2. Firm Deposit Supply. Generous state UI benefits may also reduce the amount of deposits firms hold at banks because firms may be the ones financing more generous UI benefits by paying more taxes. As the SOD data do not provide the composition of deposit holdings at bank branches, we cannot directly separate the impact of UI policies on the household and firm deposit holdings. Yet, we use several exercises to explore which of these two channels is more likely to be the main driver of the decline in deposits.

In our first exercise, we exclude the bank branches that firms are more likely to work with (column (1) of Table 6). Specifically, we exclude the largest branches (i.e., top 1%) 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). 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. Our second exercise builds on the idea that if the negative effect of UI generosity on deposits were driven by firms, we would expect that firms' contribution to state UI funds would be the main channel. Therefore, controlling for firms' contribution to state UI funds should make the coefficient of the UI insignificant. The remaining columns of Table 6 show that this is not the case. In these columns, the coefficient of firms' UI tax contributions is negative as expected but insignificant. More importantly, the coefficient of UI benefits stays unchanged. Overall, the results in Table 6 suggest that the decline in deposits is more likely to be driven by households rather than by firms.

3.3.3. Bank Deposit Demand. An alternative explanation for the decline in deposits with more generous UI could be 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 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. 2021).

We rule out this demand-driven explanation with 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 in the other county in the pair. This within-bank estimation controls for bank deposit demand with the assumption that the deposit demand of a bank is determined at the bank level and 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. 2017). Therefore, the bank demand for deposits stays constant across its branches, which allows us to measure the impact of UI benefits

Table 6. Deposits and UI Benefits: Controlling for Firm Deposit Holdings

	Dependent variable: Δlog(*County Deposit*)			
Variable	(1)	(2)	(3)	(4)
Δlog(*UIBenefit*), State	−0.042***	−0.055***	−0.058***	−0.058***
	(0.015)	(0.014)	(0.015)	(0.015)
Δlog(*Income*), County	0.048***	0.037**	0.036**	0.036**
	(0.014)	(0.014)	(0.014)	(0.014)
Δlog(*wage base*), State		0.005		0.006
		(0.008)		(0.008)
Δlog(*Firm UI Contr.*), State			−0.004	−0.005
			(0.004)	(0.004)
Unemployment	Yes	Yes	Yes	Yes
cubic(Unemployment)	Yes	Yes	Yes	Yes
Pair × year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Observations	36,596	36,596	36,596	36,596
R^2	0.599	0.601	0.601	0.601

Notes. 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.
$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 7 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 (results are reported in the Online Appendix). If the results are demand-driven, then the price (deposit rate) and quantity (deposit amount) should move in the same direction; on the other hand, if the results are supply-driven, they should move in the opposite direction. Similar to comparing two neighboring counties across a state border, we compare the deposit amount and deposit rate of two comparable banks with different levels of UI exposure by using propensity score matching.[24] The results reported in Table OA8 are consistent with the supply-driven story.

3.4. Robustness Checks

In this section, we summarize the results of several robustness tests, which we report in the Online Appendix. 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 UI policies. To alleviate such concerns, in Table OA10, we control for several other state policies. Namely, we include changes in the minimum wage, health insurance payments, union coverage, total non-UI transfers, right-to-work laws, and income tax progressivity as important state-level policies as additional controls. We use both the average and the maximum amount of state income tax to proxy state income tax progressivity. We consider that tax progressivity is particularly important to control for because a more progressive tax system lowers income risk and, therefore, might lower precautionary savings. As a result, if correlated with the UI policies, it might bias our results. Including these controls either individually or altogether does not change the magnitude and significance of the coefficient of UI generosity.

In the paper, we follow the literature and measure the UI generosity by using the product of dollar cap and benefit duration (referred to as "state UI benefit" in the paper). As a robustness exercise, we estimate our main specification with five alternative replacement rates. We report the results in Table OA11. In columns (1) through (3), we calculate the replacement rates by dividing state maximum UI benefits by state average wage, county average wage, and county average income, respectively. In columns (4) and (5), we calculate the replacement rates relying on the Current Population Survey (CPS). Specifically, in column (4), we divide the average weekly UI income of the survey respondent by her average weekly wage. In column (5), we multiply the replacement rate that we obtain using CPS with the UI take-up rate.[25] Although not perfect either, these measures might be less prone to endogeneity concerns because it does not reflect UI extensions and, thus is less likely driven by local economic conditions. In line with our main results, all five alternative UI generosity measures have negative and statistically significant coefficients.[26]

Table 7. Deposits and UI Benefits: Within-Bank Estimation

	Dependent variable: Δlog(*Branch Deposit*)				
Variable	(1)	(2)	(3)	(4)	(5)
Δlog(*UIBenefit*), State	−0.092***	−0.076**	−0.079**	−0.080**	−0.079**
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
Δlog(*Income*), County			0.095*	0.092*	0.083*
			(0.048)	(0.049)	(0.048)
Unemployment	No	No	No	Yes	Yes
cubic(Unemployment)	No	No	No	No	Yes
Pair × year × bank fixed effects	No	Yes	Yes	Yes	Yes
County fixed effects	Yes	No	No	No	No
County × bank fixed effects	No	Yes	Yes	Yes	Yes
Pair × year fixed effects	Yes	No	No	No	No
Observations	38,616	38,616	38,616	38,616	38,616
R^2	0.281	0.679	0.679	0.679	0.680

Notes. 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. 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.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

The effect of UI benefits on deposits could be influenced by counties' sensitivities to national shocks, which might influence our results. This could happen, for instance, if states change their UI benefits in response to national shocks, and national shocks affect deposits. To investigate the impact of this possibility, we estimate two different county-level sensitivities to national shocks. For each county, we estimate a beta by regressing county-level income (county-level unemployment rate) on national-level GDP (national-level unemployment rate) and a linear time trend. In columns (1) and (4) of Table OA12, we interact income and unemployment rate beta with UI benefits, respectively. We would expect a significant coefficient for these interaction terms if the cyclicality plays a role in our results. Yet, neither of the interaction terms is significant. In the remaining columns, we split our sample into two with respect to these betas by using their median values. A significant difference between the coefficients would imply an important role for regional cyclicality. As reported in the last row of the table, the differences are not statistically significant. Therefore, we conclude that the cyclical differences across counties do not play an important role in our results.

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). Analyzing how UI influences stocks and bonds 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 the IRS data. The 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.[27] 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 OA13) and on stock holdings (Table OA14), suggesting that the decline in deposits is not offset by an increase in stocks or bonds.

Finally, a rise in UI generosity would influence 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 OA15). 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 importantly, the unemployment rate. Overall, these findings suggest that people are aware of the changes in UI benefits.

4. Lending Analysis

Thus far, we 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. Banks heavily rely on deposits for their funding, and they cannot perfectly replace deposits with other funding sources, hence we expect banks to squeeze their loan supply in response to an increase in their level of UI exposure (Stein 1998, Ivashina and Scharfstein 2010, Drechsler et al. 2017). 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 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 (Khwaja and Mian 2008, Amiti and Weinstein 2018).

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

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

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 and policy environment of the locations 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 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 8 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%.[28] 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 (Oster 2019). 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 and policy environment of the locations 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 the Online Appendix (Table OA16) 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 the decrease in deposits with external wholesale funding. Therefore, we expect that these banks squeeze their lending 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 3.3, 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.

Table 8. Small Business Lending and Bank UI Exposure: Within-County Estimation

	Dependent variable: log(*new lending*)			
Variable	(1)	(2)	(3)	(4)
Δlog(*UI Exposure*), Bank	−0.022**	−0.023**	−0.026**	−0.024***
	(0.010)	(0.010)	(0.010)	(0.009)
Bank controls	No	No	Yes	Yes
Bank exposures	No	No	No	Yes
Bank fixed effects	Yes	No	No	No
County × year fixed effects	Yes	Yes	Yes	Yes
County × bank fixed effects	No	Yes	Yes	Yes
Observations	364,643	364,643	364,643	364,643
R^2	0.396	0.645	0.650	0.654

Notes. 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.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

5. Real Effects

We conclude our analysis by testing whether the mechanism that we have identified 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 common in the UI literature, we focus on two labor market outcomes: the unemployment rate and the change in average wages. Because reduced 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 a county's UI exposure on its 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.[29] 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:

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

where $\Delta \log(\mathit{UI\,Exposure})_{c,y-1}$ 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.[30] 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 9 present the results for the unemployment rate. In column (1), we use the all-county sample and find a positive and significant coefficient. In 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%. 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 Section 4.

6. Discussion and Conclusion

UI policies have many benefits, such as smoothing household consumption during unemployment spells. However, UI policies also have unintended consequences, particularly in the labor market. In this paper, we uncover 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 our

Table 9. Real Effects and County UI Exposure

	log(*unemployment rate*)			Δlog(*average wage*)		
Variable	(1) All	(2) County DEF low	(3) County DEF high	(4) All	(5) County DEF low	(6) County DEF high
Δlog(*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)
State × year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
County bank exposures	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	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

Notes. 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.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 find in this paper are likely to be more prominent for European countries.[31] 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, because 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 differently. For example, recent evidence by Hsu et al. (2018) suggests that UI benefits reduce the default rate of the unemployed. Similarly, UI benefits are found to lower job search intensity and increase reservation wages for the unemployed. Unlike this literature, our results are unconditional; that is, UI benefits may affect employed individuals as well by reducing their deposits due to weakened precautionary saving motive. Therefore, the macroeconomic effects are likely to be stronger compared with the studies that base their analysis only on the unemployed, which form on average about 5%–6% 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 UI benefits change. By construction, this kind of methodology cannot say much 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 the approach that we take in Arslan et al. (2024).

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Endnotes

1 For the intended effects, see Gruber (1997), Hsu et al. (2018), Di Maggio and Kermani (2017), McKay and Reis (2016), and the U.S. Department of Labor's UI Directors' Guide. For the unintended effects, see Chodorow-Reich et al. (2019) and Hagedorn et al. (2013, 2015).

2 For the importance of deposits for banks' loan supply, see Ivashina and Scharfstein (2010), Hanson et al. (2015), and Drechsler et al. (2017).

3 In Table 2, we show that UI benefits tend to increase during times of high economic growth and low unemployment, suggesting that failing to control for economic conditions may attenuate our estimates, not enhance them.

4 These state-level variables are state income, state GDP, and state unemployment rate.

5 See for example Schmieder et al. (2012) and Kroft and Notowidigdo (2016).

6 See Engen and Gruber (2001), Gourinchas and Parker (2002), and Fuchs-Schündeln and Schündeln (2005) for the relationship between precautionary motive and household savings.

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).

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. 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 3.1.

9 The market share of banks in county small business lending is used as weights.

10 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 1(a); 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.

11 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.

12 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.

13 See Figure 1(b) for an example.

14 "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).

15 $((\$10.04 - \$6.66)/\$8.14) \times 0.056 = 2.3\%$.

16 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.

[17] Hagedorn et al. (2013) find larger effects. However, Coglianese (2015) (and several others) replicate the estimation of Hagedorn et al. (2013) and find that the results are sensitive to specification and time period. One potential explanation for finding small effects is that while a more generous UI lowers the search effort of the ones who receive it (hence increasing unemployment), it increases the job-finding probability of the ones who are unemployed but not eligible for UI (since eligible workers search less). As a result, in the aggregate, the level of unemployment rate does not change much (Farber and Valletta 2015, Lalive et al. 2015).

[18] The results of this exercise are available upon request.

[19] We perform an analysis in which we show that similar results hold in our sample as well. The results of this analysis is available upon request. Moreover, similar results are documented by others. For example, Kroft and Notowidigdo (2016) find from the U.S. data that the moral hazard cost of UI benefits is procyclical, that is, greater when unemployment rate is relatively low. Landais et al. (2018) review the literature (from the United States, Austria, and France) and argue that when UI becomes more generous, the increase in unemployment caused by lower search effort is partially offset by a reduction in unemployment caused by higher labor market tightness (positive elasticity wedge). Furthermore, the offset is large in bad times (when unemployment is high) but small in good times (when unemployment is low).

[20] 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.

[21] The median county has 311 million USD deposit holdings with a population of 26,240. Therefore, the calculation is as follows: (\$1/\$8.14) × 0.056 = 0.69% and (0.69% × \$311 mill.)/26,260 = \$82.

[22] These values are for 2004.

[23] Moreover, the findings that we report in Table OA13 and Table OA14 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.

[24] See Table OA9 for the balance table of the matching exercise.

[25] UI take-up rate is the share of unemployed people who actually receive UI benefits.

[26] Average income/wage enters in these models as the denominator of the replacement rates. Therefore, we do not control for income in these models. Furthermore, we do not include county-fixed effects in columns (4) and (5). This is because, in line with Di Maggio and Kermani (2017), we calculate the state-level replacement rate over a five-year period to keep the sample size reasonable for each state. Hence, including county fixed effects would eliminate much of the variation in replacement rate.

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

[28] When comparing the magnitudes of the decreases 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. Moreover, the share of deposits in bank balance sheets is much higher than that of small business lending. As small business lending is likely to be funded mainly by deposits due to agency problems (Berlin and Mester 1999), this difference in shares suggest that small business lending can decline more than deposits in percentage terms.

[29] The labor search literature discusses two types of effects: micro and macro. 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 (Hagedorn et al. 2013).

[30] 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.

[31] In a separate analysis (results not reported) we collect bank-level balance sheet information on the banks that are located in OECD countries. For the UI benefits measure, we use the OECD database and use country-level replacement rates. Confirming our findings from the US in this paper, we estimate a negative effect of UI on bank deposits. These results provide support for the external validity of our results.

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