The Impact of Public Pension Deficits on Households' Investment and Economic Activity *

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Abstract

US public state pension deficits are very large, accounting for 18.5% of an average state's GDP and up to 50% in Illinois. In principle, households should respond to this heavy future burden by increasing current savings, particularly in safe assets, since pension deficits are countercyclical. Comparing households residing on opposing sides of a state's border, I document that households in larger-deficit states save more, investing more in safe bank deposits and less in risky stocks. Specifically, households hold 0.70 dollars more in deposits and 0.33 dollars less in stocks for each additional dollar of pension deficit. This effect strengthened further following the implementation of new accounting standards in 2015 that made deficits more salient by requiring states to publicly disclose them. Exploiting staggered state pension reforms, I also find that households respond consistently when states reduce pension deficits; they decrease deposits and increase stock holdings. These reallocations spill over onto local economic activity: as households withdraw deposits following a pension reform, exposed local banks cut lending to local businesses.

Keywords: public pensions; pension reform; household savings; banking
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1 Introduction

Governments worldwide are grappling with public pension deficits due to aging populations, low birth rates, and a shrinking workforce. According to a Citi report on pension crises, the 20 OECD countries in its sample collectively bore \$78 trillion unfunded or underfunded government pension shortfalls in 2016, the equivalent of nearly 1.9 times those countries' gross domestic product (GDP).[1] In order to cover the accrued benefits and the additional contributions needed to reduce deficits, OECD countries divert on average 8% of their GDP each year to the public pension system. Studies have examined the drivers of public pension deficits (Novy-Marx and Coval 2011; Novy-Marx and Rauh 2011) as well as their effects on aggregate household savings (Attanasio and Rohwedder 2003; Lachowska and Myck 2018), macroeconomic consequences (Feldstein 1974; Karam et al. 2010), and fund managers' portfolios (Andonov, Bauer, and Cremers 2017; Lu et al. 2019). Yet as Scharfstein (2018) stated in his 2018 American Finance Association (AFA) presidential address: "while abundant academic literature studies the effect of pension policy on savings, there is little theory and evidence on how pension policy affects the way the financial system transforms savings into investment." This question is important because households' savings decisions play an essential role in resource allocation and economic growth.

This paper contributes by (i) providing causal evidence of how pension deficits and reforms alter households' savings allocation between bank deposits and stock investments and (ii) investigating the implications for banks' balance sheets and the real economy.

According to theory, public pension deficits induce households—to the extent that they are forward-looking—to shift their savings from risky stocks to safe bank deposits.[2] Indeed, households

[1] For the same 20 OECD counties, the reported amount of national debt (excluding unfunded public pension liabilities) on government balance sheets totals only 109% of GDP, far less than the shortfalls in the public pension system.

[2] Public pension plans cover public workers only, so their deficits do not directly affect the pension income of the general public, to which I refer as "households." This paper argues that households react to public pension deficits because they internalize the government's budget constraint. Moreover, the paper studies bank deposits because they are the most prominent form of safe assets. According to the 2016 Survey of Consumer Finances, 98% of US households

anticipate that larger public pension deficits will lower their future disposable income (i.e., after-tax income minus necessary expenses) and hence consumption; this is because governments will have to reduce the provision of public services or raise taxes to cover funding shortfalls.[3] In order to smooth consumption over time, households consume less and save more today. This, in turn, leads households to tilt their portfolio towards safe deposits through (at least) two distinct channels. The first channel relies on the countercyclical nature of pension deficits. Since public pension plans promise fixed payouts and invest mostly in the stock market, their deficits surge in a market downturn. Households hedge against such countercyclical payout exposure by shifting their savings from risky stocks to safe deposits. The second channel is based on the well-documented fact that absolute risk aversion decreases in consumption.[4] Accordingly, the drop in current consumption triggers an increase in risk aversion and hence a reallocation of savings towards safe assets. Therefore, the theory suggests that an economy with higher public pension deficits should exhibit more bank deposits and fewer stock holdings.

I test this hypothesis by focusing on the US public pension system, a unique "laboratory" to investigate the research question for two reasons. First, nearly all public employees (about 13.8% of the US workforce) participate in *defined-benefit* pension programs offered by state and local governments.[5] According to the US Federal Reserve, the total promised obligations (discounted to present values) surpassed the total market value of assets held in pension plans by $4 trillion in

held bank deposits, but fewer than 10% invested in bonds.

[3] This mechanism relies on households' forward-looking behavior, but it does not require a one-to-one relationship between tax cuts and the increase in savings implied by Ricardian equivalence. Although the empirical findings on Ricardian equivalence are inconclusive, researchers report consistently that households do optimize intertemporarily; see Aschauer (1985); Evans (1988); Haug (1990), among others. The main goal of this paper is not to examine Ricardian equivalence, but my empirical findings suggest that households internalize the government's budget constraint and reallocate savings in the face of public debt.

[4] Preferences with constant or decreasing relative risk aversion (CRRA or DRRA) also display decreasing absolute risk aversion (DARA). The evidence on households' CRRA utility is documented by, for example, Morin and Suarez (1983) and Ogaki and Zhang (2001).

[5] Retirement plans are of two main types. Defined-benefit plans guarantee pensioners' income at retirement. They are in deficit if their assets are insufficient to cover guaranteed payments. In contrast, defined-contribution plans offer no income guarantee, and so are not subject to deficits. Public pensions are typically defined-benefit plans.

2017—even after a decade-long bull equity market. Second, the size of pension deficits varies as a function of the individual states' respective fiscal policies and budget allocations. The ratio of state public pension deficits to state GDP ranges from 4% in Wisconsin to more than 50% in Illinois. This sizeable variation across states provides opportunities to empirically identify the effects of public pension deficits.

To start with, I examine the effects of public pension deficits on households' allocation of savings in three steps. First, I exploit the cross-sectional variation in public pension deficits across US states using panel regressions. State-level deposits (or stock investments) and public pension deficits are naturally endogenous to the corresponding states' economic conditions.[6] To address this omitted variables problem, I restrict the analysis to contiguous interstate pairs of counties that share borders (hereafter "contiguous border counties") and hence can be presumed to have similar economic conditions. An additional concern for analyzing bank deposits is that banks in booming states with smaller pension deficits can—if they have better lending opportunities—offer higher deposit rates to attract more deposits. To control for this effect, I conduct the analysis at the bank branch level while controlling for bank fixed effects. Summarily, for bank deposits, I compare the deposits at different branches of the same bank in contiguous border counties; while for stock investments, I compare the dividends reported in tax filings in contiguous border counties.[7]

The empirical findings are consistent with the prediction that households in states with larger pension deficits invest more in bank deposits and less in stocks. If a state has a 1% larger pension deficit than a contiguous state, then a bank branch sees 1.77% higher deposits than the same bank's other branches in a contiguous border county, while dividends at the county level are 0.55% lower. In dollar terms, a $1 increase in the public state pension system's deficit translates into (on average)

[6] Suppose, for example, that state pension funds invest a disproportionate amount in local projects (i.e., they display a "home bias"). Weak state economic conditions can simultaneously lead to large pension deficits and debilitated economic activity, which in turn give rise to low bank deposits and low stock investments. Failing to control for economic conditions could underestimate the relationship between pension deficits and bank deposits (or stock investments).

[7] The within-bank and contiguous border county identification strategies are carried forward for the next two steps.

a \$0.70 increase in retail deposits and a \$0.33 decrease in stock holdings.

In the second step, I emphasize the importance of households' *perceptions* of public pension deficits in determining their savings allocation by exploiting changes in rules enacted by the Governmental Accounting Standards Board (GASB). Before 2015, GASB Statements No. 25 and No. 27 left a "gray area" whereby state governments could cover up their pension deficits (see e.g., Rauh and Novy-Marx 2009; Naughton, Petacchi, and Weber 2015; Andonov, Bauer, and Cremers 2017). The resulting obfuscation could mislead households into underestimating the severity of those deficits, hence over-investing savings in stocks and under-investing in bank deposits. The new GASB Statements No. 67 and No. 68, effective 2015 across US states, updated the guidelines and mandated the presentation of a more accurate picture of state public pension deficits. I take these GASB updates as a quasi-exogenous shock to households' assessment of state public pension deficits and posit that households—with this more accurate information—rebalanced their savings allocation optimally. This hypothesis is supported by a difference-in-differences (DiD) analysis. After the GASB updates, households saved additional 0.23% more in deposits and additional 0.07% less in stocks for a 1% increase in pension deficit, compared to periods under the old rules. Moreover, the estimated upper bound for households' miscalibration of pension deficits *before* the GASB updates was 17%.

The third step consists of examining households' reaction to an *actual reduction* in pension deficits. To do so, I exploit the staggered adoptions of public pension reforms by state governments in attempting to lower their pension deficits. Since the timing of reforms does not exactly coincide with other state policy changes, a household's reallocation of its savings during a one-year reform window can be mostly attributed to that reform. This feature also helps rule out confounding factors due to time-varying state fiscal policies that might have confounded the panel analysis in the previous steps. I first confirm that reforms reduced state pension deficits. According to the theory, a reduction in deficits should encourage households to shift their savings from banks deposits to stocks. This conjecture is supported by the data. Following reforms, bank branches in pension-reforming states experience 1.22% more deposit outflows, on average, than branches of the same bank on the other side of the border in a non-reforming state. In contrast, the dividends reported in tax filings increase by 2.54%, compared to non–pension-reforming contiguous border counties.

This three-step analysis establishes that households shift their savings from bank deposits to stocks after a state undertakes reforms that reduce public pension deficits. This result offers a new perspective on how pension reforms affect the way that the financial system transforms savings into investment. The extant literature focuses on the capital market and thus tends to ignore the role of banks as a financial intermediary transmitting pension-related shocks. My results highlight the influence of pension reforms on households' bank savings. As I show next, their withdrawals (or deposits) generate non-negligible economic effects that differ fundamentally from those manifested via the capital market.

Accordingly, I trace deposit outflows resulting from pension reforms and examine their subsequent effects on banks' balance sheets and on economic activity. Starting with banks' balance sheets, I examine whether banks with more branches located in pension-reforming states experience greater reductions in liabilities. When a state enacts a pension reform, which reduces deficits by an average of \$1.87 billion, banks lose (in aggregate) \$1.02 billion in total deposits, of which \$0.91 billion correspond to retail deposits. To avoid cutting profitable investments, banks raise external capital but their equity proceeds—\$0.15 billion on average—cover only a small fraction of their losses in deposits. Hence bank assets shrink by \$1.01 billion, of which \$0.25 billion reflect reduced commercial and industrial (C&I) loans while most of the rest reflects curtailment of securities holdings.

The above finding is subject to the omitted variables concern that reforms may alter banks' lending opportunities. I address this issue by comparing (following Drechsler, Savov, and Schnabl 2017) small business lending, a category of C&I loans, across different banks within the same county. Also, I examine only the changes in loan activity observed in *non*–pension-reforming states to avoid any confounding bias that could result from reform-induced changes in fundamentals. The empirical tests establish that, when a state passes a pension reform, banks reduce (in aggregate) their lending to small businesses by \$0.47 billion in *non*–pension-reforming states.[8] In short: pension reforms in one state impose—through households' savings reallocation—externalities on other states.

These externalities are non-trivial for real economic activity in non–pension-reforming states. The lending downturn due to concurrent pension reforms in other states leads to layoffs and reduced wages in non-tradable sectors as well as to a decline in the number of new business establishments. I quantify these negative externalities by adopting an instrumental variables (IV) framework. I instrument county-level lending using a county's exposure to pension reforms passed in *other* states and then examine how that exposure is related to county-level real activities. According to these IV regressions, a reduction of \$108,784 (resp., \$11,899) in lending results in the shuttering of one business establishment (resp., the layoff of one worker in a non-tradable sector). Using these estimates, I calculate the economic effects of pension reforms: on average, a single enactment of pension reform that reduces pension deficits by \$1.87 billion leads to \$0.47 billion worth of cuts in small business lending in non–pension-reforming states; this figure translates to 39,498 layoffs (a 0.4% increase in unemployment) and 4,320 fewer business establishments (a 0.1% decrease in total establishments) in those states. These effects are economically sizable, considering that 58 pension reforms have been enacted in the past decade.

Overall, I find that pension deficits and reforms can alter households' allocation of savings between bank deposits and stocks. Such behavioral changes have economically significant effects on banks' balance sheets and the real economy. Pension reforms, which reduce state public pension deficits and improve state fundamentals, encourage local households to shift savings from bank deposits to the stock market. These deposit withdrawals force exposed local banks to cut lending to local businesses. These findings highlight an unexpected effect of pension reforms and call for

[8] The \$0.47 billion lending cuts in non–pension-reforming states surpass \$0.25 billion reductions in C&I loans—referenced in the previous paragraph. This result arises from the re-allocation of credit from non–pension-reforming states to pension-reforming ones.

coordinating pension reforms with monetary and banking authorities in order to limit fallout from the resulting credit contraction.

Literature and contribution This paper contributes to three strands of research. First, it fills a gap in the literature on the relationship between pension wealth and savings allocation. Scholars have long attempted to assess the effects of public pensions on aggregate savings—for example, in the context of Italy's 1992 pension reform (Attanasio and Brugiavini 2003), three UK reforms (Attanasio and Rohwedder 2003), and Polish reforms (Lachowska and Myck 2018); see Attanasio and Weber (2010) for a review. These studies all focus on households' aggregate savings as approximated by post-expenditure income. In contrast, I decompose aggregate savings into bank deposits and stock holdings, and examine households' re-allocation of savings between the two in response to pension deficits and reforms.

Second, the extant literature concentrates on the effects of pension reform through the capital market. I contribute by stressing the banking sector's role as an alternative channel. Niggemann and Rocholl (2010) document a surge in stock and bond issuance following reforms that increased pension fund assets. Giannetti and Laeven (2009) similarly find an increase in equity holdings and an improvement in firm monitoring after reforms had been enacted. Greenwood and Vayanos (2010) provide evidence that pension funds began to purchase long-term assets after the 2004 UK pension reform, which instituted fines for underfunded pension plans. With the notable exception of Mehran et al. (2018), there is scant work addressing the alternative channel of banking. Those authors examine the impact of legislated pension reductions in two states, Wisconsin and Ohio, on banks' assets. They report a drop in municipal bond spreads and a subsequent increase in credit supplied by local banks. I differ from Mehran et al. (2018) by studying the impact of reforms (i) operating through banks' liabilities, (ii) on non–pension-reforming states, and (iii) by dealing with the endogeneity of bank lending.

My work connects with the AFA presidential address of Scharfstein (2018), who raises the question of "how pension policy affects the way the financial system transforms savings into investment." Scharfstein compares two types of national pension policies—the pay-as-you-go (PAYGO) scheme and the private savings scheme—and suggests that pension policy choices can determine the financial system's structure. In particular, PAYGO pension schemes promote a bank-centered system whereas a private savings scheme promotes development of the capital market. He provides supporting evidence by comparing countries with different retirement systems. However, the empirical analysis suffers from a reverse causality problem in that a country's pension system might itself be determined by its financial market's development. My research complements Scharfstein's work by offering identified empirical evidence on this question. Although I examine pension deficits rather than pension policy, the two are fundamentally related.

Third, my paper contributes to the active debate on public pension deficits. Since Brown and Wilcox (2009) and Novy-Marx and Rauh (2011) document the considerable underfunding problem in the US public pension system, a large number of studies have attempted to understand the consequences of pension deficits. Pennacchi and Rastad (2011), Naughton, Petacchi, and Weber (2015), and Andonov, Bauer, and Cremers (2017) point out that the accounting standard prior to 2015, which advised liabilities to be discounted by the expected return on assets, incentivized pension fund managers to invest in riskier portfolios and to report a smaller funding gap. Andonov, Rauh, and Hochberg (2018) argue that political expediency affects the performance of public pension plans. Other research shows that large public pension deficits are associated with high municipal yields (Lekniūtė, Beetsma, and Ponds 2019), negative swap spreads (Klingler and Sundaresan 2019), and low housing prices (Grace 2020). My study documents the understudied *negative* externalities of pension reforms on non–pension-reforming states, including wage reductions, layoffs, and the closure of business establishments. Regulators should account for these negative externalities when designing policy interventions.

The rest of this paper proceeds as follows. Section 2 presents background information on state public pension plans and reforms. Section 3 describes the data. Section 4 examines households' savings allocation between bank deposits and stocks in response to state pension deficits and reforms. In Section 5, I explore how pension reforms affect banks' balance sheets and economic activity. Section 6 concludes.

2 Background

2.1 State and Local Public Pension Plans

It has been more than a century since the first state public employee retirement system was established (in Massachusetts) in 1911. As of 2018, there were more than 5,000 state and local public pension plans in the United States that covered 21 million public workers: 14.6 million active employees (about 13.8% of the US workforce) and 6.5 million inactive ones. Although the number of locally administered pension plans (5,123 in 2018) far surpassed the number of state-administered plans (297 in 2018), more than 80% of local assets are managed by the state-administrated system (Urban Institute 2018). It follows that the state is an appropriate unit of analysis for the study of public pension plans.

The defined-benefit pension plan is the type most commonly adopted in the public sector. According to the US Bureau of Labor Statistics, more than 86% of public workers were participating in such plans during 2018. Defined-benefit plans pre-specify retirement payments based on employees' earnings and years in the workforce. The payments are sourced both from employers' and employees' contributions and from the investment returns on collected contributions. The ideal scenario is one in which the value of a pension plan's assets grows enough over time such that public employees' pre-defined pension payments can be made. Otherwise, the state or local government entity is responsible for covering any gaps. To resolve deficits, the government either increases its income (by raising tax rates) or reduces its expenses (by cutting the budget for public services). As a result, all taxpayers and residents in the state bear the burden and must collectively compensate for the shortfall when state public pension plans are underfunded.

There was a long time during which no one paid much attention to the funding status of public pension plans. An alarm eventually rang when Novy-Marx and Rauh (2011) reported \$4.43 trillion in unfunded liabilities for these plans—equivalent to 20%–30% of US GDP. The problem worsened in the years that followed, and media argued that unfunded public pensions could trigger an unprecedented credit crisis.[9]

What caused such big trouble for public pension plans? The 2008 financial crisis was the first culprit. In 2007, one year before that crisis, the aggregate underfunding of public pension plans amounted to 9% of GDP; that proportion skyrocketed to more than 20% in 2008.[10] Furthermore, previous state governors had not set aside enough contributions for making the payments promised to current and future retirees, which led to a gradual increase in pension deficits over time.[11] That mistake is being magnified by retirements among the Baby Boomer generation; retirement of the largest generational cohort in history places an extra burden on the fragile public pension system.

Once warned about such substantial pension deficits, states began to ramp up contributions in an attempt to close the underfunding gaps. Government spending diverted to pension plans, as a share of GDP, grew from 3.4% in 2008 to 4.7% in 2017 (NASRA). Some states, including Illinois and Connecticut, spent around 10% of their budgets on public pension plans. When more capital is allocated to a pension system, fewer resources are available to finance investment in local infrastructure, social needs, and public services. Hence the problem of underfunded pubic pensions affects not only the public workers covered but also society in general.

[9] See, for example, Forbes news, Financial Times news, and Barron's news.

[10] Public pension plans invested heavily in equities prior to the crisis: according to a report by Pew Research Center, 61% of plan portfolios in 2006 consisted primarily of equities. The financial crisis wiped out almost \$8 trillion in stock market wealth. The value of public pension plan assets plummeted, which doubled the underfunding gaps. Public pension plans did not fully capitalize on the decade-long bull market because many moved away from equity investment during that period. As reported by PEW, the average proportion of equity-based assets in pension plan portfolios declined to 48% in 2016.

[11] Each year, state pension actuaries must report the total costs of covering accrued benefits as well as the additional contributions needed to shore up underfunded plans. However, those latter contributions are not actually the state's legal responsibility. Many states have chosen to delay such contributions, but they cannot delay forever. The "Edgar ramp" is an illustrative example. In 1994, Illinois Governor Jim Edgar signed a 50-year plan that aimed to resolve the state's \$15 billion pension deficit. Yet the plan did not stipulate any means of doing so, and Edgar pushed the pension obligations far off into the future. After 20 years, the \$15 billion deficit had snowballed to \$479 billion, leaving Illinois as the state with the most public pension debt.

2.2 State Public Pension Reforms

In addition to allocating more government funds to make up for the existing pension deficits, states also tried to enact pension reforms to reduce future liabilities. These reforms were intended to balance the interests of public employees and taxpayers. Almost all states have legislated some changes to their public pension systems in recent years. Most early changes were modest and applied only to future employees. Over time, reforms increased both in both scale and aggressiveness—extending their provisions to current employees and retirees.

The most common reforms include benefit reductions and contribution increases. Benefit reductions take a variety of forms: increasing the time in service required for vesting of benefits, raising age and service requirements, and eliminating cost-of-living adjustments. More than 70% of the reforms between 2009 and 2018 included clauses that reduce benefits for current workers and/or new hires. Based on the report by NASRA, reforms passed between 2009 and 2013 cut benefits for new employees by 1%–20%. At the same time, employees were required to increase their contributions in increments whose size depended on whether or not they were also covered by Social Security. For those in non–Social Security states, the median contribution rate has increased to 8%; for individuals protected by Social Security, that rate has risen from 5% to 6%.

The reform process was thorny. More than half of the states faced lawsuits claiming that the reforms violated the contractual nature of pensions. There was variation in the obstacles encountered by different states: what was endorsed in one state might be struck down in another, which made the result of reforms uncertain. For instance, a contribution increase was quickly passed in Florida but was adjudged illegal in Arizona; and a cost-of-living adjustment was deemed constitutional in Colorado yet was rejected in Oregon. Each state's legislature faced different challenges in balancing public retirement security and the economic efficiencies sought by the general public.

3 Sample and Data

3.1 Data

State public pensions. The funding status of state and local government employee defined-benefit retirement plans for the period 2009–2018 is extracted from the Federal Reserve.[12] The Federal Reserve made the data available for the first time in February 2016. Pension deficits are quantified as the difference between the present value of total liabilities and total assets, scaled by state GDP:

$$\text{(1)} \qquad \text{Pension Deficits}^{\text{GDP}} = \frac{\text{PV of Pension Liabilities} - \text{PV of Pension Assets}}{\text{State GDP}}.$$

The value of pension assets is derived from the US Census Bureau's Annual Survey of Public Pensions and is marked-to-market. The value of pension liabilities is collected from the Bureau of Economic Analysis (BEA). The present value of future obligations is discounted using the interest rates on AAA-rated corporate bonds; see FEDS note for details. Thus, the measure of pension deficits accurately reflects the status of state pension plans and is free from local government manipulation.

State public pension reforms are recorded by the National Association of State Retirement Administrators (NASRA). For each reform since 2009, I manually collect information about the year of reform passage, the chief modifications made, and the group of workers affected. The pension reforms studied are listed in Table OA.17 of the Online Appendix.

Deposit holdings. Branch-level deposits are extracted from the Summary of Deposits database of the Federal Deposit Insurance Corporation (FDIC). These data record the branch office deposits as of June 30 each year for all FDIC-insured institutions.

[12] The website provides downloadable data from 2002 to 2017. Data for 2018 is from the Census and the BEA. My sample is restricted to the 2009–2018 period for two reasons. First, pension deficits of state public pension plans were relatively low—and so did not garner much public attention—prior to the financial crisis. As shown in Panel (a) of Figure 1, the average pension deficit (as a share of GDP) was about 9% before 2009 but jumped to 20% after 2009. Second, households' savings behavior may have changed in the era of low interest rates that followed the financial crisis. Hence I focus on the post-crisis period. However, the main results are robust to extending the sample to encompass 2002–2018.

Deposit rates. The interest rates paid on branch-level deposits are obtained from RateWatch, which provides weekly deposit rates on products that include certificates of deposit (CDs), money market accounts, and business savings. The data are aggregated to the yearly frequency by averaging the deposit rates for each product of each branch.[13] I select the most popular products offered across all US branches. The following abbreviations are used: INTCK0K references an interest-bearing checking account; MM25K, a $25,000 money market deposit account; and X-MCD10K, a $10,000 X-month certificate of deposit account.

Bank data. Bank-level data are collected from US Call Reports provided by the Federal Reserve Bank of Chicago. I use year-end data on the balance sheets of all US commercial banks. Some balance sheet variables are missing from the data set after year 2013. Missing end-of-year observations are filled with averages of the previous (non-missing) quarter's values.[14]

Small business lending data. Data on small business lending by US banks are obtained via the Community Reinvestment Act (CRA) of the Federal Financial Institutions Examination Council (FFIEC).[15] These data contain the total dollar value— for each US commercial bank in each county and for each year—of new loans of less than $1 million and of new loans granted to small businesses with gross annual revenues of less than $1 million. Details on the loan terms are not disclosed, so

[13] As of 2018, RateWatch covered only 54% of all US branches. One should bear in mind that not all covered branches have the authority to set their deposit rates; some branches follow the rates set by nearby branches belonging to the same bank. RateWatch provides detailed information on whether a branch actively sets product-specific rates. To preclude duplicate observations, the analyses presented here are based on those branches that actively set their own deposit rates.

[14] Call Reports contain detailed on– and off–balance sheet information. I use total deposits (Bank Regulatory item RCFD2200), total assets (RCFD2170), total liabilities (RCFD2948), and total equities (RCFD3210). Retail deposits are the sum of time deposits of less than $100,000 (RCON6648), savings deposits (RCON2389), and demand deposits (RCON2210). Wholesale funding is defined as the sum of brokered deposits (RCON2365), foreign deposits (RCFN2200), other borrowed money (RCFD3190), and federal funds purchased (RCONB993 + RCONB995). Most loan information is available only through 2013, so instead I use last-quarter averages for loans (RCON3360), C&I loans (RCON3387), real estate loans (RCON3385), and personal loans (RCONB561 + RCONB562). Securities are the sum of securities held to maturity (RCFD1754) and securities available for sale (RCFD1773). Data on securities also end in 2013. The variables are constructed following the procedure outlined by Philipp Schnabl.

[15] Starting in 2005, only those banks with assets in excess of $1 billion were required to disclose information on small business lending.

I cannot analyze effects on lending rates.

Dividend data. Following Lin (2020), I use the aggregate dividends reported by the Internal Revenue Service (IRS) to proxy for stock investments at the county level. Because they incorporate information from all individual income tax returns filed with the IRS, these data are the most comprehensive available. I adopt the sample from 2010 to 2017.[16]

County data. I obtain contiguous county pairs along state borders from the US Census Bureau County Adjacent file. There are a total of 1,308 unique county pairs. All analyses exclude Alaska and Hawaii because they do not share a border with any other state.[17]

County characteristics—including GDP, population, employment, and per capita income—are collected from the BEA. Variables pertaining to real economic outcome are obtained from the Quarterly Census of Employment and Wages; these variables include the annual average of quarterly business establishment counts and of monthly employment.

3.2 Summary Statistics

Table 1 presents summary statistics for the key variables. To avoid the influence of outliers, all variables (expect for pension deficits and county characteristics) are winsorized at the 0.5% quantile from both tails of the distribution. For each variable, the mean and standard derivation are reported for the full sample and also for subsamples based on a split of each year's median pension deficit.

Panel A of Table 1 reports the funding status of US state public pension plans for the period 2009–2018. Pension deficits, or the differences between liabilities and assets, account for 18.5% (on average) of state GDP. There is considerable variation in pension deficits across states, from only 4.4% in Wisconsin to more than 50% in Illinois. Moreover, pension plans are larger in the states with greater pension deficits than in those with lesser pension deficits: the respective averages

[16] In 2009, the US stock market experienced the greatest reduction in dividends since 1938. Total dividends reported were around \$105 billion in 2009, merely 60% (resp. 75%) of that reported in 2010 (resp. 2008). I therefore exclude the abnormal year 2009 when analyzing dividends.

[17] Figure OA.1 in the Online Appendix illustrates the locations of contiguous counties along the state borders.

of assets under management are $91.5 billion and $52.6 billion. Panel (b) of Figure 1 plots average state pension deficits for the 2009–2018 period.

Panel B presents the summary statistics of county characteristics. As compared with counties in states with small pension deficits, counties in large–pension-deficit states have on average a higher GDP ($6.8 billion vs. $4.4 billion), a larger population (124.4 thousand vs. 84.0 thousand), more employment (71.8 thousand vs. 49.6 thousand), a greater value of bank deposits ($2.77 billion vs. $1.98 billion), and more reported dividends ($79.7 million vs. $51.1 million). Large–pension-deficit states also have relatively lower income per capita: $37.2 thousand versus $39.9 thousand. These statistics imply the existence of hidden variables—such as economic conditions—that jointly affect pension deficits, bank deposits, and stock investment. Hence the omitted variables problem poses a challenge to identification.

Panel C reports summary statistics for branches' deposits from the FDIC database. Branches in large–pension-deficit states hold slightly more deposits ($72.0 million vs. $64.1 million) than branches in small–pension-deficit states, but there is no significant difference vis-à-vis (logged) changes in deposits.

Finally, Panel D gives—for branches that actively set rates—summary statistics for the deposit rates of various products.[18] These branches are almost three times larger than the average branch reported in Panel C. For short-term deposits (e.g., MM25K), there is no apparent difference in rates set by branches located in large– versus small–pension deficit states. Yet the difference is increasing in the maturity of deposits, so deposit rates offered in large–pension-deficit states are lower.

[18] Since not all branches offer or actively set rates for certain types of deposit products, there are slight differences in the number of observations for different types—ranging from 62,896 for 60MCD10K to 75,862 for 12MCD10K. The number of observations reported in Panel D is for the INTCK0K product.

4 Effect on Households' Savings Allocation

This section presents an empirical examination of how households, in response to state public pension deficits and reforms, allocate their savings between bank deposits and stocks. After providing a conceptual framework to guide the empirical analysis, I conduct the analysis in three steps. In the first step, I exploit the cross-sectional variation in pension deficits across US states by running panel regressions that use the "contiguous border county" strategy. In the second step, I emphasize the importance of households' perceptions of public pension deficits in determining their savings allocation by exploiting the GASB updates. In the third step, I examine households' reaction to an actual reduction in public pension deficits by analyzing staggered adoptions of state public pension reforms.

4.1 Conceptual Framework

I guide the empirical analyses via a standard portfolio choice framework. The framework predicts that an increase in public pension deficits leads households to tilt their savings allocation towards bank deposits and away from stocks.

As mentioned in Section 2, even though public pension plans cover public workers only, their deficits are ultimately financed by state budgets. To set aside funding for pension shortfalls, the state government can either raise tax rates or lower government spending. An increase in the tax rate directly reduces households' after-tax income. Cutting government spending likely increases the cost of necessities, such as schooling and medical services; in that case, even though labor income remains constant, its disposable component is reduced. Therefore, if households are forward-looking and can internalize the government's budget constraint, they, in the face of larger public pension deficits, will anticipate a reduction in their future disposable income (i.e., after-tax income minus necessary expenses). In order to smooth consumption, households consume less today and save more for the future. This, in turn, induce households' to save more via safe bank deposits and less via risky stocks.

In the following, I provide three channels to justify such savings reallocation. The first channel relies on the countercyclical nature of pension deficits. Public pension plans promise fixed payouts and invest mostly in the stock market. In a market downturn, lower returns from investments widen the deficits and vice versa. To hedge against such countercyclical payout exposure, households allocate their savings from risky stocks to safe deposits. The second channel is based on well-documented fact that households exhibit decreasing relative risk aversion (Ogaki and Zhang 2001). Accordingly, households become more risk averse after consumption reduction and hence reallocate savings from risky stocks towards safe bank deposits.[19] The third channel sits on the observation that disposable income is relatively safe, and hence can be viewed as a substitute for safe deposits.[20] It follows that an increase in public pension deficits leads to a decrease in households' disposable income and thus to a disproportionately high share of risky stocks in portfolios. To re-optimize their portfolio, households reallocate savings from stocks to bank deposits. Therefore, an economy with higher public pension deficits should exhibit more bank deposits and fewer stock holdings.

4.2 Cross-sectional Evidence

My objective is to investigate whether households living in states with larger public pension deficits save more via bank deposits and less via stock holdings. I start by addressing challenges in identification and then presenting cross-sectional evidence for bank deposits, stock investments, and deposit rates.

[19] Campbell (2017) presents results with a model where households have CRRA and stock returns are log-normally distributed. In Online Appendix A, I generalize the result to any utility function with DARA and any distribution of stock returns. Utility functions with CRRA and DARA belong to the DARA utility family.

[20] A big part of disposable income is labor income, which is relatively safe compared to risky stocks (see Campbell 2017, sec. 10).

4.2.1 Identification Strategy

The challenge of obtaining *unbiased* estimators is rooted in a potential omitted variables problem. I hereby elaborate on two types of omitted variables. First, recall from Table 1's summary statistics that GDP, population, and employment are all positively correlated with both pension deficits, bank deposits and dividends. If there are non-identified omitted factors that simultaneously determine a state's pension deficits and households' allocation of savings, then the estimated relationship between the two could be biased. For instance: if public workers are relatively more risk averse and thus hold more safe assets and fewer risky assets than do other workers, then states with a larger proportion of public employees would have greater pension liabilities, larger bank deposits, and fewer stock investments. Hence the relationship between pension deficits and bank deposits (resp, stock investments) would be overestimated (resp., underestimated) unless I control for the share of public employees among all households.

The second type of omitted variable is unique to bank deposits: such deposits are simultaneously determined by banks' lending opportunities (i.e., demand for deposits) and households' supply of deposits. Banks with more branches in booming states have access to better lending opportunities, and so might seek to attract more deposits by offering higher deposit rates. Since these booming states are likely to have smaller pension deficits, the estimated relationship between pension deficits and bank deposits will be biased upward. It is therefore necessary for the analysis to control for banks' lending opportunities. In what follows, I propose two identification strategies aimed at overcoming the aforementioned econometric challenges.

Contiguous border county strategy. Public pension deficits are measured at the state level. In order to rule out common drivers of state pension deficits and households' savings, I compare counties that have similar demographic and economic characteristics but are located in different states. This approach is equivalent to adding observed and unobserved confounding factors as control variables.

There are two reasons why contiguous border counties provide a natural laboratory for the analysis.[21] First, contiguous border counties tend to share similar demographic and economic characteristics yet exhibit sufficient heterogeneity in pension deficits. To support this argument, I compare a range of observable measurements for contiguous border county pairs during the sample period: GDP, population, employment, per capita income, public employment, and population under age 30; see columns (1)–(6) in Table 2. There are no significant differences in these aspects, even though the contiguous border county pairs have an average difference of 9.58% in state pension deficits (column (7)). It is worth noting that contiguous counties may share similarities also along dimensions that are hard to measure, e.g., culture, risk preferences, and common values.

Second, households' saving behaviors in border counties are affected by their own states' public pension health but the reverse is not likely to hold given the small size of a county. As mentioned in Section 2, local households are influenced by state public pension deficits because state governments can adjust state taxing policies and thereby affect the budget allocations of county governments.[22] Nevertheless, the saving behavior of households in border counties is not, in itself, likely to induce extensive changes in states' pension conditions and fiscal policies. It follows that reverse causality is less of a concern when focusing on contiguous border counties.

I apply this strategy to the analysis of bank deposits and stock investments. Toward that end, I consider two types of contiguous border counties: one based on Combined Statistical Areas (CSAs) and the other based on county pairs along state borders (as listed in the Census Bureau's County Adjacent file).[23]

[21] The empirical design of using contiguous border counties has been widely adopted in the economics and finance literature. See, for example, studies on US bank branching deregulation (Huang 2008), minimum wage regulations (Dube, Lester, and Reich 2010), and foreclosure regulations (Mian, Sufi, and Trebbi 2015).

[22] According to the Tenth Amendment, a state government can decide how much authority is granted to local governments. In addition, Dillon's Rule (which is followed by 39 states) stipulates that a local government can engage in only those activities that are authorized by its state government (National League of Cities 2016).

[23] According to the Office of Management and Budget, "a CSA consists of two or more adjacent metropolitan and micropolitan statistical areas that have a high degree of social and economic integration and have an employment interchange measure of 15 or more." A CSA covers adjacent counties with social and economic connections, and the counties in question need not be located in the same state. In 2018, 41 of 169 CSAs contained adjacent counties located

Within-bank strategy. I use a within-bank strategy to deal with the problem of omitted lending opportunities. Households usually choose to deposit in bank branches located close to where they live. Banks then aggregate deposits collected from different branches and allocate these funds to projects that promise high returns irrespective of the projects' locations. That is to say: (i) all branches of the same bank share, in effect, the same lending opportunities; and (ii) the variation in branch deposits is due primarily to local deposit supplies. This feature allows me to isolate the supply of deposits from the demand for deposits by analyzing deposits in different branches belonging to the *same* bank. This approach is widely used in the banking literature (see e.g. Drechsler, Savov, and Schnabl 2017; Skrastins et al. 2019; Lin 2020).

4.2.2 Results on Bank Deposits

In analyzing deposits, I combine the contiguous border county strategy and the within-bank strategy in the following regression specification, which quantifies the relationship between state pension deficits and bank deposits:

$$\log(\text{Deposits}_{i,b,c,t}) = \alpha_{b,\text{csa},t} + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} + \textit{Controls}_{c,t} + \varepsilon_{i,b,c,t}; \tag{2}$$

$$\log(\text{Deposits}_{i,b,c,t}) = \alpha_{b,\text{cp},t} + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} + \textit{Controls}_{c,t} + \varepsilon_{i,b,c,t}, \tag{3}$$

where $\text{Deposits}_{i,b,c,t}$ denotes deposits at branch i of bank b located in county c at the end of June in year t; $\text{Pension Deficits}^{\text{GDP}}_{s,t}$ represents the pension deficit in state s in year t, as defined by Equation (1); and the *Controls* include county-level GDP and per capita income, both in log scales.[24] To control for the supply effect, I interact bank × year fixed effects with CSAs ($\alpha_{b,\text{csa},t}$)

in multiple adjacent states. The County Adjacent file lists all the border counties and their contiguous counties across such borderlines; there are 1,308 such county pairs. These contiguous border counties represent 37.11% of total GDP and 37.43% of total population of the US. The main distinction between the two samples is that a single county can appear in multiple county pairs along one border segment but can only appear once in a CSA region; in other words, there are "mechanically" repeated entries in the contiguous border county pair sample. The methodology employed to deal with this issue is discussed later.

[24] Population and employment are not included as control variables because they are strongly correlated with GDP. Results are much the same, however, when they are included as controls.

or county pairs ($\alpha_{b,\text{cp},t}$). These interaction terms sweep out between–CSA or between–county pair variations, allowing me to explore within-region annual variations for each bank. The β term, in essence, captures the effects of state pension deficits on deposits in a bank's branches located in contiguous counties on the opposite sides of state borders. The regressions also incorporate county fixed effects (α_c) to control for county-related time-invariant components, such as local policy or cultural differences.

In the foregoing specifications, there are two sources of serial correlation that must be taken into account. First, there is a positive serial correlation in deposits at the same bank branch or dividends of the same county, which could bias the estimated standard errors downward (Bertrand, Duflo, and Mullainathan 2004). For all specifications, I cluster standard errors at the state level in order to accommodate such a bias. Second, recall that—for the sample of contiguous border county pairs—a single county can appear in multiple pairs and so lead to repeated entries and mechanical correlation across county pairs. To account for this source of correlation in specification (3), I follow Dube, Lester, and Reich (2010) and cluster standard errors for any county pairs that share the same borderline.

Table 3 presents the regression results for bank deposits. I consider two samples: a full sample covering all banks and a subsample covering interstate banks with branches located in at least two states. All four regressions yield significantly positive estimates of β, confirming a positive relationship between state pension deficits and local bank deposits. For two contiguous border counties characterized by a 1% difference in their state public pension deficits, a bank's branches located in the state with larger pension deficits has 1.77% more deposits than its branches in the other state. Thus \$1 more of pension shortfall has the effect of increasing retail deposits by \$0.70.[25]

I interpret the coefficient reported in column (4) of the table because the sample with contiguous

[25] An average state in the sample has a GDP of \$347.15 billion and deposits amounting to \$195.07 billion. A 1% increase in the pension deficit corresponds to \$3.47 billion, and a 1.77% increase in bank deposits is equivalent to \$3.45 billion. If one assumes (based on Call Report data) that 70% of bank deposits are due to retail households, then each additional \$1 in the pension deficit leads to ($3.45 \times 70\%/3.47$) = \$0.70 worth of more retail deposits.

border county pairs should give estimates that are less biased—as can be inferred from the insignificant coefficients on controls in columns (2) and (4) but significant coefficients in columns (1) and (3). The former suggest that contiguous border county pairs are more similar in characteristics than are contiguous border counties within a CSA region, which means that the omitted variables problem should be less of a concern. Furthermore, the similar results in columns (1) and (3) (and in columns (2) and (4)) confirm that the identification relies on interstate banks. Note that there could be spillover effects between contiguous border counties—as when, for example, households migrate across states. Such effects would suppress the estimates and make it more difficult to document a significant relationship between pension deficits and bank deposits.

4.2.3 Results on Stock Investments

Next, I examine how stock investments are affected by public pension deficits. Stock investments at the local level are approximated by the county-level dividends reported to the IRS. I adopt the contiguous border county strategy to account for omitted economic conditions. The specifications are as follows:

$$\log(\text{Dividends}_{c,t}) = \alpha_{\text{csa},t} + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} + \mathit{Controls}_{c,t} + \varepsilon_{c,t}, \tag{4}$$

$$\log(\text{Dividends}_{c,t}) = \alpha_{\text{cp},t} + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} + \mathit{Controls}_{c,t} + \varepsilon_{c,t}; \tag{5}$$

here $\text{Dividends}_{c,t}$ denotes total dividends in county c during year t, and all other variables are defined as before. The fixed effects $\alpha_{\text{csa},t}$ or $\alpha_{\text{cp},t}$ sweep out annual between-region variations. Since two thirds of the assets held by public pension funds are invested in the stock market, it is possible that the stock market's aggregate performance simultaneously affects both pension deficits and dividend payouts. However, such aggregate factors are absorbed by year fixed effects. Note also that region–year fixed effects ($\alpha_{\text{csa},t}$ or $\alpha_{\text{cp},t}$) control for any home bias in households' investments (Coval and Moskowitz 1999) because the regression compares households that live close by and so are likely to have the same home bias. The standard errors are clustered as in the corresponding specifications for bank deposits.

The results are reported in Table 4. Columns (1) and (2) confirm a negative relationship between pension deficits and dividends, based on the contiguous border county strategy. Roughly speaking, if one state has a 1% larger pension deficit than a contiguous state, then households in that state receive 0.49% less dividends than those in the other state. In dollar terms, households receive \$0.0066 less in dividends as a result of each additional \$1 of pension shortfall.[26] If one assumes a dividend yield of 2% (the approximate average dividend yield of the S&P 500), then households reduce their stock investments by \$0.33.[27]

It is worth discussing a few limitations of adopting dividends as the proxy for stock investments. First, state income taxes may be correlated with state pension deficits and also with households' investment strategies. If a state sets high tax rates because of large pension deficits and if households respond by shying away from high-dividend stocks, then the estimate of β will be biased downward. Second, not all stocks pay dividends. Even among stocks in the S&P 500 index, more than 15% do not pay regular dividends—and the ratio is higher for small stocks. Third, share buybacks have become a popular way to reward shareholders in the past few years. Companies indexed by the S&P 500 hit a record high (\$806 billion) for stock buybacks in 2018, up 55% from 2017. Finally, there is a time lag between stock purchases and dividend payouts; this means that dividend payouts in a given year are not proportional to stock purchases made in that year. These issues may explain the relatively weak evidence for dividends.

[26] Households in the average state receive \$4.62 billion in dividends, and a 0.49% cut in dividends corresponds to \$0.023 billion. Therefore, \$1 more of pension shortfall reduces dividends by 0.023/3.47 = 0.0066.

[27] Given the results from Section 4.2.2, it follows that an additional \$1 in the pension deficit leads to a \$0.37 increase in aggregate savings; here deposits increase by \$0.70 and stocks decrease by \$0.33. In other words, the degree of substitution between public pension wealth and private savings (both deposits and stocks) is about 0.37; this figure is similar to the estimate of 0.3 reported by Lachowska and Myck (2018), which was based on Poland's 1999 pension reform.

4.2.4 Results on Deposit Rates

If households indeed save more deposits in states with larger pension deficits, then we should expect banks to set lower deposit rates in those areas. Not only that, households should prefer longer-term savings products for retirement purposes. Here I test these predictions using data (from RateWatch) on branch-level deposit rates of products with different maturities.

As shown in Table 1, branches that actively set deposit rates are three times larger than the average branch. Most small branches follow nearby rate-setting branches that belong to the same bank, so there is little difference in the rates of products offered by a given bank's branches located within a CSA region or a contiguous county pair. I therefore relax region restrictions and compare deposit rates of same-bank branches that are located in different states. The regression specification is

$$\text{Rate}^{x}_{i,b,c,t} = \alpha_{b,t} + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} + \textit{Controls}_{c,t} + \varepsilon_{i,b,c,t}. \tag{6}$$

In this expression, $\text{Rate}^{x}_{i,b,c,t}$ is the deposit rate of product x at branch i of bank b located in county c during year t; the $\alpha_{b,t}$ term represents bank × year fixed effects.

Figure 2 plots the estimated β for different products. The figure exhibits a decreasing pattern for the estimated βs as the maturity of deposit products increases. For products INTCK0K and MM25K, there is no significant difference in deposit rates offered by the same banks' branches located in states with large versus small pension deficits. The negative coefficient becomes significant (at the 90% confidence level) for 12-month CDs and reaches the minimum for 60-month CDs. In terms of magnitude, a 1% increase in pension deficits results in a 0.38–basis point decrease in the 60-month CD deposit rate. These findings are indicative of an upward shift in households' bank savings—especially long-term deposit products—in response to large state pension deficits, complementing the analysis in Section 4.2.2.

In sum: the evidence on bank deposits, stock investments, and deposit rates support the conjecture that large public pension deficits result in more bank deposits and fewer stock investments.

4.3 DiD Analysis of the 2015 GASB Updates

So far, the analysis has relied on the assumption that households can assess the severity of state pension deficits in their respective states; that is, their perceptions of deficits are consistent with the funding status calculated by the Federal Reserve. Yet, that was a challenging task in the past because (i) pre-2015 accounting guidelines did not make it mandatory for the governments to disclose enough information, and (ii) the Federal Reserve did not make the pension data, dating back to early 2000, publicly available until February 2016.

Accounting guidelines for state and local governments are set by the GASB. State public pension plans were governed by GASB Statement Nos. 25 and 27, in effect since 1994. These standards enabled state governments to cover up their state pension deficits in three ways. First, governments could engage in "asset smoothing;" that is, they could average the performance of pension investments over a five-year span to smooth asset values. Second, the discount rate for pension liabilities was mapped with the expected return on investment portfolios. Andonov, Bauer, and Cremers (2017) provided evidence that, in order to report a better funding status, fund managers invested more in risky assets—to the extent that the average annual discount rate reached 7%. Third, state governments were not required to report pension liabilities in their comprehensive annual financial reports (CAFRs) and so often painted a distorted picture of the government's actual financial status.

In the late 2000s, Novy-Marx and Rauh (2011) documented $4.43 trillion in outstanding debt of public pensions when liabilities were discounted with zero-coupon Treasury yields. Thereafter, policymakers and newspapers began to pay attention to the issue. In 2012, GASB updated its guidelines for public pension plans; by fiscal year 2015, state and local governments started adopting the new standards (known as GASB 67 and 68). The new guidelines require that state and local governments (i) report the market value of assets managed by their public pension plans, (ii) give details on how discount rates are constructed when calculating pension liabilities, and (iii) report unfunded pension liabilities in CAFRs. All these changes help residents to assess state public pension deficits more accurately.

I take the GASB updates as a quasi-exogenous shock and examine households' reactions to pension deficits once governments have less leeway regarding their reporting of pension funding status. Since the GASB updates did not affect the level of public pension deficits over a short time span, this exercise emphasizes the importance of households' *perceptions* of pension deficits in determining their savings allocation.[28]

My identification relies on the assumption that governments with large pension deficits were more likely to manipulate pension status under the old accounting standards. This idea is illustrated in the following example. Alice and Bob, who have the same income, live in states *A* and *B* (respectively). State *A*'s pension deficits are 30% of its GDP, and state *B*'s are 10%. Owing to government *A*'s manipulation of its pension status, Alice perceives only 20% pension deficits in her state and saves 40% of her income via bank deposits. In contrast, state *B* reports its true pension status—whereupon Bob saves 20% of his income. In reality, individuals' perceptions of pension deficits are unobservable. What can be observed by econometricians is the *actual* pension deficits which correspond to those given by the Federal Reserve. In this case, the slope β between the actual pension deficits and bank deposits is equal to 1 $\left(= \frac{40\% - 20\%}{30\% - 10\%} \right)$. After the actual pension status is revealed in state *A*, Alice increases her deposits to 60%. Then the slope β becomes 2 $\left(= \frac{60\% - 20\%}{30\% - 10\%} \right)$. Therefore, comparing the relations between pension deficits and bank deposits (or stock investments) before and after the GASB updates allows me to identify households' saving behaviors as a response to the change in perceived pension deficits. These tests are specified in the

[28] Table OA.2 in the Online Appendix shows that there is no significant difference in public pension deficits among counties in one CSA region or between counties in one contiguous county pair before and after the GASB updates.

following regressions:

$$\log(\text{Deposits}_{i,b,c,t}) = \alpha_{b,\text{csa},t} \text{ (or } \alpha_{b,\text{cp},t}) + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} \times \text{Post}_t$$
$$+ \mathit{Controls}_{c,t} \times \text{Post}_t + \varepsilon_{i,b,c,t},$$
$$\log(\text{Dividends}_{c,t}) = \alpha_{\text{csa},t} \text{ (or } \alpha_{\text{cp},t}) + \alpha_c + \beta \text{Pension Deficits}^{\text{GDP}}_{s,t} \times \text{Post}_t$$
$$+ \mathit{Controls}_{c,t} \times \text{Post}_t + \varepsilon_{c,t},$$

where the indicator variable Post_t is set to 1 for 2015–2017 or to 0 for 2013–2014.

Table 5 presents the results. In all specifications for bank deposits, the coefficient for the interaction term is positive and statistically significant, suggesting that the relationship between actual pension deficits and bank deposits was stronger following the GASB updates. Specifically, compared to two years before the GASB updates, households saved additional 0.23% more in deposits for a 1% increase in pension deficit after the GASB updates. A calculation indicates that the upper bound on misperceptions of pension deficits before the GASB updates was about 17%.[29] Columns (5) and (6) present results for stock investments. Negative coefficients for the interaction terms establish (relatively weak evidence) that after the GASB updates, households withdrew additional 0.07 % more capital from the stock market for a 1% increase in pension deficit, compared to periods under the old rules. The coefficient in column (6) translates into an upper bound of 12% $\left(= \frac{0.072}{0.072+0.525}\right)$ on the misperception under old GASB 25 and 27, which is slightly lower than 17% calculated using results for bank deposits.

Overall, the results offer evidence that households save more via bank deposits and less via stocks in response to an increase in *perceived* pension deficits. However, one could reasonably

[29] Suppose that the relationship between perceived pension deficits and deposit savings is linear. In the example of Alice and Bob, let PD_A (resp. PD_B) denote the true pension deficits of state A (resp. B) and let PD^{w}_A denote Alice's wrong perception before the GASB updates. Also, let D_A (resp. D_B) denote the right amount of deposit savings for Alice (resp. Bob) and let D^{w}_A denote the wrong amount under Alice's wrong perception. Then $\beta_{\text{before}} = (D^{\text{w}}_A - D_B)/(\text{PD}_A - \text{PD}_B)$ and $\beta_{\text{after}} = (D_A - D_B)/(\text{PD}_A - \text{PD}_B)$. The ratio $(\beta_{\text{after}} - \beta_{\text{before}})/\beta_{\text{after}} = (D_A - D^{\text{w}}_A)/(D_A - D_B)$ is larger than the "misperception ratio" for Alice, $(\text{PD}_A - \text{PD}^{\text{w}}_A)/\text{PD}_A$, from which it follows that $(\beta_{\text{after}} - \beta_{\text{before}})/\beta_{\text{after}}$ is the upper bound of misperception for Alice.

challenge an analysis (such as this) that relies on a single nationwide shock—that is, the observed phenomenon could have been caused by some other national policy change (or changes) made about the same time. In the next section, I investigate state pension reforms in order to solidify the identification.

4.4 Staggered DiD Analysis of State Public Pension Reforms

After knowing that households' perceptions of pension deficits affect their savings decisions, I move towards examining how households react to an *actual* reduction in pension deficits. To do so, I exploit the staggered adoptions of public pension reforms by state governments in attempting to narrow down their pension deficits.

This exercise also addresses the drawback from the foregoing analyses: state pension deficits may be correlated with state-level fiscal policies, such as taxes and spending, which in turn might affect households' savings. This concern makes it questionable to attribute the observed effects to pension deficits.[30] Given that reforms were enacted with heterogeneity in both treatment and timing such that there was little synchronization with changes in other states' policies, it is reasonable to attribute most of a household's re-allocation of its savings—during a one-year reform "window"—to the reform itself.

4.4.1 Details of State Pension Reforms

The period 2009–2018 saw, altogether, 94 reforms of state public pension plans. For each reform, I collect information on the main modifications and the affected worker groups. I classify the modifications into three categories—benefit reductions, contribution increases, and plan design changes—and label the affected worker groups as either new hires or "others".[31]

[30] If the GASB updates also affected other states-level fiscal policies, then the DiD analysis in Section 4.3 is not enough to conclude a causal relationship between public pension deficits and households' savings allocation.

[31] There are three affected worker groups: new hires, current employees, and retired employees. If the reform applied both to new hires and to current workers, then I classify it as "others". Each reform can contain several

All reforms have the effect—though to various degrees—of reducing pension liabilities and hence pension deficits. Given the intuition presented in Section 4.1, I expect that a pension reform that lowers deficits will be followed by fewer bank deposits and more stock investments. Yet reforms that reduce pension benefits for current public employees may have double-edged effects. On one hand, reduced government debt encourage all state residents to shift their savings from deposits to stocks; on the other hand, directly affected current public workers would increase their deposit savings because reforms reduce their future benefits. To avoid these mixed effects, I exclude reforms that reduce pension benefits for current employees.[32] Hence there are 58 reforms in the treatment group. Figure 3 plots the distribution of reforms over the study period. Table 6 addresses the determinants of pension reform, which (it turns out) can be predicted by *none* of these factors: the size of the pension deficit, the governor's political party, the strength of unions, or the tax margin (whether low or high).

I first use the following panel regression to confirm that pension reforms reduce pension deficits:

$$Y_{s,t} = \alpha_s + \alpha_t + \sum_{\tau=-2}^{2} \text{Treatment}_{s,t+\tau} + \varepsilon_{s,t}, \tag{7}$$

where $Y_{s,t}$ is the ratio of a state's pension assets, pension liabilities, or pension deficits to that state's GDP. The indicator variable $\text{Treatment}_{s,t}$ is set to 1 if state s enacts a reform in year t (and is otherwise set to 0); $\text{Treatment}_{s,t+\tau(\text{or } t-\tau)}$ is the same as $\text{Treatment}_{s,t}$ but τ years *after (or before)* the reform in year t. Adding leads and lags of $\text{Treatment}_{s,t}$ helps me trace the effects of pension reform. The regression also includes state and year fixed effects (resp., α_s and α_t) to control for time-invariant state characteristics and the aggregate time trend.

Figure 4 illustrates the regression results. For the reform year, coefficients are both negative and significant for all three measures, with the largest magnitude for pension liabilities. Note also that the reductions in pension liabilities and deficits are persistent. On average, pension reforms

modifications; for instance, Rhode Island's 2011 reform of its Employees' Retirement System adopted a new hybrid plan and also reduced the cost-of-living adjustment for all new hires.

[32] The results are robust to including these reforms in the sample; see Section C of the Online Appendix.

reduce state pension liabilities by 0.63% (= |−0.78% − (−0.15%)|) and pension deficits by 0.54% (= |−0.49% − 0.06%|) during the year of reform. In dollar terms, pension deficits decrease by an average of $1.87 billion. These results confirm that pension reforms are effective at cutting liabilities and narrowing the gaps in funding for state public pension plans.

4.4.2 Impact of Reforms on Bank Deposits and Stock Investments

Since pension reforms reduce the financial burden on state government, households should reallocate their savings. Here I test for whether savings are indeed transfered from banks deposits to stocks after pension reforms. The research design follows DiD with dynamic treatment effects adopted by Gilje, Loutskina, and Strahan (2016) and Smolyansky (2019). To sharpen the identification, I use the same within-bank and contiguous border county identification strategies as in Section 4.2. The regression for deposits is specified as follows:

$$\Delta \log \text{Deposits}_{i,b,c,t} = \alpha_{c,\text{csa},t} \text{ (or } \alpha_{c,\text{cp},t}) + \alpha_c + \sum_{\tau=-2}^{2} \text{Treatment}_{s,t+\tau} + \Delta \mathit{Controls}_{c,t} + \varepsilon_{i,b,c,t};$$

where $\Delta \log \text{Deposits}_{i,b,c,t}$ is the deposit growth at branch i of bank b in county c at the end of June in year t, and all other variables are as in the text following Equations (2) and (7). The deposit growth in one-year ($\Delta \log$ Deposits) captures households' reactions to pension reforms. This regression compares the deposit growth of bank branches in pension-reforming states with the same bank's branches located in contiguous border counties of non–pension-reforming states. To be classified as non–pension-reforming states, states must enact *no* pension reforms in neither the preceding year nor the subsequent two years.

The leads and lags of $\text{Treatment}_{s,t}$ play multiple roles. First, because deposit data are recorded at the end of June in each year, concurrent deposit growth reflects only the impact of pension reforms passed in the first half of the focal year. Therefore, a regression that incorporates $\text{Treatment}_{s,t+1}$ would cover reforms passed in the year's other half. Second, the specification helps address the omitted variables problem whereby pension reforms might be the outcome of some unknown factor(s)—for instance, a deteriorating economic environment—by which households' saving behaviors are also affected. In the face of a shrinking economy, households will choose to save more while the state government seeks to modify public pension terms in order to diminish its financial burden. In that case, households may even change their savings behavior *before* a pension reform. The lead terms of $\text{Treatment}_{s,t}$ can be used to examine this concern.

Columns (1)–(4) of Table 7 give the regression results for deposits. None of the lead terms of $\text{Treatment}_{s,t}$ is significant in all specifications. This evidence rules out the possibility that omitted factors drive both pension reforms and households' saving behaviors. During a reform year, affected bank branches experience substantial deposit outflows—which carry into the next year but die out within two years. These results offer clear evidence that deposits are withdrawn from banks in response to pension reforms. The average outflow from bank branches is about 1.22%, the equivalent of $370.14 per household in an average state.[33]

Columns (5) and (6) of the table present results for county-level dividends. Here the coefficients' signs are the opposite of those for deposits. After a pension reform, dividends are 2.54% higher than in non–pension-reforming contiguous border counties—the equivalent of a $912 stock investment for a household living in an average county.[34] Overall, these results support the hypothesis that pension reforms incentivize households to shift their savings from banks to the capital market, in line with the results from Sections 4.2 and 4.3.

[33] An average state has $195.07 billion in deposits and a population of 6.43 million. Hence the 1.22% worth of deposit outflows amounts to some $2.38 billion, which is equivalent to $\$370.14 = (2.38 \times 1000)/6.43$ per capita. Moreover, the coefficient magnitude, 1.22%, is only slightly higher than the estimation based on Table 3. For an average 0.54% reduction in pension deficits after reforms—referenced in the previous section—deposits decrease by $0.96\% = (0.54\% \times 1.77)$ accordingly.

[34] For dividends, the coefficient magnitude is not consistent with the Table 4 results. According to column (2) of that table, if pension reforms reduce deficits by 0.54% then stock investments should increase by 0.26% $(= 0.54\% \times 0.49)$. This inconsistency may be due to an inaccurate approximation of the dividends for stock investments and/or to households being overoptimistic about the effectiveness of pension reforms.

4.5 Robustness

I summarize the results from robustness tests reported in Section B of the Online Appendix. First, I illustrate graphically the relationship between pension deficits and allocation of savings without complex identification strategies. Second, I decompose pension deficits into liabilities (scaled by GDP) and assets (scaled by GDP), and find that households are capable of assessing two components separately and make allocation decisions according to two components, especially liabilities. Third, the results on deposit rates are robust when controlling for countypair × year fixed effects instead of bank × year fixed effects. Fourth, I see similar results (with smaller effect sizes) if extending the sample to 2002–2018.

5 Economic Consequences

The preceding findings offer a new perspective for understanding the effects of pension reforms on the transformation of savings into investment through the financial system. The extant evidence addresses only the impact of pension reforms on the capital market and ignores the intermediary role of banks in transmitting pension shocks. My results underscore that pension deficits and pension reforms profoundly affect households' saving decisions. Their withdrawals from (or deposits to) banks can generate non-negligible economic effects, through banks' internal networks, that differ from the effects that arise via the capital market.

This section addresses the economic consequences of pension reforms, mainly via the banking sector. The analysis is conducted in several steps. First, I show that deposit outflows induced by pension reforms influence banks' aggregate balance sheets: total liabilities and assets decline more for banks with more branches located in states that have enacted pension reforms. Next, I document that (i) banks reduce their lending to small businesses because of their eroded balance sheets and (ii) the reduction is concentrated in non–pension-reforming states. Finally, lending cuts further impair the local economy: states that do not reform their pension systems witness fewer new business establishments and experience higher unemployment. My analysis showcases the negative spillovers of pension reforms from one state to others through households' re-allocation of capital away from banks.

5.1 Effects on Bank Balance Sheets

If the branch deposit outflows induced by pension reforms are negligible at the bank level, then we will see little further effects. Accordingly, I start by examining the aggregate reform-related effects on banks' balance sheets. Since balance sheet data are available only at the bank level, I construct a measure to capture a single bank's exposure to pension reforms across different states. Thus the ratio of a bank's branches in one state to all of its branches is used to measure a bank's exposure in that state.[35] I then sum the exposure-weighted reform treatments across all states. The formula used to calculate bank b's exposure to all pension reforms in year t is

$$\text{Exposure}_{b,t} = \sum_{s} \text{Treatment}_{s,t} \times \text{Proportion of Branches}_{b,s,t}. \tag{8}$$

By construction, $\text{Exposure}_{b,t}$ ranges between 0 and 1.

I now estimate the following regression for items on banks' balance sheets:

$$\Delta \log \mathrm{Y}_{b,t} = \alpha_b + \alpha_t + \text{Exposure}_{b,t} + \varepsilon_{b,t},$$

where $\mathrm{Y}_{b,t}$ represents liability items (i.e., total liabilities, total deposits, retail deposits, total equities, wholesale funding), or book value of equity, or asset items (i.e.., total assets, C&I loans, personal loans, real estate loans, security holdings). Because deposit outflows occur during the treatment year, the regression incorporates only the current extent of $\text{Exposure}_{b,t}$. The terms α_b and α_t capture bank and time fixed effects, which account for (respectively) time-invariant bank characteristics and the aggregate time trend. Standard errors are clustered at the bank level.

[35] Using the proportion of branches, instead of the proportion of deposits, reduces the influence due to the outflows (or inflows) of deposits resulting from pension reforms. However, results are robust to using the deposits-based measure.

Panel A of Table 8 presents results for *liability* items and *equity*. The significantly negative coefficient in column (2) suggests that banks with more branches in pension-reforming states suffer a greater reduction in deposits. This finding confirms that reform-induced deposit outflows at the branch level are non-trivial when aggregated at the bank level. The decline in deposits also affects banks' total liabilities. When a pension reform is passed in one state, banks lose (in aggregate) \$1.12 billion in total liabilities, \$1.02 billion in total deposits, and \$0.91 billion in retail deposits.[36] The small discrepancy between total deposits and retail deposits reflects that deposit withdrawals are mostly due to households—rather than other sources, such as corporations. This result accords with the analysis, presented in Section 4, based on FDIC deposits data.

In a frictionless world where banks have easy access to external capital markets, a reduction in deposits could be compensated by other sources of funding. However, the result in column (5) of the table runs counter to this hypothesis. Although banks raise 0.45% more equity, that amounts to only \$0.15 billion—much less than the loss in deposits. Hence total assets shrink by 0.36% (about \$1.01 billion), based on column (1) of Panel B. The reported values imply the existence of financial frictions in the banking sector, which aligns with prior work on the bank lending channel (see e.g. Schnabl 2012; Gilje, Loutskina, and Strahan 2016; Smolyansky 2019).

A close inspection of the *asset* items on banks' balance sheets reveals a significant contraction in C&I loans and in securities holdings.[37] In terms of magnitude, a 0.88% cut in C&I loans corresponds to \$0.25 billion and so accounts for 25% of the reduction in bank assets; the rest, about \$0.80 billion, largely reflects curtailment of securities holdings. In sum: total liabilities and assets shrink more when banks are more exposed to pension reforms; as a result, banks reduce their C&I loans and securities holdings.

It is worth noting that pension reforms generate two competing forces on the stock market. On

[36] If a pension reform is randomly enacted in one of the 49 sample states (District of Columbia is treated as one state), then the expected change in $\text{Exposure}_{b,t}$ is 1/49. I calculate the expected effect on item Y for bank b as $Y_{b,t} \times \text{Coeff}_{\text{Y}} \times 1/49$ and then aggregate across all banks.

[37] C&I loans are used by businesses for working capital or to finance capital expenditures. In 2018, C&I loans at all commercial banks reached an all-time high of approximately \$2.3 trillion.

one hand, deficit cuts encourage households to invest more in stocks; on the other hand, banks liquidate security positions because reforms erode their balance sheets. Hence, the aggregate impact on the stock market is ambiguous.

5.2 Effects on Small Business Lending

Although the foregoing results suggest that banks with greater exposure to pension reforms are more likely to reduce their C&I lending, the analyses cannot eliminate the omitted variables concern that pension reforms might also affect banks' lending opportunities. This possibility is a challenge commonly faced in studies of bank lending problems. To address the issue, I turn to data (from CRA) on small business lending, a category of C&I loans. The database records newly originated loans to small businesses for each reporting bank at the county–bank–year level. These granular data allow me to control for unobserved lending opportunities. Notably, it is essential to understand the pension reform–related effects on small businesses because they employ nearly half (47%) of the private-sector workforce (SBA Advocacy, 2017).

Following Drechsler, Savov, and Schnabl (2017), I adopt a within-county strategy to isolate the demand for and the supply of bank lending. The underlying assumption is that banks located in the same county face similar lending opportunities. In that case, if banks that are relatively more exposed to pension reforms reduce their lending to a greater extent, then we can infer that the effect arises from the reduction in banks' funding supplies. I run the following regression specification:

$$\log(\text{New Lending}^{\$/\#}_{b,c,t}) = \alpha_{c,t} + \alpha_b + \text{Exposure}_{b,t} + \mathit{Controls}_{b,t} + \varepsilon_{b,c,t},$$

where I use $\text{New Lending}^{\$}_{b,c,t}$ (resp., $\text{New Lending}^{\#}_{b,c,t}$) to denote newly originated loans of less than \$1 million (resp., in quantities) granted to small businesses by bank b in county c during year t.[38] Here $\alpha_{c,t}$ captures county $\times$ year fixed effects, which restrict the comparison to that between banks

[38] Results for bank lending to small businesses whose *gross annual revenues* are less than \$1 million are reported in Section C of the Online Appendix.

with different exposures—but located within the same county—each year. The α_b term represents bank fixed effects; including this term controls for time-invariant bank-specific characteristics (e.g., brand effects). Bank controls include (logged) age and assets. The inclusion of time-varying bank controls ensures that lending variations do not derive from the size of banks—as would be the case, for instance, if expanding banks lent more. Error terms are clustered at the bank level.

It is critical to note that, for purposes of this regression, I use a subsample consisting only of non–pension-reforming states that enacted *no* pension reforms in neither the preceding year nor the subsequent two years.[39] This exclusion of pension-reforming states allows me to rule out the direct effects of reforms on bank lending; then any changes in lending behaviors in non–pension-reforming states can be safely attributed to the effects of pension reforms via banks' balance sheets.

If banks that are more exposed to pension reforms extend less credit to small businesses in non–pension-reforming states, then the coefficient for $\text{Exposure}_{b,t}$ should be negative. The regression results in columns (1) and (2) of Table 9 confirm this conjecture in terms of both dollars and quantities. When a pension reform is passed in one state, banks reduce their small business lending by \$0.47 billion (in aggregate) in non–pension-reforming states. This figure is, surprisingly, much greater than the reduction of \$0.25 billion in C&I loans calculated previously. To uncover the reason for this considerable difference, I use the full sample of banks and report the results in the table's last two columns. The coefficients for $\text{Exposure}_{b,t}$ then become insignificant, and the estimated lending cuts are only \$0.13 billion. One interpretation of these findings is that pension reforms improve states' economic fundamentals and create profitable business opportunities, which encourages banks to re-allocate credit from non–pension-reforming states to pension-reforming states. That re-allocation, in turn, leads to much sharper lending cuts in non–pension-reforming states. In short: pension reforms in one state can impose—through the bank lending channel—negative externalities on other states.

[39] Although local branches facilitate access to credit (Nguyen 2019), about 45% of the full sample of banks extend credit also to businesses in regions where they do not have branches. Therefore, the analysis also covers banks without branches in non–pension-reforming states.

5.3 Effects on the Local Real Economy

The previous analyses not only identify pension reforms' effects on bank lending but also emphasize those reforms' negative externalities on non–pension-reforming states. Given that small business loans are the critical financing source for local firms with limited access to the capital market, I expect that such firms will bear the brunt of banks' reduced lending. The literature has documented that contraction of the bank credit supply to local small businesses is inimical to local investment and employment (see e.g. Chodorow-Reich 2014; Drechsler, Savov, and Schnabl 2017; Gilje 2019. However, it is still worthwhile examining the real impacts in my context because *quantifying* the negative externalities of pension reforms is valuable to researchers and policymakers alike.

To study county-level economic impacts, I need a variable that measures a county's exposure to pension reforms in other states. I follow Smolyansky (2019) in constructing a county-level exposure measure. As there are multiple banks in each county, it follows that a single bank cannot determine the county's overall exposure. I therefore average $\text{Exposure}_{b,t}$ across all banks in one county, weighted by each bank's propensity to grant loans in that county. I define County Exposure as follows:

$$\text{County Exposure}_{c,t} = \sum_b \text{Exposure}_{b,t} \times \underbrace{\log \text{Deposits}_{b,t-1} \times \frac{\text{New Lending}_{b,c,t-2\to t}}{\text{Total New Lending}_{c,t-2\to t}}}_{\text{Lending Propensity}_{b,c,t}}. \tag{9}$$

The $\text{Lending Propensity}_{b,c,t}$ of bank b with respect to county c is thus the product of bank b's (logged) deposits in the preceding year and the proportion of that bank's lending in county c during the last two years.[40] The value of $\text{County Exposure}_{c,t}$ is high if county c has a close lending relationship with big banks in year t. It is clear that a county with favorable economic conditions is likely to attract large banks and thus to have high County Exposure. To control for this effect,

[40] Recall that a bank can make loans in areas that contain none of its branches. For this reason, I cannot use the proportion of *branches* to proxy for lending propensity.

I include county-level lending propensity as a control variable:

$$\text{Lending Propensity}_{c,t} = \sum_b \text{Lending Propensity}_{b,c,t}. \tag{10}$$

In a regression including both County Exposure$_{c,t}$ and Lending Propensity$_{c,t}$, any effect on County Exposure must work through the Exposure component.[41]

Next, I estimate the following regression to establish a relationship between the county's exposure to pension reforms and its real economic activity:

$$\Delta \text{Y}_{c,t} = \alpha_c + \alpha_t + \text{County Exposure}_{c,t} + \text{Lending Propensity}_{c,t} + \Delta Controls_{c,t} + \varepsilon_{c,t};$$

here $\Delta \text{Y}_{c,t}$ is either the (logged) total new loans of less than \$1 million to small business or the (logged) change in economic activity—business establishments, employment or wages—in county c during year t. County controls include local population and GDP, both in log scales. Once again, α_c and α_t denote (respectively) county and year fixed effects. Standard errors are clustered at the county level. As in Section 5.2, I restrict the analysis to non–pension-reforming states such that any effect from reforms in *other* states is passed through banks' balance sheets.

Column (1) of Table 10 shows that effects of lending cuts persist at the county level. A county located in a non–pension-reforming state experiences a pronounced drop in small business lending when firms located in that county depend heavily on banks with a high exposure to pension reforms enacted in *other* states. This result is the prerequisite to observing further effects on economic activity. Column (2) confirms that high exposure to pension reforms reduces the number of new business establishments in non–pension-reforming states. Columns (3)–(5) report the effects on employment. That impact is negligible in all sectors, but it is negative for bank-dependent sectors and strongly negative for non-tradable sectors.[42] Moreover, wages in non-tradable sectors

[41] The correlation between County Exposure and Lending Propensity is only 0.19, so multicollinearity is not an issue.

[42] Bank-dependent sectors are industries that require, according to the US Census Bureau's 2012 Survey of Business Owners, above-median use of external financial capital. Per Chen, Hanson, and Stein (2017), these include establishments in the following North American Industry Classification System (NAICS) sectors (2-digit level): 31, 32, 33, 42,

get deducted for counties with high exposure to pension reforms. In terms of coefficients, an increase of one standard deviation in County Exposure (approximately 1.04) leads to an increase of 2.05% (= $1.04 \times 1.97\%$) in layoffs and a decrease of 5.19% (= $1.04 \times 4.99\%$) in wages for non-tradable sectors, a 1.68% (= $1.04 \times 1.62\%$) reduction in lending to small businesses, and a 0.35% (= $1.04 \times 0.34\%$) decline in new business establishments. Overall, the results confirm the negative effects of pension reforms on economic activity in non–pension-reforming states.

Yet the magnitude of County Exposure is difficult to grasp, so interpreting the coefficients is not intuitive. As an alternative, I (i) use County Exposure as an instrument for county lending activities to establish a relationship between lending and economic activity, and (ii) calculate the economic effects of pension reforms through the lending channel. In order to qualify as a valid instrument, County Exposure must fulfill (i) the *relevance* condition that County Exposure is strongly correlated with bank lending and (ii) the *exclusion* restriction that County Exposure affects economic activity only through the bank lending channel. For column (1) of Table 10, the Cragg–Donald F-statistic is 21.03; hence the null hypothesis of a weak instrument is rejected. Furthermore, I argue that County Exposure satisfies the exclusion restriction because the pension reforms captured by County Exposure are enacted in *separate* states. The exclusion restriction would be violated only if economic conditions in non–pension-reforming states drive the adoption of pension reforms in pension-reforming states—which is unlikely given that the enactment of pension reforms is associated with uncertainties and is not predicted by a state's economic fundamentals (see Table 6). It follows that County Exposure can serve as an instrument for bank lending in non–pension-reforming states.

I run IV regressions using instrumented new lending (the predicted values in column (1) of Table 10) and report the results in Table 11. For comparison purposes, I also report results from ordinary least-squares (OLS) regressions without instrumental variables. The estimates in OLS regressions may be biased because bank lending and local economic activity can affect each other

44, 45, 48, 49, 53, 55, 62, and 72. The non-tradable sectors are classified as in Mian and Sufi (2014); these include establishments—such as retail stores and restaurants— that are heavily dependent on local consumption and demand.

and/or be driven by omitted factors (e.g., government policies).

The significantly positive coefficients in columns (2) and (4) of Table 11 indicate that a 1% reduction in lending leads to a 1.13% decrease of employment in non-tradable sectors and to a 0.19% decline in new business establishments. The effect on wages in non-tradable sectors is positive but not statistically significant. In dollar terms, there is one layoff in non-tradable sectors if banks reduce their lending by $11,899 and one less business establishment if banks cut lending by $108,784.[43] The economic magnitude is slightly smaller than Smolyansky's (2019) estimate of about $25,000 in lending needed to create a job. The difference stems mainly from my focus on the non-tradable sectors. Combining these results with those from Section 5.2, I find that a single enactment of pension reforms in a given state, that reduces pension deficits by an average of $1.87 billion, can result in $0.47 billion worth of cuts in small business lending in non–pension-reforming states; this figure translates into 4,320 fewer business establishments (about 0.1% of total establishments) and 39,498 layoffs (about 0.4% total unemployment) in those states. These numbers are economically sizable, considering that 58 reforms have been enacted in the past decade. Overall, I find that pension reforms in one state impose sizeable negative externalities on non–pension-reforming states through households' re-allocation of capital away from banks.

6 Conclusion

The past decade has witnessed an acute funding problem across the US state pension system. This paper studies how households re-allocate their savings between bank deposits and stocks in response to pension deficits and reforms—as well as the consequences of that re-allocation on banks' balance sheets and economic activity. I find that households living in states with larger pension deficits

[43] For an average county, total new lending is $61.61 million, total business establishments are 2,949, and total employment in non-tradable sectors is 4,587. So the amount of bank lending required to create a job is $(61.61 \times 10^6 \times 1\%)/(4{,}587 \times 1.13\%) = 11{,}899$ and, for a new establishment, is $(61.61 \times 10^6 \times 1\%)/(2{,}949 \times 0.19\%) = 108{,}784$.

save more in banks and invest less in stocks. When states pass pension reforms that aim to improve the local funding situation, households rebalance their portfolios by shifting savings from banks to the stock market. Deposit outflows lead to major reductions not only in banks' liabilities but also in their assets available for lending. As a result, banks reduce their lending to small businesses in non–pension-reforming states, scaling down both employment in non-tradable sectors and new local business establishments. Thus, I highlight one negative effect of state public pension reforms that follow households' re-allocation of savings away from banks. These unexpected negative consequences of pension policy underlie the challenges faced by policymakers yet point to the possibility of improving current regulatory frameworks by internalizing those negative externalities.

The problem becomes more pressing as the already fragile public pension system is battered by the COVID-19 pandemic. The resulting widespread job losses and business closures have led to massive reductions in state and local tax revenues, and exploding healthcare expenditures impose an enormous burden on government budgets. All these factors increase the difficulties faced by state and local governments seeking to sustain the pension system during a period of unprecedented financial uncertainty. I hope that the research reported here encourages more research devoted to understanding the interplay among public pension systems, household savings, and the banking system. Building a comprehensive framework and fostering discussion on this topic should pave the way for better solutions to the pension crisis.

This study has several limitations. First, the analysis is not based on direct observation of households' savings allocations; instead, I infer the allocation from aggregate bank branch deposits and county-level dividends as revealed by tax filings. The results could be further validated if granular data (i.e., at the household level) were available. Second, I simplify the analysis by focusing on the two most important and representative types of savings: bank deposits and stocks. In reality, there is a wide spectrum of savings vehicles that include bonds and real estate. Future studies could contribute by exploring the effects of pension reforms on all types of investment to offer a comprehensive understanding of their consequences.

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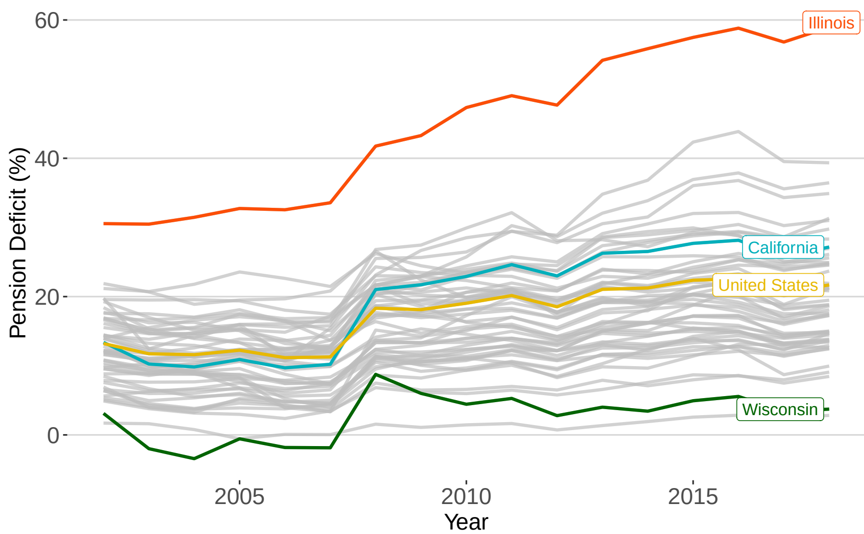
(a) Time-series plot



(b) Map plot

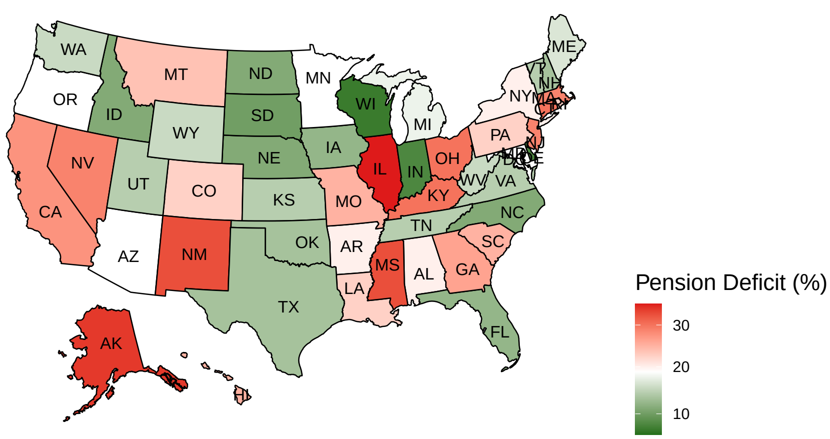


Figure 1: Plots of pension deficits of U.S. state public pension plans. The panel (a) depicts the time-series plot of pension deficits of U.S. state public pension plans over 2002-2018, and the panel (b) depicts a map of average pension deficits of U.S. state public pension plans over 2009-2018.

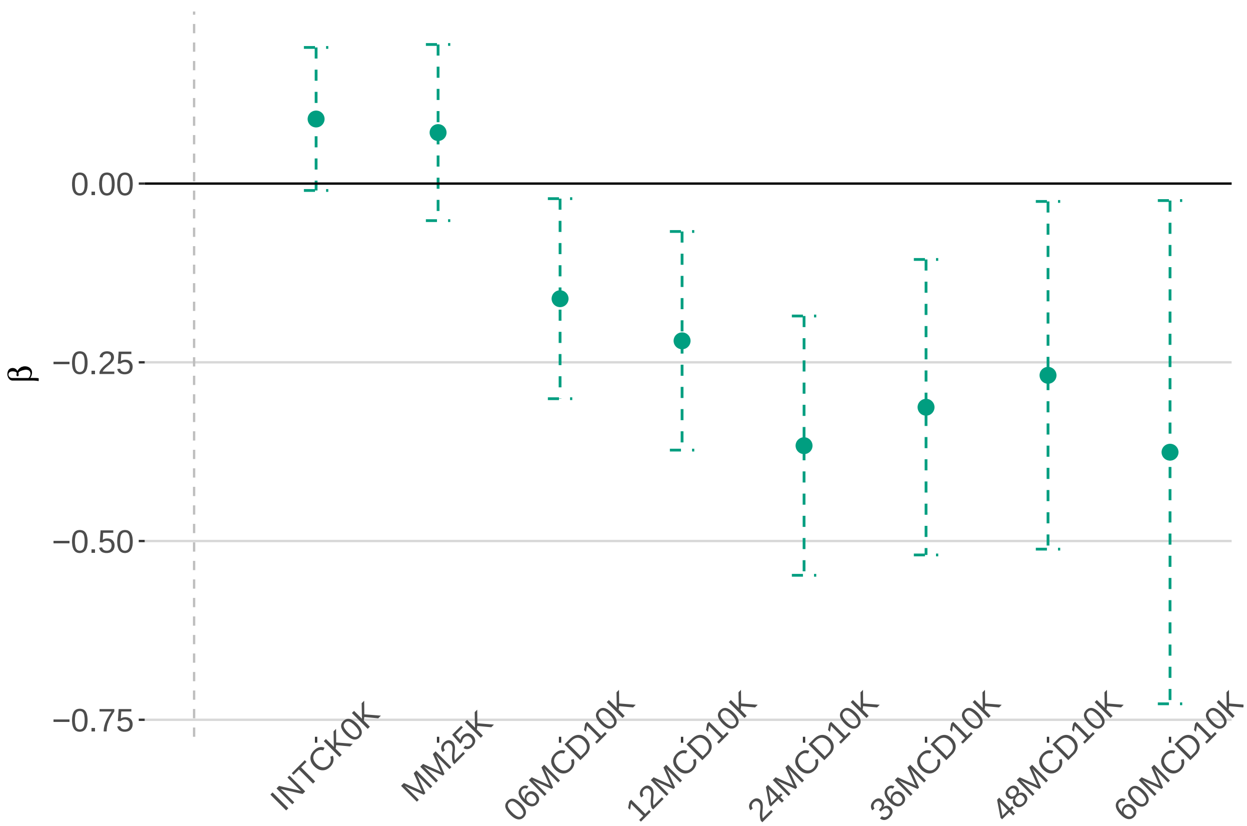


Figure 2: Bank deposit rates and state public pension deficits. This figure visualizes the coefficients of regressions (6) for difference deposit products. Dotted lines represent the 90% confidence interval. The regression results are presented in Table OA.1 of Online Appendix.

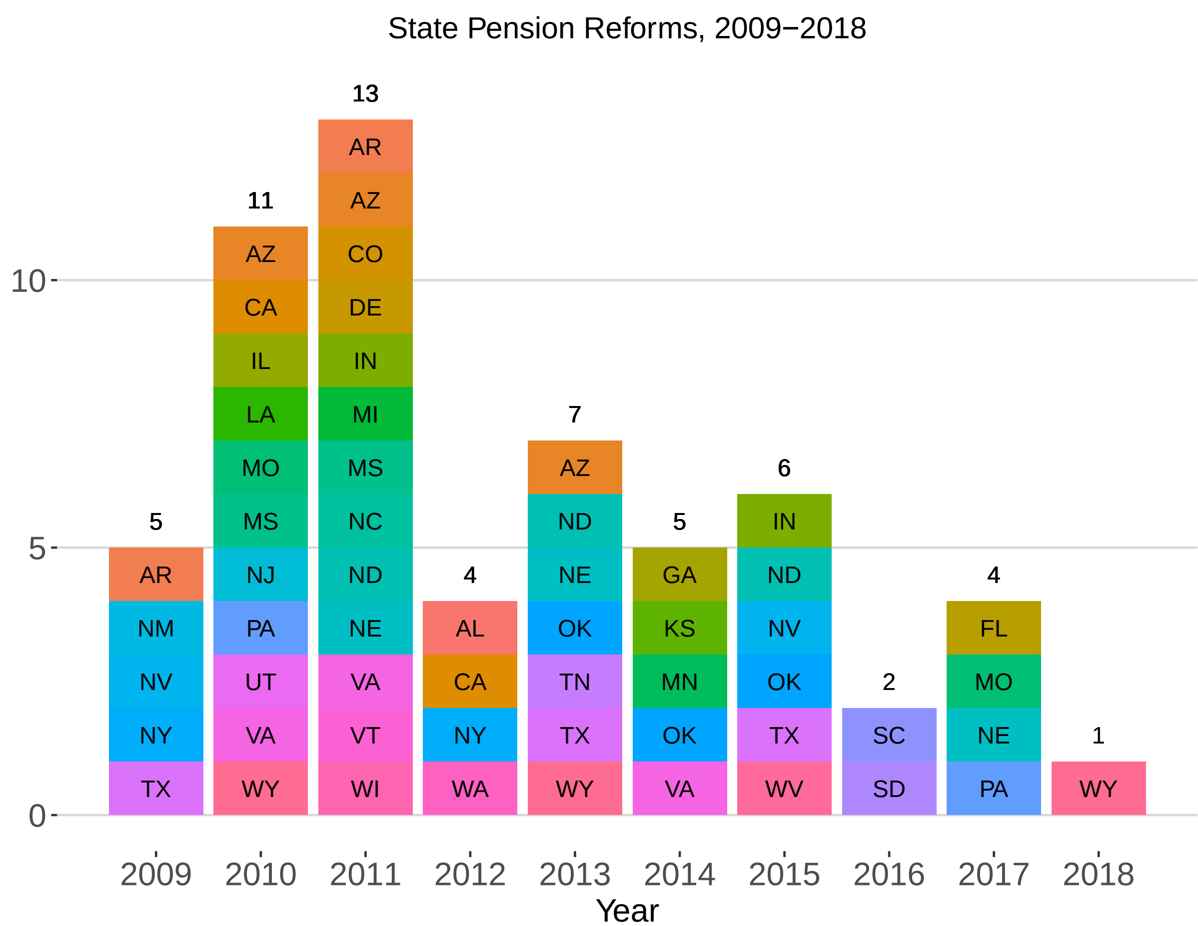


Figure 3: Distribution of state public pension reforms. This figure plots the enactment year of each reform in the sample, with state abbreviations presented in the box.

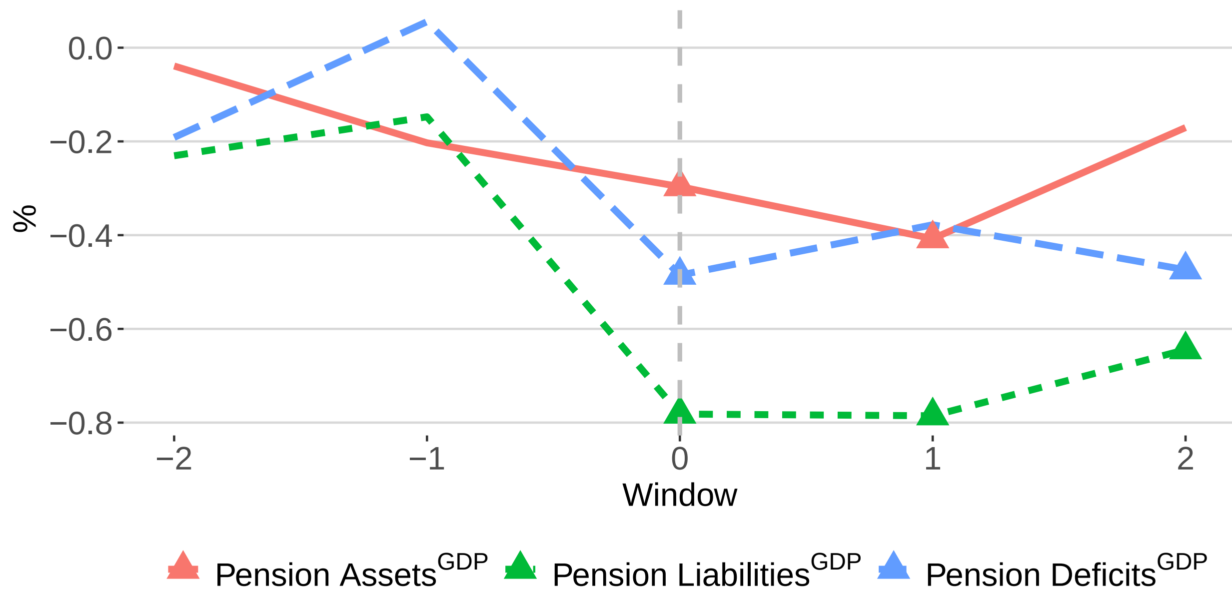


Figure 4: The impacts of pension reforms on state pension status. This figure visualizes the coefficients of regressions (7) for state pension assets, liabilities and deficits, all scaled by state GPD. Coefficients that are significant at 90% confidence interval are highlighted in triangle dot. The regression results are presented in Table OA.3 of Online Appendix.

	Panel A: U.S. state public pension plans funding status					
	All		Large Pension Deficit		Small Pension Deficit	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Assets, in \$B	70.84	117.75	91.51	149.14	52.55	76.13
Liabilities, in \$B	142.78	221.42	200.86	282.66	91.41	127.57
Pension deficits^{GDP}, in %	18.53	8.62	25.20	7.46	12.63	4.07
Obs. (branch × year)	490		230		260	

	Panel B: County characteristics					
	All		Large Pension Deficit		Small Pension Deficit	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
GDP, in \$B	5.52	23.90	6.84	28.70	4.44	18.98
Population, in K	102.30	325.97	124.42	405.68	84.02	239.41
Employment, in K	59.68	208.50	71.84	252.39	49.62	162.89
Per capita income, in \$K	38.68	11.21	37.24	11.08	39.87	11.18
Deposits, in \$B	2.33	8.65	2.77	9.36	1.98	8.01
Dividends*, in \$M	63.94	237.20	79.66	276.39	51.11	198.68
Obs. (county × year)	30,810		13,938		16,872	

	Panel C: Branch deposits					
	All		Large Pension Deficit		Small Pension Deficit	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Deposits, in \$M	68.41	111.87	72.03	113.21	64.13	110.11
Δ log(Deposit), in %	6.43	22.54	6.34	22.31	6.55	22.81
Obs. (branch × year)	891,636		482,771		408,865	

Panel D: Branch rates

	All		Large Pension Deficit		Small Pension Deficit	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
INTCK0K, in %	0.11	0.13	0.12	0.14	0.11	0.12
MM25K, in %	0.30	0.29	0.30	0.30	0.30	0.28
06MCD10K, in %	0.44	0.41	0.43	0.42	0.46	0.41
12MCD10K, in %	0.64	0.50	0.62	0.50	0.65	0.49
24MCD10K, in %	0.88	0.57	0.87	0.58	0.90	0.56
36MCD10K, in %	1.10	0.63	1.08	0.63	1.12	0.62
48MCD10K, in %	1.27	0.66	1.25	0.66	1.30	0.65
60MCD10K, in %	1.49	0.69	1.47	0.70	1.52	0.68
Deposits, in $M	184.38	625.41	188.28	632.14	180.44	618.51
Obs. (branch × year)	73,356		36,782		36,574	

Table 1: Summary Statistics. Panel A-D report the summary statistics of bank and county data used. Variables are defined in section 3.1. For each variable, the mean and standard derivation are reported for the full sample and also for subsamples based on a split of each year's median pension deficit. All variables (expect for pension deficits and county characteristics) are winsorized at the 0.5% quantile from both tails of the distribution. (* The data on dividends covers the period 2010–2017.)

	Diff GDP	Diff Pop	Diff Emp	Diff PCI	Diff PubEmp	Diff Under30	Diff Deficits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Average	0.057	0.059	0.063	−0.007	−0.052	−0.002	9.584***
	(0.843)	(0.944)	(0.868)	(−0.611)	(−0.512)	(−0.762)	(8.635)
Observations	12,740	13,080	12,740	12,740	9,636	13,080	13,080

Table 2: The test for average differences in contiguous border counties' characteristics. This table presents test results for differences in contiguous border counties' GDP, population (Pop), employment (Emp), per capita income (PCI), public employment (PubEmp), the proportion of the population under 30 (Under30) and public pension deficits (PD). All variables are in log scales except for the measurement "Under30". For each pair of contiguous border counties, I assign the one located in the state with larger public deficits as "treatment" and the other as "control". Then I compare all measurements using paired-t tests. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	log(Deposits$_{i,b,c,t}$)			
	All		≥ 2 States	
	(1)	(2)	(3)	(4)
Pension Deficits$^{GDP}_{s,t}$	2.119***	1.773**	2.053***	1.773**
	(2.762)	(2.255)	(2.840)	(2.489)
log(GDP$_{c,t}$)	0.162***	0.024	0.104	0.024
	(4.422)	(0.132)	(1.252)	(0.145)
log(PCI$_{c,t}$)	−0.235**	−0.149	−0.376**	−0.149
	(−1.999)	(−0.560)	(−2.318)	(−0.618)
County	Y	Y	Y	Y
Bank × Year × CSA	Y	N	Y	N
Bank × Year × County-pair	N	Y	N	Y
Observations	638,666	820,384	167,008	308,691
Adjusted R^2	0.359	0.292	0.304	0.293

Table 3: State public pension deficits and bank deposits. This table estimates the effects of state public pension deficits on bank deposits over 2009-2018. The first two columns employ a full sample covering all banks and the last two columns employ a subsample covering interstate banks with branches located in at least two states. log(Deposits)$_{i,b,c,t}$ is the deposits, in log scale, at branch i of bank b in county c at the end of June in year t, and is winsorized at 0.5% in each tail. Pension Deficits$^{\text{GDP}}_{s,t}$ represents the pension deficit in state s in year t, as defined by Equation (1). Control variables include GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1) and (3), and at both state and border segment levels for regressions (2) and (4). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	log(Dividends$_{c,t}$)	
	(1)	(2)
Pension Deficits$_{s,t}^{GDP}$	-0.559^{***}	-0.494^{*}
	(-2.720)	(-1.776)
log(GDP$_{c,t}$)	0.017	0.059
	(0.405)	(1.332)
log(PCI$_{c,t}$)	0.851^{***}	0.361^{***}
	(5.628)	(3.795)
County	Y	Y
Year × CSA	Y	N
Year × County-pair	N	Y
Observations	8,993	20,142
Adjusted R^2	0.996	0.992

Table 4: State public pension deficits and stock investments. This table estimates the effects of state public pension deficits on stock investments over 2010–2017. log(Dividend)$_{c,t}$ is the dividend income in log scales of county c during year t, and is winsorized at 0.5% in each tail. Pension Deficits$_{s,t}^{\mathrm{GDP}}$ represents the pension deficit in state s in year t, as defined by Equation (1). Control variables include GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regression (1), and at both state and border segment levels for regression (2). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	log(Deposits$_{i,b,c,t}$)				log(Dividends$_{c,t}$)	
	All		$\geq$ 2 States		All	
	(1)	(2)	(3)	(4)	(5)	(6)
Pension Deficits$^{GDP}_{s,t}$ $\times$ Post$_t$	0.221***	0.225**	0.231***	0.225***	-0.028	-0.072^{*}
	(3.280)	(2.477)	(3.558)	(2.731)	(-0.758)	(-1.715)
Pension Deficits$^{GDP}_{s,t}$	1.786***	1.077	1.712***	1.077	-0.180	-0.525
	(2.627)	(1.052)	(2.772)	(1.160)	(-0.368)	(-1.321)
Controls$_{c,t}$ $\times$ Post$_t$	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Bank $\times$ Year $\times$ CSA	Y	N	Y	N	N	N
Bank $\times$ Year $\times$ County-pair	N	Y	N	Y	N	N
Year $\times$ CSA	Y	N	Y	N	Y	N
Year $\times$ County-pair	N	Y	N	Y	N	Y
Observations	319,860	410,827	84,945	156,501	5,617	12,594
Adjusted R^2	0.363	0.302	0.305	0.298	0.998	0.997

Table 5: Difference-in-Differences on the 2015 GASB updates. This table estimates the effects of changes in GASB guidance on the relationship between public pension deficits on households' savings allocation over 2013-2017. log(Deposits)$_{i,b,c,t}$ is the deposits, in log scale, at branch i of bank b in county c at the end of June in year t, and log(Dividend)$_{c,t}$ is the dividend income in log scales of county c during year t (both are winsorized at 0.5% in each tail). Pension Deficits$^{\text{GDP}}_{s,t}$ represents the pension deficit in state s in year t, as defined by Equation (1). Post$_t$ equals 1 for years 2015–2017 and 0 for 2013–2014. Control variables include GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1), (3) and (5), and at both state and border segment levels for regressions (2), (4) and (6). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Summary statistics		Regressions	
	Mean	St. Dev.	$\mathbb{1}(\text{Pension reform}^{all}_{s,t})$	$\mathbb{1}(\text{Pension reform}^{sample}_{s,t})$
Pension Deficits$^{GDP}_{s,t-1}$, %	17.905	8.542	−0.002	−0.011
			(−0.186)	(−1.053)
$\mathbb{1}(\text{Democratic}_{s,t-1})$	0.406	0.492	−0.085	−0.005
			(−1.560)	(−0.102)
Union$_{s,t-1}$, %	11.016	4.999	0.009	0.008
			(0.472)	(0.474)
LowTax$_{s,t-1}$, %	2.227	1.761	0.007	−0.001
			(0.157)	(−0.026)
HighTax$_{s,t-1}$, %	5.245	2.973	−0.017	−0.051
			(−0.495)	(−1.295)
State			Y	Y
Year			Y	Y
Observations			480	444
Adjusted R^2			0.058	0.033

Table 6: Determinants for state public pension reforms. This table tests the determinants for state pension reforms over 2009-2018. The dependent variables, $\mathbb{1}(\text{Pension reform}^{all}_{s,t})$ ($\mathbb{1}(\text{Pension reform}^{all}_{s,t})$), take a value of 1 if state s passes any pension reform (pension reform included in the sample) at year t. $\mathbb{1}$(Democratic) takes a value of 1 if the state governor is democratic, and 0 otherwise. Union is the coverage ratio of union membership. LowTax and HighTax are the lower and upper bound of state tax rate, respectively. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	Δ log(Deposits$_{i,b,c,t}$)					Δ log(Dividends$_{c,t}$)	
	All		$\geq$ 2 States				
	(1)	(2)	(3)	(4)		(5)	(6)
Treatment$_{s,t-2}$	−0.192	0.421	−0.220	0.421		−1.743	−1.528
	(−0.328)	(0.670)	(−0.404)	(0.739)		(−0.954)	(−0.719)
Treatment$_{s,t-1}$	−0.144	−0.400	−0.203	−0.400		1.184	1.290
	(−0.232)	(−0.634)	(−0.333)	(−0.699)		(0.756)	(0.852)
Treatment$_{s,t}$	−1.232***	−1.217*	−1.302***	−1.217*		1.568	2.538*
	(−2.674)	(−1.685)	(−3.000)	(−1.859)		(1.263)	(1.828)
Treatment$_{s,t+1}$	−1.702	−0.700	−1.786*	−0.700		0.937	3.110**
	(−1.575)	(−0.709)	(−1.711)	(−0.782)		(0.577)	(2.126)
Treatment$_{s,t+2}$	0.793	0.001	0.688	0.001		2.322	2.078
	(1.566)	(0.002)	(1.457)	(0.002)		(1.620)	(1.335)
ΔControls$_{c,t}$	Y	Y	Y	Y	ΔControls$_{c,t}$	Y	Y
County	Y	Y	Y	Y	County	Y	Y
Bank × Year × CSA	Y	N	Y	N	Year × CSA	Y	N
Bank × Year × County-pair	N	Y	N	Y	Year × County-pair	N	Y
Observations	551,153	685,844	138,270	254,213	Observations	7,818	15,108
Adjusted R^2	0.163	0.154	0.111	0.114	Adjusted R^2	0.376	0.211

Table 7: The effects of pension reforms on bank deposits and stock investments. This table estimates the effects of state public pension reforms on bank deposits and stock investments over 2009-2018. Δ log(Deposits)$_{i,b,c,t}$ is log change in percentage of deposits of branch i of bank b in county c at the end of June in year t, and Δ log(Dividends)$_{c,t}$ is log change in percentage of dividends in county c at year t (both are winsorized at 0.5% in each tail). The indicator variable Treatment$_{s,t}$ is set to 1 if state s enacts a reform in year t (and is otherwise set to 0); Treatment$_{s,t+\tau}$ is the same as Treatment$_{s,t}$ but τ years *after* the reform in year t. Control variables include log changes in GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1), (3) and (5), and at both state and border segment levels for regressions (2), (4) and (6). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Panel A: Liabilities + Equity				
(%)	Δ log(Liabilities$_{b,t}$)	Δ log(Deposits$_{b,t}$)	Δ log(RetailDeposits$_{b,t}$)	Δ log(Wholesale$_{b,t}$)	Δ log(Equity$_{b,t}$)
	(1)	(2)	(3)	(4)	(5)
Exposure$_{b,t}$	-0.444^{***}	-0.554^{***}	-0.560^{***}	2.696	0.446^{**}
	(-2.918)	(-3.495)	(-3.036)	(0.990)	(2.353)
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	56,510	56,505	56,419	42,351	56,484
Adjusted R^2	0.213	0.204	0.163	0.234	0.261

	Panel B: Assets				
(%)	Δ log(Assets)$_{b,t}$	Δ log(CiLoan)$_{b,t}$	Δ log(PersLoan)$_{b,t}$	Δ log(ReLoan)$_{b,t}$	Δ log(Securities)$_{b,t}$
	(1)	(2)	(3)	(4)	(5)
Exposure$_{b,t}$	-0.357^{**}	-0.878^{**}	0.306	0.231	-1.823^{*}
	(-2.565)	(-2.119)	(0.637)	(1.066)	(-1.833)
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	56,510	55,469	55,750	56,104	30,466
Adjusted R^2	0.233	0.063	0.053	0.204	0.092

Table 8: The effects of pension reforms on banks' balance sheets. This tables estimates the effects of pension reforms on banks' liability and asset items over 2009-2018. Δ log($Y_{b,t}$) is the log changes in percentage of bank b's item Y at year t, winsorized at 0.5% in each tail. Exposure$_{b,t}$ is the weighted sum of Treatment$_{s,t}$ by the proportion of bank b' branches located in state s, see Equation (8). Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the bank level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Non-pension-reforming states		All states	
	$\log(\text{New Lending}^{\$}_{b,c,t})$	$\log(\text{New Lending}^{\#}_{b,c,t})$	$\log(\text{New Lending}^{\$}_{b,c,t})$	$\log(\text{New Lending}^{\#}_{b,c,t})$
	(1)	(2)	(3)	(4)
$\text{Exposure}_{b,t}$	-0.325^{**}	-0.187^{**}	-0.042	0.022
	(-2.274)	(-1.988)	(-0.747)	(0.859)
$\log(\text{Age}_{b,t})$	-0.025	-0.410	-0.219	-0.488
	(-0.054)	(-0.888)	(-0.574)	(-1.323)
$\log(\text{Assets}_{b,t})$	0.346	0.081	0.163	-0.035
	(1.398)	(0.432)	(0.983)	(-0.269)
County × Year	Y	Y	Y	Y
Bank	Y	Y	Y	Y
Observations	273,292	273,292	667,233	667,233
Adjusted R^2	0.431	0.467	0.400	0.389

Table 9: The effects of pension reforms on banks' lending to small businesses. This table estimates the effects of pension reforms on banks' lending under \$1 million to small businesses over 2009-2018. The sample covers non–pension-reforming states only in the firs two columns, and all states in the last two columns. $\text{New Lending}^{\$}_{b,c,t}$ ($\text{New Lending}^{\#}_{b,c,t}$) is newly originated loans under \$1 million, in dollars (in quantities), to small businesses of bank b in county c of year t, and is winsorized at 0.5% in each tail. $\text{Exposure}_{b,t}$ is the weighted sum of $\text{Treatment}_{s,t}$ by the proportion of bank b' branches located in state s, see Equation (8). Bank controls include bank's age and assets, both in log scales. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the bank level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	log(New Lending$_{c,t}$)	Δ log(Est$_{c,t}$)	Δ log(Emp$_{c,t}$)	Δ log(Emp$^{BD}_{c,t}$)	Δ log(Emp$^{NT}_{c,t}$)	Δ log(Wage$^{NT}_{c,t}$)
	(1)	(2)	(3)	(4)	(5)	(6)
CountyExposure$_{c,t}$	-1.618^{***}	-0.337^{***}	0.0635	-0.326	-1.972^{***}	-4.994^{*}
	(-3.218)	(-7.851)	(1.329)	(-1.408)	(-2.678)	(-1.687)
LendingPropensity$_{c,t}$	3.070^{***}	0.0298	0.0281	0.0140	0.411^{*}	1.331
	(9.855)	(1.549)	(1.307)	(0.121)	(1.693)	(1.637)
Δ log(Pop$_{c,t}$)	25.163	17.433^{***}	29.340^{***}	38.116	-1.220	-136.874
	(0.385)	(3.283)	(3.765)	(1.055)	(-0.017)	(-0.470)
Δ log(GDP$_{c,t}$)	9.489	1.694^{***}	8.453^{***}	2.875	28.648^{***}	101.673^{***}
	(1.420)	(3.548)	(11.335)	(0.844)	(3.164)	(2.828)
Year	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Observations	11,065	10,831	10,831	10,831	10,826	10,826
Adjusted R^2	0.973	0.274	0.248	-0.0705	0.0208	-0.0721

Table 10: Economy consequences of pension reforms in non–pension-reforming states. This table estimates the effects of pension reforms on counties' economic activity over 2009-2018. log(New Lending$_{c,t}$), Δ log(Est$_{c,t}$), Δ log(Emp$_{c,t}$) and Δ log(Wage) are the total new loans under \$1 million to small business in log scales, log changes in establishments or employment or wages in county c of year t, and all are winsorized at 0.5% in each tail. Empty BD and NT superscript represent all sectors, bank-dependent sectors and non-tradable sectors,respectively. Control variables include log changes in GDP and population, both at the county-year level. CountyExposure$_{c,t}$ is the weighted sum of all banks' exposure to pension reforms in county c, where the weights are based on the lending shares of banks in county c prior to the reform, see Equation (9). LendingPropensity$_{c,t}$ is similar to CountyExposure$_{c,t}$ without considering banks' exposure to pension reforms, see Equation (10). Pop and GPD are county-level population and GDP, respectively. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the county level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	$\Delta\log\text{Est}_{c,t}$		$\Delta\log\text{Emp}^{NT}_{c,t}$		$\Delta\log\text{Wage}^{NT}_{c,t}$	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
$\log(\text{New Lending})_{c,t}$	0.001		0.005		0.006	
	(1.252)		(0.321)		(0.097)	
$\log(\widehat{\text{New Lending}})_{c,t}$		0.192***		1.129**		2.858
		(3.083)		(2.085)		(1.515)
$\text{LendingPropensity}_{c,t}$	0.003	−0.566***	0.261	−3.084*	0.973	−7.516
	(0.155)	(−2.888)	(1.044)	(−1.814)	(1.138)	(−1.272)
$\Delta\log(\text{Pop}_{c,t})$	18.511***	13.432	5.181	−22.048	−120.497	−189.603
	(3.433)	(1.077)	(0.072)	(−0.237)	(−0.414)	(−0.611)
$\Delta\log(\text{GDP}_{c,t})$	1.609***	−0.183	28.194***	17.385	100.592***	73.158
	(3.345)	(−0.118)	(3.103)	(1.288)	(2.794)	(1.620)
Year	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Observations	10,831	10,831	10,826	10,826	10,826	10,826
Adjusted R^2	0.265		0.0191		−0.0730	

Table 11: Real consequences—reduced-form and instrument variable results. This table estimates the relationship between lending and local economic consequences under both reduced-form and instrument variable form. $\Delta\log(\text{Est}_{c,t})$, $\Delta\log(\text{Emp}_{c,t})$ and $\Delta\log(\text{Wage})$ are log changes in establishments or employment or wages in county c of year t, and all are winsorized at 0.5% in each tail. Empty and NT superscript represent all sectors and non-tradable sectors,respectively. $\log(\text{New Lending}_{c,t})$ is the total new loans under \$1 million to small business in log scales, and $\log(\widehat{\text{New Lending}})_{c,t}$ is instrumented new lending using $\text{CountyExposure}_{c,t}$ (the predicted values in regression (1) of Table 10). Control variables include log changes in GDP and population, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the county level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

Public Pension Deficits and Bank Deposits
Online Appendix

A Conceptual Framework

Consider a simple two-period model composed of a representative agent and a government. The agent is endowed with income I_1 (resp. I_2) at time T_1 (resp. T_2). The government has pension deficits cumulated from (un-modeled) previous generations. The agent foresees that, to cover funding shortfalls, the government needs to raise tax rates or cut the budget for public services at T_2. For this reason, the agent' disposable income at T_2 gets reduced to $I_2^D = I_2 \times (1 - \xi)$, where ξ is either tax rates or expenses for necessities, such as schooling and medical services, and is positively correlated with position deficits. Accordingly, I interpret ξ as the size of the pension deficit.

The agent chooses how much to consume at each period, c_1 and c_2, and how much to save via a risky market portfolio and risk-less bank deposits, M and D, at T_1. The uncertain gross return on the make portfolio is $\tilde{R}_M$ and the deposit gross rate is R_D. His total savings at T_1 is $S = M + D$. Suppose the agent has the utility function $U(c)$ with the following properties:

- $U^{'}(c) = \frac{\partial U(c)}{\partial c} > 0$ and $U^{''}(c) = \frac{\partial U^{'}(c)}{\partial c} < 0$;
- $A(c) := -\frac{U^{''}(c)}{U^{'}(c)}$ and $\frac{\partial A(c)}{\partial c} <= 0$.

Then his optimization problem is characterized as follows:

$$
\begin{aligned}
\max_{S,D} \quad & U(c_1) + \beta \mathbb{E}[U(c_2)] \\
s.t. \quad & c_1 = I_1 - S \\
& c_2 = I_2(1-\xi) + DR_D + (S-D)\tilde{R}_M.
\end{aligned}
\tag{OA.A}
$$

The framework leads to the following prediction:

Theorem 1. *A representative agent in an economy with larger pension deficits allocate more savings to bank deposits and less to stocks:* $\frac{\partial D}{\partial \xi} \geq 0$ *and* $\frac{\partial M}{\partial \xi} \leq 0$.

The intuition is as follows. When the future income reduces, the consumption decreases due to the representative agent's desire to smooth consumption over time. It follows from the assumption of $\frac{\partial A(c)}{\partial c} <= 0$ that the agent becomes more risk averse (with a high absolute risk aversion). Hence, she saves more in the safe asset (bank deposits) and less in the risk asset (stocks). Alternatively, one can view that the agent' wealth comprises financial wealth (deposits and stocks) and labor income. Because labor income is relatively safe, it can be deemed as a substitute for riskless deposits. Hence, when the agent' labor income decreases, she should allocate more of financial wealth to bank deposits and less to stocks.

It is worth pointing out that the assumption of $\frac{\partial A(c)}{\partial c} <= 0$ is very general. Any utility function with constant relative risk aversion (CRRA) and decreasing relative risk aversion (DRRA) satisfies the assumption. This assumption is supported by empirical evidence; see, for example, Morin and Suarez (1983); Ogaki and Zhang (2001), among others.

Proof. I will prove that $\frac{\partial D}{\partial I_2^D} \leq 0$ and $\frac{\partial M}{\partial I_2^D} \geq 0$, and it follows from the relationship $I_2^D = (1-\xi)$ that $\frac{\partial D}{\partial \xi} \geq 0$ and $\frac{\partial M}{\partial \xi} \leq 0$.

The first order conditions of Equation (OA.A) with respect to S and D are

$$0 = \mathbb{E}[U^{'}(c_2)(\tilde{R}_M - R_D)] \tag{OA.B}$$

$$U^{'}(c_1) = \beta\mathbb{E}[U^{'}(c_2)\tilde{R}_M] = \beta\mathbb{E}[U^{'}(c_2)R_D]. \tag{OA.C}$$

Taking derivative with respect to I_2^D for Equation (OA.C) yields

$$U^{''}(c_1)\Big(-\frac{\partial S}{\partial I_2^D}\Big) = \mathbb{E}\Big[\beta R_D U^{''}(c_2)\Big(1 + \frac{\partial S}{\partial I_2^D}\tilde{R}_M + \frac{\partial D}{\partial I_2^D}(R_D - \tilde{R}_M)\Big)\Big]$$

$$\implies \frac{\partial S}{\partial I_2^D} = -\frac{\mathbb{E}\Big[\beta Z R_D\Big(1 + \frac{\partial D}{\partial I_2^D}(R_D - \tilde{R}_M)\Big)\Big]}{1 + \mathbb{E}[\beta Z\tilde{R}_M R_D]}, \quad \text{where } Z := \frac{U''(c_2)}{U''(c_1)}.$$

Taking derivative with respect to I_2^D for Equation (OA.B) and then plugging in the expression of $\frac{\partial S}{\partial I_2^D}$ yields

$$0 = \mathbb{E}\Big[(\tilde{R}_M - R_D)U^{''}(c_2)\Big(1 - \frac{\mathbb{E}\Big[\beta Z R_D\Big(1 + \frac{\partial D}{\partial I_2^D}(R_D - \tilde{R}_M)\Big)\Big]}{1 + \mathbb{E}[\beta Z\tilde{R}_M R_D]}\tilde{R}_M + \frac{\partial D}{\partial I_2^D}(R_D - \tilde{R}_M)\Big)\Big]$$

$$\implies \frac{\partial D}{\partial I_2^D} = -\frac{\mathbb{E}\Big[U''(c_2)(\tilde{R}_M - R_D)\Big(1 + \mathbb{E}[\beta Z\tilde{R}_M R_D] - \mathbb{E}[\beta Z R_D]\tilde{R}_M\Big)\Big]}{\mathbb{E}\Big[U''(c_2)(\tilde{R}_M - R_D)\Big(\mathbb{E}[\beta Z\tilde{R}_M R_D^2] - \mathbb{E}[\beta Z R_D^2]\tilde{R}_M - (\tilde{R}_M - R_D)\Big)\Big]} := -\frac{Numerator}{Denominator}.$$

Note that

$$\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)\tilde{R}_M] = \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)R_D].$$

I reorganize terms in *Denominator*:

$$
\begin{aligned}
Denominator &= \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]\mathbb{E}[\beta Z\tilde{R}_M R_D^2] - \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] \\
&\quad - \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)\tilde{R}_M]\mathbb{E}[\beta Z R_D^2] \\
&= \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]\mathbb{E}[\beta Z\tilde{R}_M R_D^2] - \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] \\
&\quad - \mathbb{E}[\beta Z R_D^2]\Big(\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)R_D]\Big) \\
&= \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]\mathbb{E}[\beta Z R_D^2(\tilde{R}_M - R_D)] - (1 + \mathbb{E}[\beta Z R_D^2])\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] \\
&= \frac{\beta R_D^2}{U''(c_1)}\Big(\mathbb{E}^2[-U^{''}(c_2)(\tilde{R}_M - R_D)] - \mathbb{E}[-U^{''}(c_2)]\mathbb{E}[-U^{''}(c_2)(\tilde{R}_M - R_D)^2]\Big) \\
&\quad - \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2].
\end{aligned}
$$

Based on Cauchy-Schwarz inequality, we know $\mathbb{E}^2[XY] \leq \mathbb{E}[X^2]\mathbb{E}[Y^2]$. Let $X = \sqrt{-U''(c_2)}$ and $Y = \sqrt{-U''(c_2)(\tilde{R}_M - R_D)^2}$, we have $\mathbb{E}^2[-U^{''}(c_2)(\tilde{R}_M - R_D)] - \mathbb{E}[-U^{''}(c_2)]\mathbb{E}[-U^{''}(c_2)(\tilde{R}_M - R_D)^2] \leq 0$. Since $U^{''}(c) < 0$, we have *Denominator* ≥ 0.

Similarly,

$$
\begin{aligned}
Numerator &= \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)] + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]\mathbb{E}[\beta Z\tilde{R}_M R_D] \\
&\quad - \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)\tilde{R}_M]\mathbb{E}[\beta Z R_D] \\
&= \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)] + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]\mathbb{E}[\beta Z\tilde{R}_M R_D] \\
&\quad - \mathbb{E}[\beta Z R_D]\Big(\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)R_D]\Big) \\
&= \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]\mathbb{E}[\beta Z(\tilde{R}_M - R_D)R_D] - \mathbb{E}[\beta Z R_D]\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2] \\
&\quad + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)] \\
&= \frac{\beta R_D}{U''(c_1)}\Big(\mathbb{E}^2[U^{''}(c_2)(\tilde{R}_M - R_D)] - \mathbb{E}[U^{''}(c_2)]\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)^2]\Big) \\
&\quad + \mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)].
\end{aligned}
$$

Following Cauchy-Schwarz inequality, the first term is positive. Once then second term is positive as well, we have $\frac{\partial D}{\partial I_2^D} \leq 0$. The remaining is to prove $\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)] \geq 0$. Using definition of

$A(c)$, the expectation can be written as

$$\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)] = -\mathbb{E}[A(c_2)U^{'}(c_2)(\tilde{R}_M - R_D)].$$

In evaluating this expected value, let us discuss case by case. Remind that $c_2 = I_2^D + (S - D)\tilde{R}_M + DR_D = I_2^D + SR_D + (S - D)(\tilde{R}_M - R_D) = I_2^D + S\tilde{R}_M - D(\tilde{R}_M - R_D)$.

1. if and when $S - D \geq 0$ and $\tilde{R}_M \geq R_D$, then $c_2 \geq I_2^D + SR_D$. Using the first order condition and the decreasing property of $A(c)$, we have

$$-\mathbb{E}[A(c_2)U^{'}(c_2)(\tilde{R}_M - R_D)] \geq -\mathbb{E}[A(I_2^D + SR_D)U^{'}(c_2)(\tilde{R}_M - R_D)] = 0;$$

2. if and when $S - D \geq 0$ and $\tilde{R}_M \leq R_D$, then $c_2 \leq I_2^D + SR_D$. Using the first order condition and the decreasing property of $A(c)$, we have

$$-\mathbb{E}[A(c_2)U^{'}(c_2)(\tilde{R}_M - R_D)] \geq -\mathbb{E}[A(I_2^D + SR_D)U^{'}(c_2)(\tilde{R}_M - R_D)] = 0;$$

3. if and when $S - D \leq 0$ and $\tilde{R}_M \geq R_D$, then $c_2 \geq I_2^D + S\tilde{R}_M \geq SR_D$. Using the first order condition and the decreasing property of $A(c)$, we have

$$-\mathbb{E}[A(c_2)U^{'}(c_2)(\tilde{R}_M - R_D)] \geq -\mathbb{E}[A(I_2^D + SR_D)U^{'}(c_2)(\tilde{R}_M - R_D)] = 0;$$

4. if and when $S - D \leq 0$ and $\tilde{R}_M \leq R_D$, then $c_2 \leq I_2^D + S\tilde{R}_M \leq SR_D$. Using the first order condition and the decreasing property of $A(c)$, we have

$$-\mathbb{E}[A(c_2)U^{'}(c_2)(\tilde{R}_M - R_D)] \geq -\mathbb{E}[A(I_2^D + SR_D)U^{'}(c_2)(\tilde{R}_M - R_D)] = 0.$$

Therefore, $Numerator \geq 0$ and $\frac{\partial D}{\partial I_2^D} \leq 0$.

Following the same procedure, one can get

$$\begin{aligned}\frac{\partial M}{\partial I_2^D} &= \frac{\partial S}{\partial I_2^D} - \frac{\partial D}{\partial I_2^D} \\ &= -\frac{\mathbb{E}[U^{''}(c_2)(\tilde{R}_M - R_D)]}{\mathbb{E}[U''(c_2)(\tilde{R}_M - R_D)^2] + \frac{\beta R_D^2}{U''(c_1)}\Big(\mathbb{E}[U''(c_2)]\mathbb{E}[U''(c_2)(\tilde{R}_M - R_D)^2] - \mathbb{E}^2[U''(c_2)(\tilde{R}_M - R_D)]\Big)} \geq 0.\end{aligned}$$

□

B Effect on Households' Savings Allocation: Robustness Checks

B.1 Graphs for Association Relationship

I illustrate the relationship between pension deficits and allocation of savings in the raw data (without the contiguous border county strategy and within-bank strategy). I first sort states into eleven bins by pension deficits (scaled by GDP) each year and then regress county-level Deposits/GPD (resp., Dividends/Income) on the level of bins with county and year fixed effects. Figure OA.2 plots the coefficients of bins with the lowest bin as the benchmark group. The coefficient of bin i, in essential, measures the difference of average Deposits/GPD (or Dividends/Income) between bin i and the benchmarking bin, adjusted for county and year fixed effects. Panel (a) shows a positive association between pension deficits and bank deposits scaled by GDP, while panel (b) reveals a negative association between pension deficits and dividends scaled by income, consistent with my hypothesis.

B.2 Decomposition of Pension Deficits

I decompose pension deficits in Equation (1) into pension assets scaled by GDP ($\text{Assets}_{s,t}^{GDP}$) and pension liabilities scaled by GDP ($\text{Liabilities}_{s,t}^{GDP}$) and reexamine their relationship with households' allocation of savings following identification strategy in Section 4.2. This exercise aims to understand which component drives the findings for pension deficits shown in Table 3, 4 and 5.

Table OA.4 reports results of both baseline analysis and the DiD test for the GASB updates for bank deposits. The results show that an increase in pension assets (liabilities) reduces (enlarges) pension deficits, and so households allocate less (more) savings to bank deposits. Following the GASB updates, households respond more strongly to both pension assets and liabilities. The evidence are align with my main findings.

Table OA.5 reports results for county dividends. According to the hypothesis, the coefficients of $\text{Assets}_{s,t}^{GDP}$ and $\text{Liabilities}_{s,t}^{GDP}$ should be opposite from Table OA.4, i.e., households invest more (less) in stocks when pension assets (liabilities) increase. The regression coefficients on $\text{Liabilities}_{s,t}^{GDP}$ are in line with the conjecture, while the evidence on $\text{Assets}_{s,t}^{GDP}$ are relatively weak.

Overall, the evidence suggests that households are capable to assess the assets and liabilities of pension plans correctly. Moreover, pension liabilities play a slightly more critical role in determining households' savings allocation.

B.3 Deposit Rates

Since small branches do not set deposit rates and follow nearby rate-setting branches that belong to the same bank, there is little difference in rates offered by a given bank's branches located within a contiguous county pair. Therefore, a regression with bank × countypair × year fixed effects does not capture any variation. In the main analysis, I relax region restrictions and include bank × year only. That analysis controls for lending opportunities but not for economic fundamentals of different regions. Here, I offer the complementary test that controls for economic fundamentals rather than lending opportunities. The regressions include countypair × year fixed effects and compare deposit rates of branches (belonging to different banks) in contiguous border counties:

$$\text{Rate}_{i,b,c,t}^{x} = \alpha_{cp,t} + \alpha_c + \beta \text{Pension Deficits}_{s,t}^{\text{GDP}} + Controls_{c,t} + \varepsilon_{i,b,c,t}.$$

Table OA.6 reports the regression results. The estimated $beta$ of different deposit products are negative, and are statistically significant for 12MCD10K, 24MCD10K, 36MCD10K. The results support my conjecture that households' supplies of deposits increase in response to larger state pension deficits, and so banks offer lower deposit rates.

B.4 Full Sample from 2002 to 2018

The main analysis uses post-crisis data only. I extend the sample to years with data on state pension deficits. Table OA.7 presents baseline results for both deposits and stock investments. Consistent with the hypothesis, an increase in pension deficits leads to more bank deposits and fewer stock investments. However, the effect sizes are smaller compared to the post-crisis sample reported in Table 3 and 4. For the sample covering 2002-208 only, regression coefficients have the same signs as the full sample but are not statistically significant. I believe this is because pension deficits of state public pension plans were relatively low—and so did not garner much public attention—prior to the financial crisis.

C Economic Consequences: Robustness Checks

C.1 Loans to Small Businesses with Gross Annual Revenues less than $1 Million

Data on small business lending contains (i) new loans of less than $ 1 million and (ii) new loans granted to businesses with gross annual revenues of less than $1 million. The latter is a subset of the former. This section repeats the analysis of Tables 9, 10 and 11 for the second type of loans. The results are reported in Tables OA.8, OA.9 and OA.10. Similar to the main findings, the results show that banks with higher exposure to pension reforms extend less credit to small businesses in non–pension-reforming states, and the lending cuts sabotage local investment and employment.

C.2 All Reforms

In the main analysis, I, in order to avoid mixed effects, exclude pension reforms that reduce benefits for the current employees. I include all reforms in the analysis and show that results are slightly weaker yet largely consistent with previous findings; see Tables OA.11–OA.16.

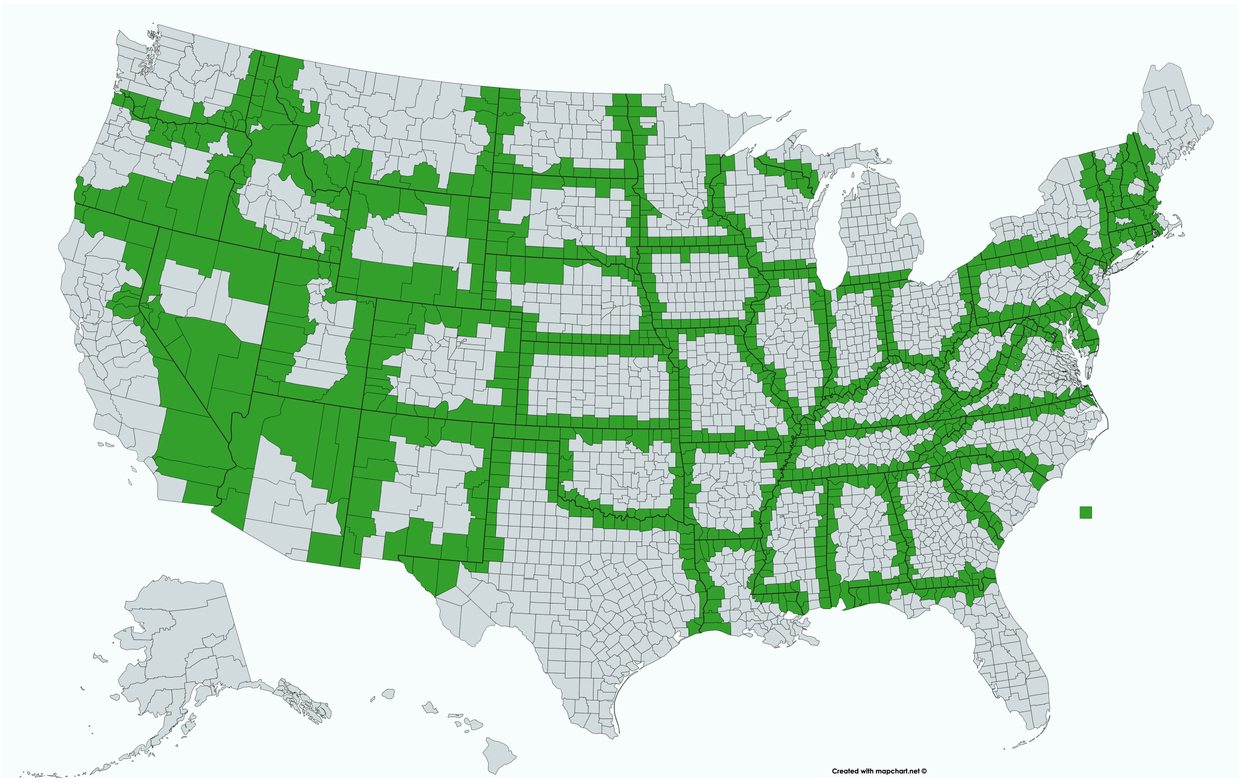


Figure OA.1: Map of contiguous border county-pairs. This figure maps the contiguous border county-pairs.

(a) Deposits/GDP

(b) Dividends/Income

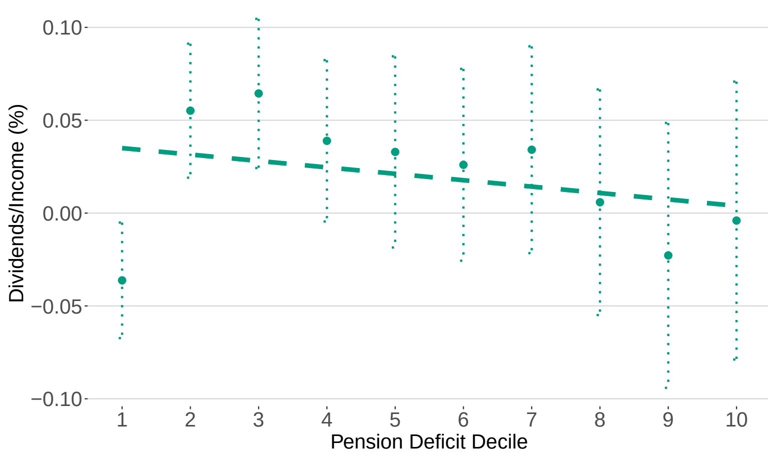


Figure OA.2: Bank deposits and dividends by state public pension deficit deciles. The figures plot the coefficients on $\text{bin}_{s,t}$ of the following regression:

$$Y_{c,t} = \alpha_c + \alpha_t + \text{bin}_{s,t} + \varepsilon_{c,t},$$

where $Y_{c,t}$ is either the county-level Deposits/GDP (a) or Dividends/Income (b), and $\text{bin}_{s,t}$ is the level of bin for states sorted by pension deficits each year. Bin 1 (bin 10) corresponds to states with the lowest (highest) public pension deficits.

	Deposit rates$_{i,b,c,t}$							
	INTCK0K	MM25K	06MCD10K	12MCD10K	24MCD10K	36MCD10K	48MCD10K	60MCD10K
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pension Deficits$_{s,t}$	0.090	0.071	-0.161^{*}	-0.220^{**}	-0.367^{***}	-0.313^{**}	-0.268^{*}	-0.376^{*}
	(1.486)	(0.953)	(−1.893)	(−2.366)	(−3.325)	(−2.490)	(−1.814)	(−1.756)
Log(GDP$_{c,t}$)	0.014	−0.012	0.038	0.029	0.062^{*}	0.062^{*}	0.075^{**}	0.014
	(1.434)	(−0.526)	(1.026)	(0.859)	(1.892)	(1.945)	(2.126)	(0.331)
Log(PCI$_{c,t}$)	-0.042^{**}	-0.091^{*}	−0.067	−0.048	−0.085	−0.112	-0.124^{*}	-0.138^{*}
	(−2.222)	(−1.805)	(−0.816)	(−0.677)	(−1.226)	(−1.562)	(−1.721)	(−1.813)
Bank × Year	Y	Y	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y	Y	Y
Observations	21,114	20,945	21,733	21,747	21,348	21,282	20,201	20,639
Adjusted R^2	0.801	0.891	0.934	0.942	0.945	0.949	0.954	0.952

Table OA.1: State public pension deficits and bank deposit rates. This table estimates the effects of state public pension deficits on bank deposit rates of various deposit products over 2009-2018. Deposit rates$_{i,b,c,t}$ is the annual average deposit rate, in percent, of corresponding product set by branch i of bank b in county c at year t, and is winsorized at 0.5% in each tail. Pension Deficits$^{GDP}_{s,t}$ represents the pension deficit in state s in year t, as defined by Equation (1). Control variables include GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	Pension Deficits$_{c,s,t}^{GDP}$	
	(1)	(2)
$\mathbb{1}$(High PD$_{c,t}$) ×Post$_t$	0.572	0.518
	(1.382)	(1.478)
$\mathbb{1}$(High PD$_{c,t}$)	1.211***	0.792**
	(2.597)	(2.305)
County	Y	Y
Year × CSA	Y	N
Year × County-pair	N	Y
Observations	15,455	13,080
Adjusted R^2	0.994	0.992

Table OA.2: The changes in public pension deficits around the 2015 GASB updates. This table examines whether public pension deficits of contiguous border counties change around the 2015 GASB updates using the DiD method. Pension Deficits$_{c,s,t}^{\mathrm{GDP}}$ represents the pension deficit in county c of state s in year t, as defined by Equation (1). $\mathbb{1}$(High PD$_{c,t}$) equals 1 if the pension deficit of county c is above the median of those in one CSA region or above the counterparty for one county pair, and 0 otherwise. Post$_t$ equals 1 for years 2015–2017 and 0 for 2013–2014. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regression (1), and at both state and border segment levels for regression (2). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	$\text{Assets}_{s,t}^{GDP}$	$\text{Liabilities}_{s,t}^{GDP}$	$\text{Pension Deficits}_{s,t}^{GDP}$
	(1)	(2)	(3)
$\text{Treatment}_{s,t-2}$	−0.039	−0.231	−0.191
	(−0.155)	(−0.509)	(−0.518)
$\text{Treatment}_{s,t-1}$	−0.203	−0.148	0.055
	(−1.021)	(−0.414)	(0.190)
$\text{Treatment}_{s,t}$	-0.296^{*}	-0.782^{***}	-0.485^{**}
	(−1.781)	(−2.621)	(−1.991)
$\text{Treatment}_{s,t+1}$	-0.407^{**}	-0.785^{**}	−0.378
	(−2.334)	(−2.511)	(−1.478)
$\text{Treatment}_{s,t+2}$	−0.170	-0.644^{**}	-0.474^{*}
	(−0.970)	(−2.047)	(−1.843)
State	Y	Y	Y
Year	Y	Y	Y
Observations	454	454	454
Adjusted R^2	0.970	0.975	0.968

Table OA.3: The effects of pension reforms on state pension status. This table estimates the impacts of pension reforms on state pension status over 2009-2018. $\text{Assets}_{s,t}^{GDP}$, $\text{Liabilities}_{s,t}^{GDP}$ and $\text{Deficits}_{s,t}^{GDP}$ are state s' public pension asset, liabilities and deficits, all scaled by state GDP at year t. The unit of analysis is state-year. The indicator variable $\text{Treatment}_{s,t}$ is set to 1 if state s enacts a reform in year t (and is otherwise set to 0); $\text{Treatment}_{s,t+\tau}$ is the same as $\text{Treatment}_{s,t}$ but τ years *after* the reform in year t. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	log(Deposit$_{i,b,c,t}$)			
	≥ 2 States			
	(1)	(2)	(3)	(4)
$\text{Assets}^{GDP}_{s,t}$	-3.891^{***}	-3.276^{**}	-1.388^{**}	-1.577
	(-3.619)	(-2.209)	(-2.163)	(-1.384)
$\text{Liabilities}^{GDP}_{s,t}$	1.220^{***}	1.276^{***}	1.276^{***}	0.237
	(4.329)	(3.587)	(2.699)	(0.433)
$\text{Assets}^{GDP}_{s,t}\times\text{Post}_t$			-0.465^{***}	-0.605^{***}
			(-3.374)	(-3.738)
$\text{Liabilities}^{GDP}_{s,t}\times\text{Post}_t$			0.235^{***}	0.243^{***}
			(3.772)	(3.207)
$\text{Controls}_{c,t}$	Y	Y	Y	Y
$\text{Controls}_{c,t}\times\text{Post}_t$	N	N	Y	Y
Bank × Year × CSA	Y	N	Y	N
Bank × Year × County-pair	N	Y	N	Y
County	Y	Y	Y	Y
Observations	167,008	308,691	84,945	156,501
Adjusted R^2	0.304	0.293	0.305	0.298

Table OA.4: Bank deposits and state public pension deficits. This table estimates the effects of state public pension deficits on bank deposits over 2009-2018. The sample covers banks with branches located in at least two states. log(Deposits)$_{i,b,c,t}$ is the deposits, in log scale, at branch i of bank b in county c at the end of June in year t, and is winsorized at 0.5% in each tail. $\text{Assets}^{GDP}_{s,t}$ ($\text{Liabilities}^{GDP}_{s,t}$) is the state s' public pension assets (liabilities), scaled by state GDP at year t. Control variables include GDP and per capita income, both at the county-year level. Post_t equals 1 for years 2015–2017 and 0 for 2013–2014. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1) and (3), and at both state and border segment levels for regressions (2) and (4). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	log(Dividends$_{c,t}$)			
	(1)	(2)	(3)	(4)
Assets$^{GDP}_{s,t}$	0.376	-1.315*	-0.627	-0.343
	(1.097)	(-1.960)	(-1.415)	(-0.713)
Liabilities$^{GDP}_{s,t}$	-0.598**	-0.596**	-0.384	-0.566
	(-2.555)	(-2.042)	(-0.739)	(-1.418)
Assets$^{GDP}_{s,t}$×Post$_t$			0.0382	0.0768
			(0.576)	(0.811)
Liabilities$^{GDP}_{s,t}$×Post$_t$			-0.0298	-0.0766**
			(-0.887)	(-2.074)
Controls$_{c,t}$	Y	Y	Y	Y
Controls$_{c,t}$×Post$_t$	N	N	Y	Y
Bank × Year × CSA	Y	N	Y	N
Bank × Year × County-pair	N	Y	N	Y
County	Y	Y	Y	Y
Observations	8,993	20,142	5,617	12,594
Adjusted R^2	0.996	0.992	0.998	0.997

Table OA.5: Stock investments and state public pension deficits. This table estimates the effects of state public pension deficits on stock investments over 2010-2007. log(Dividend)$_{c,t}$ is the dividend income in log scales of county c during year t, and is winsorized at 0.5% in each tail. Assets$^{GDP}_{s,t}$ (Liabilities$^{GDP}_{s,t}$) is the state s' public pension assets (liabilities), scaled by state GDP at year t. Control variables include GDP and per capita income, both at the county-year level. Post$_t$ equals 1 for years 2015–2017 and 0 for 2013–2014. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1) and (3), and at both state and border segment levels for regressions (2) and (4). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Deposit rates$_{i,b,c,t}$							
	INTCK0K	MM25K	06MCD10K	12MCD10K	24MCD10K	36MCD10K	48MCD10K	60MCD10K
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pension Deficits$_{s,t}$	−0.258	−0.458	−0.349	−0.561**	−0.489**	−0.625**	−0.405	−0.371
	(−1.513)	(−1.512)	(−1.255)	(−2.194)	(−2.498)	(−2.368)	(−1.600)	(−1.011)
Log(GDP$_{c,t}$)	0.026	0.076*	0.046	0.041	0.064	0.020	−0.031	−0.031
	(0.998)	(1.807)	(0.902)	(0.757)	(1.189)	(0.332)	(−0.417)	(−0.375)
Log(PCI$_{c,t}$)	0.002	−0.002	−0.059	−0.042	−0.058	−0.028	0.090	0.170
	(0.043)	(−0.019)	(−0.550)	(−0.493)	(−0.599)	(−0.278)	(0.898)	(1.391)
County-pair × Year	Y	Y	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y	Y	Y
Bank	Y	Y	Y	Y	Y	Y	Y	Y
Observations	66,797	65,748	69,216	69,339	66,813	64,640	58,221	58,708
Adjusted R^2	0.664	0.781	0.890	0.894	0.900	0.907	0.903	0.887

Table OA.6: Bank deposit rates and state public pension deficits. This table estimates the effects of state public pension deficits on bank deposit rates of various deposit products over 2009-2018. Deposit rates$_{i,b,c,t}$ is the annual average deposit rate, in percent, of corresponding product set by branch i of bank b in county c at year t, and is winsorized at 0.5% in each tail. Pension Deficits$_{s,t}^{\text{GDP}}$ represents the pension deficit in state s in year t, as defined by Equation (1). Control variables include GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state and border segment levels. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	log(Deposits$_{i,b,c,t}$) 2002-2018 & ≥ 2 States		log(Dividends$_{c,t}$) 2002-2017	
	(1)	(2)	(3)	(4)
Pension Deficits$_{s,t}^{GDP}$	0.629**	0.706**	−0.465	−0.348
	(2.414)	(2.327)	(−1.464)	(−1.381)
log(GDP$_{c,t}$)	0.062	−0.186	0.177***	0.075**
	(0.351)	(−0.780)	(3.948)	(2.122)
log(PCI$_{c,t}$)	−0.452***	−0.010	0.564***	0.196*
	(−3.191)	(−0.064)	(5.101)	(1.800)
Bank × Year × CSA	Y	N	Y	N
Bank × Year × County-pair	N	Y	N	Y
County	Y	Y	Y	Y
Observations	247,958	450,106	17,986	41,031
Adjusted R^2	0.290	0.280	0.989	0.986

Table OA.7: Allocation of savings and state public pension deficits. This table estimates the effects of state public pension deficits on the allocation of savings over 2002-2018. log(Deposits)$_{i,b,c,t}$ is the deposits, in log scale, at branch i of bank b in county c at the end of June in year t, and is winsorized at 0.5% in each tail. log(Dividend)$_{c,t}$ is the dividend income in log scales of county c during year t, and is winsorized at 0.5% in each tail. Pension Deficits$_{s,t}^{\text{GDP}}$ represents the pension deficit in state s in year t, as defined by Equation (1). Control variables include GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1) and (3), and at both state and border segment levels for regressions (2) and (4). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Non-pension-reforming states		All states	
	$\log(\text{New Lending}^{\$,rev1m}_{b,c,t})$	$\log(\text{New Lending}^{\#,rev1m}_{b,c,t})$	$\log(\text{New Lending}^{\$,rev1m}_{b,c,t})$	$\log(\text{New Lending}^{\#,rev1m}_{b,c,t})$
	(1)	(2)	(3)	(4)
$\text{Exposure}_{b,t}$	-0.446^{***}	-0.300^{***}	-0.088	-0.014
	(-4.451)	(-3.472)	(-1.523)	(-0.605)
$\log(\text{Age}_{b,t})$	0.627	0.028	0.277	-0.156
	(1.164)	(0.078)	(0.778)	(-0.755)
$\log(\text{Assets}_{b,t})$	0.579^{***}	0.104	0.189	-0.084
	(2.663)	(0.511)	(1.105)	(-0.549)
County × Year	Y	Y	Y	Y
Bank	Y	Y	Y	Y
Observations	189,447	189,447	470,103	470,103
Adjusted R^2	0.437	0.430	0.400	0.337

Table OA.8: The impacts of pension reforms on banks' lending to small businesses. This table estimates the effects of pension reforms on banks' lending to small businesses with gross annual revenues under $1 million over 2009-2018. The sample covers non–pension-reforming states only in the firs two columns, and all states in the last two columns. $\text{New Lending}^{\$,rev1m}_{b,c,t}$ ($\text{New Lending}^{\#,rev1m}_{b,c,t}$) is new lending, in dollars (in quantities), to small businesses with gross annual revenues under $1 million of bank b in county c of year t, and is winsorized at 0.5% in each tail. $\text{Exposure}_{b,t}$ is the weighted sum of $\text{Treatment}_{s,t}$ by the proportion of bank b' branches located in state s, see Equation (8). Bank controls include bank's age and assets, both in log scales. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the bank level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	log(New Lending$_{c,t}^{rev1m}$)	Δ log(Est$_{c,t}$)	Δ log(Emp$_{c,t}$)	Δ log(Emp$_{c,t}^{BD}$)	Δ log(Emp$_{c,t}^{NT}$)	Δ log(Wage$_{c,t}^{NT}$)
	(1)	(2)	(3)	(4)	(5)	(6)
CountyExposure$_{c,t}^{rev1m}$	-2.338^{***}	-0.290^{***}	0.094^{*}	-0.406	-1.634^{**}	-4.188
	(-3.888)	(-7.267)	(1.903)	(-1.385)	(-2.323)	(-1.583)
LendingPropensity$_{c,t}^{rev1m}$	3.522^{***}	0.026	-0.003	0.061	0.055	0.476
	(10.992)	(1.628)	(-0.150)	(0.593)	(0.275)	(0.680)
Δ log(Pop$_{c,t}$)	88.120	17.744^{***}	29.326^{***}	38.051	0.827	-132.682
	(1.026)	(3.329)	(3.754)	(1.054)	(0.011)	(-0.456)
Δ log(GDP$_{c,t}$)	-0.399	1.664^{***}	8.441^{***}	2.875	28.385^{***}	100.975^{***}
	(-0.045)	(3.493)	(11.316)	(0.845)	(3.127)	(2.807)
Year	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Observations	11,065	10,831	10,831	10,831	10,826	10,826
Adjusted R^2	0.955	0.273	0.248	-0.0701	0.0202	-0.0725

Table OA.9: Economy consequences of pension reforms in non–pension-reforming states. This table estimates the effects of pension reforms on counties' economic activity over 2009-2018. log(New Lending$_{c,t}^{rev1m}$), Δ log(Est$_{c,t}$), Δ log(Emp$_{c,t}$) and Δ log(Wage) are new loans to small businesses with gross annual revenues under \$1 million in log scales, log changes in establishments or employment or wages in county c of year t, and all are winsorized at 0.5% in each tail. Empty BD and NT superscript represent all sectors, bank-dependent sectors and non-tradable sectors,respectively. Control variables include log changes in GDP and population, both at the county-year level. CountyExposure$_{c,t}$ is the weighted sum of all banks' exposure to pension reforms in county c, where the weights are based on the lending shares of banks in county c prior to the reform, see Equation (9). LendingPropensity$_{c,t}$ is similar to CountyExposure$_{c,t}$ without considering banks' exposure to pension reforms, see Equation (10). Pop and GPD are county-level population and GDP, respectively. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the county level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	Δ log$\text{Est}_{c,t}$		Δ log$\text{Emp}^{NT}_{c,t}$		Δ log$\text{Wage}^{NT}_{c,t}$	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
log(New Lending)$^{rev1m}_{c,t}$	0.001		0.014		0.036	
	(1.564)		(0.902)		(0.578)	
log$(\widehat{\text{New Lending}})^{rev1m}_{c,t}$		0.114***		0.649**		1.664
		(3.506)		(1.974)		(1.462)
LendingPropensity$^{rev1m}_{c,t}$	0.0001	−0.378***	−0.111	−2.230*	0.0532	−5.379
	(0.006)	(−3.135)	(−0.528)	(−1.916)	(0.072)	(−1.363)
Δ log($\text{Pop}_{c,t}$)	18.417***	9.050	3.956	−50.678	−124.596	−264.658
	(3.410)	(0.847)	(0.055)	(−0.583)	(−0.428)	(−0.843)
Δ log($\text{GDP}_{c,t}$)	1.618***	1.437	28.114***	26.604**	100.283***	96.412**
	(3.364)	(1.274)	(3.097)	(2.453)	(2.789)	(2.451)
Year	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Observations	10,831	10,831	10,826	10,826	10,826	10,826
Adjusted R^2	0.265		0.0192		−0.0732	

Table OA.10: Real consequences—reduced-form and instrument variable results. This table estimates the relationship between lending and local economic consequences under both reduced-form and instrument variable form. Δ log($\text{Est}_{c,t}$), Δ log($\text{Emp}_{c,t}$) and Δ log(Wage) are log changes in establishments or employment or wages in county c of year t, and all are winsorized at 0.5% in each tail. Empty and NT superscript represent all sectors and non-tradable sectors,respectively. log(New Lending$^{rev1m}_{c,t}$) is new loans to small businesses with gross annual revenues under \$1 million in log scales, and log $(\widehat{\text{New Lending}})^{rev1m}_{c,t}$ is instrumented new lending using CountyExposure$_{c,t}$ (the predicted values in regression (1) of Table OA.9). Control variables include log changes in GDP and population, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the county level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	$\text{Assets}_{s,t}^{GDP}$	$\text{Liabilities}_{s,t}^{GDP}$	$\text{Pension Deficits}_{s,t}^{GDP}$
	(1)	(2)	(3)
$\text{Treatment}_{s,t-2}$	−0.133	−0.117	0.016
	(−0.663)	(−0.324)	(0.054)
$\text{Treatment}_{s,t-1}$	−0.089	−0.150	−0.061
	(−0.554)	(−0.523)	(−0.261)
$\text{Treatment}_{s,t}$	−0.040	−0.413*	−0.373*
	(−0.288)	(−1.659)	(−1.825)
$\text{Treatment}_{s,t+1}$	−0.120	−0.414	−0.293
	(−0.832)	(−1.592)	(−1.374)
$\text{Treatment}_{s,t+2}$	−0.117	−0.330	−0.213
	(−0.787)	(−1.235)	(−0.970)
State	Y	Y	Y
Year	Y	Y	Y
Observations	490	490	490
Adjusted R^2	0.969	0.974	0.968

Table OA.11: The effects of pension reforms on state pension status. This table estimates the impacts of pension reforms on state pension (94 of all kinds) status over 2009-2018. $\text{Assets}_{s,t}^{GDP}$, $\text{Liabilities}_{s,t}^{GDP}$ and $\text{Deficits}_{s,t}^{GDP}$ are state s' public pension asset, liabilities and deficits, all scaled by state GDP at year t. The unit of analysis is state-year. The indicator variable $\text{Treatment}_{s,t}$ is set to 1 if state s enacts a reform in year t (and is otherwise set to 0); $\text{Treatment}_{s,t+\tau}$ is the same as $\text{Treatment}_{s,t}$ but τ years *after* the reform in year t. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	Δ log(Deposits$_{i,b,c,t}$) $\geq$ 2 States			Δ log(Dividends$_{c,t}$)	
	(1)	(2)		(3)	(4)
Treatment$_{s,t-2}$	−0.276	−0.031		−0.007	0.256
	(−0.605)	(−0.073)		(−0.005)	(0.168)
Treatment$_{s,t-1}$	−0.159	−0.199		0.474	0.437
	(−0.377)	(−0.572)		(0.365)	(0.438)
Treatment$_{s,t}$	−0.634*	−0.798*		2.255*	1.996*
	(−1.684)	(−1.953)		(1.878)	(1.673)
Treatment$_{s,t+1}$	−0.236	−0.187		−0.802	1.343
	(−0.770)	(−0.688)		(−0.725)	(1.024)
Treatment$_{s,t+2}$	0.645**	0.168		0.519	3.259**
	(2.540)	(0.628)		(0.448)	(2.367)
ΔControls$_{c,t}$	Y	Y	ΔControls$_{c,t}$	Y	Y
Bank × Year × CSA	Y	N	Year × CSA	Y	N
Bank × Year × County-pair	N	Y	Year × County-pair	N	Y
County	Y	Y	County	Y	Y
Observations	162,887	301,205	Observations	8,993	20,535
Adjusted R^2	0.107	0.112	Adjusted R^2	0.393	0.215

Table OA.12: The effects of pension reforms on bank deposits and stock investments. This table estimates the effects of state public pension reforms (94 of all kinds) on bank deposits and stock investments over 2009-2018. Δ log(Deposits)$_{i,b,c,t}$ is log change in percentage of deposits of branch i of bank b in county c at the end of June in year t, and Δ log(Dividends)$_{c,t}$ is log change in percentage of dividends in county c at year t. Both are winsorized at 0.5% in each tail. The indicator variable Treatment$_{s,t}$ is set to 1 if state s enacts a reform in year t (and is otherwise set to 0); Treatment$_{s,t+\tau}$ is the same as Treatment$_{s,t}$ but τ years *after* the reform in year t. Control variables include log changes in GDP and per capita income, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the state level for regressions (1) and (3), and at both state and border segment levels for regressions (2) and (4). *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

Panel A: Liabilities + Equity

(%)	Δ log(Liabilities$_{b,t}$)	Δ log(Deposits$_{b,t}$)	Δ log(RetailDeposits$_{b,t}$)	Δ log(Wholesale$_{b,t}$)	Δ log(Equity$_{b,t}$)
	(1)	(2)	(3)	(4)	(5)
Exposure$_{b,t}$	-0.444^{***}	-0.554^{***}	-0.560^{***}	2.696	0.446^{**}
	(-2.918)	(-3.495)	(-3.036)	(0.990)	(2.353)
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	56,510	56,505	56,419	42,351	56,484
Adjusted R^2	0.213	0.204	0.163	0.234	0.261

Panel B: Assets

(%)	Δ log(Assets)$_{b,t}$	Δ log(CiLoan)$_{b,t}$	Δ log(PersLoan)$_{b,t}$	Δ log(ReLoan)$_{b,t}$	Δ log(Securities)$_{b,t}$
	(1)	(2)	(3)	(4)	(5)
Exposure$_{b,t}$	-0.357^{**}	-0.878^{**}	0.306	0.231	-1.823^{*}
	(-2.565)	(-2.119)	(0.637)	(1.066)	(-1.833)
Bank FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	56,510	55,469	55,750	56,104	30,466
Adjusted R^2	0.233	0.063	0.053	0.204	0.092

Table OA.13: The effects of pension reforms on banks' balance sheets. This tables estimates the effects of pension reforms (94 of all kinds) on banks' liability and asset items over 2009-2018. $\Delta \log(Y_{b,t})$ is the log changes in percentage of bank b's item Y at year t, winsorized at 0.5% in each tail. Exposure$_{b,t}$ is the weighted sum of Treatment$_{s,t}$ by the proportion of bank b' branches located in state s, see Equation (8). Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the bank level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

	Non-pension-reforming states		All states	
	log(New Lending$^{\$}_{b,c,t}$)	log(New Lending$^{\#}_{b,c,t}$)	log(New Lending$^{\$}_{b,c,t}$)	log(New Lending$^{\#}_{b,c,t}$)
	(1)	(2)	(3)	(4)
Exposure$_{b,t}$	−0.315***	−0.192**	−0.031	0.025
	(−2.684)	(−2.211)	(−0.634)	(1.053)
log(Age$_{b,t}$)	−0.055	−0.428	−0.221	−0.486
	(−0.121)	(−0.932)	(−0.581)	(−1.317)
log(Assets$_{b,t}$)	0.354	0.086	0.164	−0.036
	(1.425)	(0.457)	(0.985)	(−0.275)
County × Year	Y	Y	Y	Y
Bank	Y	Y	Y	Y
Observations	273,292	273,292	667,233	667,233
Adjusted R^2	0.431	0.468	0.400	0.389

Table OA.14: The effects of pension reforms on banks' lending to small businesses. This table estimates the effects of pension reforms (94 of all kinds) on banks' lending under \$1 million to small businesses over 2009-2018. The sample covers non–pension-reforming states only in the firs two columns, and all states in the last two columns. New Lending$^{\$}_{b,c,t}$ (New Lending$^{\#}_{b,c,t}$) is newly originated loans under \$1 million, in dollars (in quantities), to small businesses of bank b in county c of year t, and is winsorized at 0.5% in each tail. Exposure$_{b,t}$ is the weighted sum of Treatment$_{s,t}$ by the proportion of bank b' branches located in state s, see Equation (8). Bank controls include bank's age and assets, both in log scales. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the bank level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	log(New Lending$_{c,t}$)	Δ log(Est$_{c,t}$)	Δ log(Emp$_{c,t}$)	Δ log(Emp$^{BD}_{c,t}$)	Δ log(Emp$^{NT}_{c,t}$)	Δ log(Wage$^{NT}_{c,t}$)
	(1)	(2)	(3)	(4)	(5)	(6)
CountyExposure$_{c,t}$	−0.843*	−0.163***	0.012	−0.196	−0.783	−1.579
	(−1.776)	(−4.416)	(0.275)	(−0.992)	(−1.211)	(−0.628)
LendingPropensity$_{c,t}$	3.054***	0.025	0.031	0.014	0.365	1.170
	(8.321)	(1.110)	(1.223)	(0.101)	(1.283)	(1.228)
Δ log(Pop$_{c,t}$)	28.697	18.280***	29.150***	38.875	4.030	−122.923
	(0.372)	(2.908)	(3.191)	(0.917)	(0.047)	(−0.360)
Δ log(GDP$_{c,t}$)	9.551	1.679***	8.462***	2.875	28.510***	101.187**
	(1.215)	(2.991)	(9.659)	(0.717)	(2.681)	(2.397)
Year	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Observations	11,065	10,831	10,831	10,831	10,826	10,826
Adjusted R^2	0.974	0.267	0.255	−0.040	0.015	−0.070

Table OA.15: Economy consequences of pension reforms in non–pension-reforming states. This table estimates the effects of pension reforms (94 of all kinds) on counties' economic activity over 2009-2018. log(New Lending$_{c,t}$), Δ log(Est$_{c,t}$), Δ log(Emp$_{c,t}$) and Δ log(Wage) are the total new loans under \$1 million to small business in log scales, log changes in establishments or employment or wages in county c of year t, and all are winsorized at 0.5% in each tail. Empty BD and NT superscript represent all sectors, bank-dependent sectors and non-tradable sectors,respectively. Control variables include log changes in GDP and population, both at the county-year level. CountyExposure$_{c,t}$ is the weighted sum of all banks' exposure to pension reforms in county c, where the weights are based on the lending shares of banks in county c prior to the reform, see Equation (9). LendingPropensity$_{c,t}$ is similar to CountyExposure$_{c,t}$ without considering banks' exposure to pension reforms, see Equation (10). Pop and GPD are county-level population and GDP, respectively. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the county level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

(%)	$\Delta \text{Est}_{c,t}$		$\Delta \text{Emp}^{NT}_{c,t}$		$\Delta \text{Wage}^{NT}_{c,t}$	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
$\log(\text{New Lending})_{c,t}$	0.001		0.005		0.006	
	(1.252)		(0.321)		(0.097)	
$\log(\widehat{\text{New Lending}})_{c,t}$		0.170^{**}		0.824		1.661
		(2.103)		(1.238)		(0.719)
$\text{LendingPropensity}_{c,t}$	0.003	-0.501^{**}	0.261	−2.175	0.973	−3.952
	(0.155)	(−2.004)	(1.044)	(−1.054)	(1.138)	(−0.553)
$\Delta \log(\text{Pop}_{c,t})$	18.511^{***}	14.008	5.181	−14.652	−120.597	−160.587
	(3.433)	(1.251)	(0.072)	(−0.178)	(−0.414)	(−0.551)
$\Delta \log(\text{GDP}_{c,t})$	1.609^{***}	0.020	28.194^{***}	20.321	100.592^{***}	84.677^{*}
	(3.345)	(0.014)	(3.103)	(1.570)	(2.794)	(1.904)
Year	Y	Y	Y	Y	Y	Y
County	Y	Y	Y	Y	Y	Y
Observations	10,831	10,831	10,826	10,826	10,826	10,826
Adjusted R^2	0.265		0.0191		−0.0730	

Table OA.16: Real consequences—reduced-form and instrument variable results. This table estimates the relationship between lending and local economic consequences under both reduced-form and instrument variable form. $\Delta \log(\text{Est}_{c,t})$, $\Delta \log(\text{Emp}_{c,t})$ and $\Delta \log(\text{Wage})$ are log changes in establishments or employment or wages in county c of year t, and all are winsorized at 0.5% in each tail. Empty and NT superscript represent all sectors and non-tradable sectors,respectively. $\log(\text{New Lending}_{c,t})$ is the total new loans under \$1 million to small business in log scales, and $\log(\widehat{\text{New Lending}})_{c,t}$ is instrumented new lending using $\text{CountyExposure}_{c,t}$ (the predicted values in regression (1) of Table OA.15). Control variables include log changes in GDP and population, both at the county-year level. Regression coefficients are reported in the shaded rows, and t-statistics are reported in the unshaded rows. Standard errors are clustered at the county level. *, **, *** represent statistical significance at 10%, 5% and 1% level, respectively.

State	Plan Name	Year	Affected group	Main modifications	Treatment
AL	Retirement Systems of Alabama	2012	New hires	Reduced Pension	1
AZ	Arizona State Retirement System	2010	New hires	Reduced Pension	1
AZ	Arizona State Retirement System	2011	New hires	Reduced Pension	1
AZ	Arizona State Retirement System	2013	New hires	Reduced Pension	1
AR	Arkansas Public Employees Retirement System	2009	Current employees	Increased Employee Contributions	1
AR	Arkansas Public Employees Retirement System	2011	Current employees	Increased Employee Contributions	1
CA	California Public Employees Retirement Association	2010	New hires	Increased Employee Contributions & Reduced Pension	1
CA	California Public Employees Retirement Association	2012	New hires	Increased Employee Contributions & Reduced Pension	1
CO	Colorado Public Employees' Retirement Association	2010	All employees	Increased Employee Contributions & Reduced Pension	0
CO	Colorado Public Employees' Retirement Association	2011	Current employees	Increased Employee Contributions	1
CO	Colorado Public Employees' Retirement Association	2018	All employees	Increased Employee Contributions & Reduced Pension	0
CT	Connecticut State Employees Retirement System	2011	Current employees & New hires	Reduced Pension	0
CT	Connecticut State Employees Retirement System	2017	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
DE	Delaware Public Employees Retirement System	2011	New hires	Increased Employee Contributions	1
FL	Florida Retirement System	2011	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
FL	Florida Retirement System	2017	New hires	Changed Plan Design	1

GA	Employees’ Retirement System of Georgia	2014	New hires	Increased Employee Contributions	1
IL	State Retirement Systems of Illinois	2010	New hires	Reduced Pension	1
IL	State Retirement Systems of Illinois	2018	Retired & Current employees	Reduced Pension	0
IN	Indiana Public Retirement System	2011	New hires	Changed Plan Design	1
IN	Indiana Public Retirement System	2015	New hires	Changed Plan Design	1
IA	Iowa Public Employees Retirement System	2010	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
KS	Kansas Public Employees Retirement System	2011	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
KS	Kansas Public Employees Retirement System	2014	New hires	Reduced Pension	1
KY	Kentucky Retirement Systems	2011	All employees	Reduced Pension	0
KY	Kentucky Retirement Systems	2013	All employees	Reduced Pension	0
LA	Louisiana State Employees Retirement	2009	All employees	Reduced Pension	0
LA	Louisiana State Employees Retirement	2010	New hires	Reduced Pension	1
LA	Louisiana State Employees Retirement	2014	All employees	Reduced Pension	0
ME	Maine Public Employees Retirement System	2011	Retired & Current employees	Reduced Pension	0
ME	Maine Public Employees Retirement System	2018	Retired & Current employees	Reduced Pension	0
MD	Maryland State Retirement & Pension System	2011	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
MA	Massachusetts State Employees Retirement System	2011	All employees	Reduced Pension	0
MI	Michigan State Employees’ Retirement System	2011	Current employees	Increased Employee Contributions	1

MN	Minnesota State Retirement System	2010	All employees	Reduced Pension	0
MN	Minnesota State Retirement System	2014	Current employees	Increased Employee Contributions	1
MN	Minnesota State Retirement System	2018	Retired & Current employees	Increased Employee Contributions & Reduced Pension	0
MS	Mississippi Public Employees Retirement System	2010	Current employees	Increased Employee Contributions	1
MS	Mississippi Public Employees Retirement System	2011	New hires	Reduced Pension	1
MO	Missouri State Employees' Retirement System	2010	New hires	Increased Employee Contributions	1
MO	Missouri State Employees' Retirement System	2017	Current employees	Increased Employee Contributions	1
MT	Montana Public Employee Retirement Administration	2011	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
MT	Montana Public Employee Retirement Administration	2013	All employees	Reduced Pension	0
NE	Nebraska Public Employees' Retirement System	2011	Current employees	Increased Employee Contributions	1
NE	Nebraska Public Employees' Retirement System	2013	New hires	Reduced Pension	1
NE	Nebraska Public Employees' Retirement System	2017	New hires	Reduced Pension	1
NV	Nevada Public Employees Retirement System	2009	New hires	Reduced Pension	1
NV	Nevada Public Employees Retirement System	2015	New hires	Reduced Pension	1
NH	New Hampshire Retirement System	2011	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
NJ	New Jersey Division of Pensions & Benefits	2010	New hires	Reduced Pension	1
NJ	New Jersey Division of Pensions & Benefits	2011	All employees	Increased Employee Contributions & Reduced Pension	0

NM	New Mexico Public Employees Retirement Association	2009	New hires	Increased Employee Contributions	1
NM	New Mexico Public Employees Retirement Association	2013	All employees	Reduced Pension	0
NY	New York State and Local Retirement System	2009	New hires	Reduced Pension	1
NY	New York State and Local Retirement System	2012	New hires	Increased Employee Contributions & Reduced Pension	1
NC	North Carolina Retirement System	2011	New hires	Reduced Pension	1
NC	North Carolina Retirement System	2014	Current employees & New hires	Reduced Pension	0
ND	North Dakota Public Employees Retirement System	2011	Current employees & New hires	Increased Employee Contributions	1
ND	North Dakota Public Employees Retirement System	2013	Current employees & New hires	Increased Employee Contributions	1
ND	North Dakota Public Employees Retirement System	2015	New hires	Increased Employee Contributions	1
OH	Ohio Public Employees Retirement System	2012	Current employees & New hires	Reduced Pension	0
OK	Oklahoma Public Employees Retirement System	2011	Current employees & New hires	Reduced Pension	0
OK	Oklahoma Public Employees Retirement System	2013	New hires	Reduced Pension	1
OK	Oklahoma Public Employees Retirement System	2014	New hires	Changed Plan Design	1
OK	Oklahoma Public Employees Retirement System	2015	New hires	Increased Employee Contributions	1
OR	Oregon Public Employees' Retirement System	2013	Current employees & New hires	Reduced Pension	0

PA	Pennsylvania State Employees Retirement System	2010	New hires	Increased Employee Contributions & Reduced Pension	1
PA	Pennsylvania State Employees Retirement System	2017	Current employees & New hires	Increased Employee Contributions & Increased Employee Contributions & Changed Plan Design	1
RI	Rhode Island Employees' Retirement System	2009	Current employees	Reduced Pension	0
RI	Rhode Island Employees' Retirement System	2010	Current employees	Reduced Pension	0
RI	Rhode Island Employees' Retirement System	2011	Current employees & New hires	Reduced Pension & Changed Plan Design	0
SC	South Carolina Retirement System	2012	All employees	Reduced Pension	0
SC	South Carolina Retirement System	2016	Current employees	Increased Employee Contributions	1
SD	South Dakota Retirement System	2010	Retired & Current employees	Reduced Pension	0
SD	South Dakota Retirement System	2016	New hires	Reduced Pension	1
SD	South Dakota Retirement System	2017	Retired & Current employees	Reduced Pension	0
TN	Tennessee Consolidated Retirement System	2013	New hires	Reduced Pension & Changed Plan Design	1
TX	Employees Retirement System of Texas	2009	New hires & New hires	Reduced Pension	1
TX	Employees Retirement System of Texas	2013	Current employees & New hires	Increased Employee Contributions & Reduced Pension	1
TX	Employees Retirement System of Texas	2015	Current employees	Increased Employee Contributions	1
UT	Utah Retirement System	2010	New hires	Reduced Pension & Changed Plan Design	1
VT	Vermont State Employees' Retirement System	2011	Current employees	Increased Employee Contributions	1
VA	Virginia Retirement System	2010	New hires	Reduced Pension	1
VA	Virginia Retirement System	2011	Current employees	Increased Employee Contributions	1

VA	Virginia Retirement System	2012	Current employees & New hires	Increased Employee Contributions & Reduced Pension	0
VA	Virginia Retirement System	2014	New hires	Changed Plan Design	1
WA	Washington Department of Retirement Systems	2011	Retired & Current employees	Reduced Pension	0
WA	Washington Department of Retirement Systems	2012	New hires	Reduced Pension	1
WV	West Virginia Consolidated Public Retirement Board	2015	New hires	Increased Employee Contributions & Reduced Pension	1
WI	Wisconsin Retirement System	2011	Current employees & New hires	Increased Employee Contributions	1
WY	Wyoming Retirement System	2010	Current employees & New hires	Increased Employee Contributions	1
WY	Wyoming Retirement System	2012	Current employees & New hires	Reduced Pension	0
WY	Wyoming Retirement System	2013	Current employees	Increased Employee Contributions	1
WY	Wyoming Retirement System	2018	Current employees	Increased Employee Contributions	1

Table OA.17: State Public Pension Reforms 2009-2018. This table reports the summary of state public pension reforms over 2009 and 2018: the state, the plan name, the year of reform, the groups of workers affected, the main modifications, and whether the reform is included in the treatment group.