Paid Sick Leave Mandates and Household Portfolio Choice

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Abstract

Using the staggered adoption of paid sick leave (PSL) mandates across US states, we document a 20% increase in the average household stock market participation following the enactment of a PSL policy. The effects are more pronounced among households facing greater health concerns, higher employment risks, and financially vulnerable households. Several mechanisms can explain our findings. PSL mandates offer households insurance-like protection, increase their income and wealth, and improve households' future outlook. Our findings demonstrate that PSL laws create positive economic externalities by motivating households to invest in risky assets, a key factor toward building wealth.

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1. Introduction

One in four workers in the United States (US) report losing their job or were threatened with job termination for taking time off due to illness (Smith and Kim, 2010). Moreover, workers without access to paid sick leave (PSL) face a 25% higher probability of job separation compared to those with PSL benefits (Hill, 2013). Despite this, the US remains one of a few wealthy nations that does not provide a federal PSL mandate (World Policy Analysis Center, 2023). In response, several US states, cities, and counties have enacted legislations requiring firms to provide minimum levels of paid sick leave to their employees. While PSL laws have been shown to provide positive public health benefits (Pichler and Ziebarth, 2017), relatively less is known about the economic externalities of these laws. The objective of our paper is to shed light on one channel through which PSL laws potentially provide broad-based economic benefits: stock market participation.

Few households invest in the stock market. This is puzzling, because standard models predict that all individuals, no matter how risk averse, should invest a fraction of their wealth in equities (e.g., Merton, 1969). Understanding the causes of equity market nonparticipation is important, particularly because stock investment is a key factor toward building household wealth. We hypothesize and show that PSL laws promote stock market participation by reducing household background risks related to health, income, and job security concerns.

Our conceptual framework builds on the theoretical models of risk aversion in the presence of multiple sources of risk (e.g., Gollier and Pratt, 1996; Kimball, 1993). This framework suggests that any form of background risk such as health or income risk can reduce the optimal level of financial risk that a household is willing to bear. PSL laws could promote stock market participation by mitigating various sources of household background risk. Specifically, in the absence of paid sick leave, workers face a trade-off: either go to work while sick or take unpaid

leave. Choosing unpaid leave results in lost wages and potential job termination (Miller, 2022; Susser and Ziebarth, 2016), while working through illness slows recovery, reduces productivity, and increases the likelihood of further health deterioration and more costly medical expenses (Chunyu et al., 2024; Goetzel et al., 2004; Grinyer and Singleton, 2000). Conversely, access to paid sick leave allows employees to take paid time off for necessary medical treatment while maintaining income stability and job security. Thus, PSL laws could mitigate background risk by reducing uncertainties related to health conditions, health-related expenses, individual productivity, job security, and other contingencies that could impact future earnings. As a result, these laws could increase the optimal level of household financial risk-taking and encourage them to participate in the risky stock markets.

To examine the effect of paid sick leave laws on household stock market participation, we utilize micro-level household data from the Panel Study of Income Dynamics (PSID) in a difference-in-differences (DiD) design. Specifically, we exploit the staggered adoption of PSL mandates across US states, cities, and counties from 2009 to 2019. During this sample period, 11 states and 30 localities enacted a PSL mandate, covering the majority of PSL enactments to date.[1] The law requires firms to provide minimum levels of paid sick leave and allow workers to accrue paid sick time to recover from their own short-term illnesses, take preventive care, or take care of sick family members.

We start by evaluating the relevance and potential impact of PSL laws on households. First, using Google Trends data, we show that internet search interest for the term "paid sick leave" peaks in each state around the state's PSL enactment. Second, using PSID data, we document a

[1] Although our sample ends in 2019 to avoid capturing confounding effects from the Covid-19 pandemic, there are several states that are currently considering enacting PSL mandates (e.g., Alaska and Missouri). Thus, our results could contribute to ongoing policy discussions on the economic impact of PSL provision.

sharp 54% increase in the average number of sick days that workers take to recover from their illness following the adoption of a PSL policy. The results suggest that households do recognize and utilize the benefits accrued from PSL mandates. Finally, we also evaluate the political economy of PSL adoption, finding that prior economic and political conditions in the state do not drive its adoption.

Having verified that the law matters to households, we analyze the effects of PSL adoption on household stock market participation. Consistent with our hypothesis, we document an increase in household stock market participation following the enactment of a PSL policy. Specifically, following PSL implementation, households are on average 3.83% more likely to participate in the stock market. This corresponds to a substantial marginal effect of 20% relative to the mean stockholding rate of 18.7%. Moreover, the treatment effect is larger when the expected benefits of the law are higher —such as when the law mandates a more generous paid sick leave policy, and for households working in industries where pre-mandate coverage rates of voluntary paid sick leave benefits are limited.

The results are robust to controlling for a large set of household demographic and wealth characteristics and other public safety nets, including Paid Family Leave, the Affordable Care Act, and Unemployment Insurance.[2] All specifications also include household, county, survey year, and survey month fixed effects. This allows us to compare the stock market participation behavior of the *same* household before and after the passage of a PSL mandate, while controlling for time-invariant county characteristics and macroeconomic fluctuations that could affect stockholdings. Overall, the results are most consistent with a causal relationship between PSL mandates and household stockholdings.

[2] We argue in Section 2.1 that PSL mandates differ from other public programs in its coverage and in terms of benefits provided.

We perform various tests to further support the validity of our results. First, we show that the significant treatment effects only materialize after, and not before, the effective date of PSL mandates, indicating that there is no pre-trends in stock market participation prior to the implementation of PSL mandates. Second, we conduct several placebo tests by altering the (1) treatment timing, (2) treatment location, and (3) exploiting the unsuccessful attempt to pass a PSL mandate in Orange County, Florida. We do not observe any change in household stockholdings around these placebo events, further mitigating the concern that our results are driven by omitted local characteristics. Finally, we allow for treatment effect heterogeneity using the estimates proposed by Callaway and Sant'Anna (2021) and Cengiz et al. (2019) to address the concern that a staggered treatment design could lead to a biased estimation of causal effects when the treatment effects evolve over time.

We next evaluate three economic mechanisms through which PSL mandates affect stock market participation. The first mechanism—*insurance-like protection*—posits that PSL laws promote stock market participation by offering households insurance-like protection. Specifically, by enabling sick workers to take paid time off for recovery without the risk of losing their income or job (Miller, 2022), PSL laws insure households against various sources of background risk related to health conditions, job security, and other contingencies affecting future earnings.

Our first test of this channel focuses on vulnerable households. If PSL laws offer household insurance-like protection, this protection should be especially valuable to vulnerable households facing significant health and employment concerns. In line with this channel, we find that the effect of PSL mandates on stock holdings is more pronounced for: (1) households with significant health concerns (defined as those where the household head experiences poor or declining health), (2) households facing job security concerns (defined as households located in a county with a high

layoff rate or those where the household head recently re-enters the workforce following periods of unemployment), and (3) households vulnerable to wealth shocks (defined as those with a high subsistence-level consumption-to-wealth ratio, where a small wealth decline could jeopardize the household's ability to maintain its minimum consumption levels).

As a second test of the insurance-like protection channel, we examine the impact of PSL laws on households' precautionary saving motives. Access to PSL benefits can reduce uncertainty about the future, which may lower households' need for precautionary savings and enable them to take greater financial risk (e.g., Chou et al., 2003; Engen and Gruber, 2001; Hubbard et al., 1995). Consistent with this hypothesis, we find a 9% increase in the ratio of risky over safe assets following the enactment of a PSL mandate, indicating that households adjust their portfolios by shifting from safer to riskier financial assets.

The second channel—*subjective expectations*— states that PSL policies could improve households' subjective expectations, leading to increased stock market participation. By reducing disease transmission in the workplace and strengthening job protection, PSL laws could improve employees' future expectations and incentivize them to stay longer in the workforce. All else equal, a longer expected retirement horizon and improved optimism could encourage greater financial risk-taking (Choi and Robertson, 2020; Puri and Robinson, 2007). Consistent with this channel, we find that after the enactment of PSL mandates, household heads with health concerns are more likely to extend their planned retirement age and are less likely to experience a decline in life satisfaction.

The third channel—*household wealth*— hypothesizes that PSL laws could promote stock market participation by enhancing household income and wealth. Although the laws do not provide immediate cash gains, they can increase household income and wealth by ensuring that sick

workers continue to receive pay while taking time off. PSL laws could also affect wealth by improving worker productivity while minimizing costly medical expenditures. Consistent with this channel, we find that following the enactment of a PSL mandate, households experience an increase in income and non-housing wealth, and use some of the gains in non-housing wealth to invest in the stock market.

Our paper contributes to several active strands of literature. First, it advances the literature on the economic effects of PSL laws. Since several localities are debating the adoption of local PSL mandates and the pressure to implement a federal PSL law in the US continues to grow, it becomes increasingly important to understand the economic impact of such policies. Most of the existing literature focuses on firms, showing that the enactment of PSL mandates improves labor productivity and firm profitability (Al-Sabah and Ouimet, 2023; Chunyu et al., 2024), while inhibiting firm innovation (Huang and Shu, 2024). Other recent studies examine the aggregate impact of PSL laws on local employment and wages (e.g., Al-Sabah and Ouimet, 2023; Pichler and Ziebarth, 2020).

To the best of our knowledge, we are the first to investigate whether and how households adjust their portfolio allocations in response to increased access to PSL benefits. Using variations in the adoption of PSL mandates across US localities and over time, we provide new evidence that the adoption of PSL mandates causally leads to an increase in household stock market participation by reducing various sources of households' background risk. Additionally, a novel aspect of our work is that we are able to directly link a household's health, employment, and financial vulnerability to the extent they would benefit from having access to PSL. In doing so, our findings complement studies showing that providing social security or health insurance programs can help promote stock market participation (e.g., Ayyagari and He, 2016; Gormley et al., 2010).

Second, our paper broadly contributes to the literature on the impact of various public safety net programs on households. For example, unemployment insurance benefits have been shown to help smooth household consumption (Gruber, 1994) and reduce precautionary saving motives (Arslan et al., 2024; Engen and Gruber, 2001). Medicaid eligibility reduces personal bankruptcies (Gross and Notowidigdo, 2011) and increases credit card borrowing (Bornstein and Indarte, 2023), whereas Medicare eligibility at age 65 improves risky investment among elderly households (Angrisani et al., 2018; Ayyagari and He, 2016; Christelis et al., 2020). Compared to other programs, paid sick leave laws provide full wage replacement, have a broad coverage of any medical reason requiring short-term leave, and are accessible to millions of workers in the US private sector.[3] We demonstrate that the provision of such short-term paid sick leave generates a positive economic externality on households by lowering their precautionary saving motives and promoting stock market participation—an important factor toward building wealth.

Finally, we add to the literature on stock market participation. Our work is related to the studies that examine how unexpected cash windfalls, such as inheritance or lottery winnings, affect household stock market participation (Andersen and Nielsen, 2011; Briggs et al., 2021). While these cash windfalls are typically one-off gains, the provision of sick leave provides regular, long-term fringe benefits and safety nets against potential negative wealth shocks, thereby encouraging households to invest in risky assets. More broadly, our paper also contributes to the literature on how exogenous changes in socioeconomic environments, such as corporate scandals (Giannetti and Wang, 2016), the launch of local IPOs (Jiang et al., 2024), or the introduction of employment protection laws (Jo, 2023) affect household stock market participation. Our paper extends this

[3] In contrast, unemployment insurance only offers partial wage replacement for the unemployed, and Medicaid and Medicare target low-income individuals and the elderly, respectively. Similarly, paid family laws only target serious illnesses that require long-term leave.

literature by showing that exogenous variation in the provision of public safety net programs, in particular, paid sick leave laws, plays a significant role in promoting household stockholdings. Our findings have important implications for policy discussions on the economic benefits of PSL laws.

2. Institutional setting and conceptual framework

2.1 Institutional setting

The introduction of paid sick leave dates back to the Sickness Insurance Law of 1883 in Germany, which is an important element of the world's first social insurance system. Other countries adopts similar sick leave mandates over the subsequent decades. Currently, all European countries and most OECD countries guarantee universal access to paid sick leave for employees (Maclean et al., 2020). In contrast, the US is one of two OECD countries without a federal paid sick leave law (World Policy Analysis Center, 2023). As of 2009, the BLS estimates that around 39% of the US private sector workers (45 million) do not have access to paid sick leave provisions.

In response, several US states, cities, and counties have enacted legislations requiring firms to provide minimum levels of paid sick leave. These laws enable workers to accrue paid sick time to recover from their own short-term illnesses or take care of sick family members. Importantly, PSL mandates also protect workers from being fired for taking this paid time off due to illness (Miller, 2022).

As of 2019, 11 states and 32 localities have enacted paid sick leave mandates, which are detailed in Appendix 1. As shown in Appendix 1, the generosity of PSL mandates varies across states and localities. In our sample, the accrual rates, which indicate how quickly paid sick leave is earned per hour worked, range from one hour for every 30 hours worked (e.g., Arizona) to one hour for every 87 hours worked (e.g., Washington, DC). The annual cap, which is the maximum paid sick leave hours that can be accrued each year, varies from 24 hours to 80 hours per year.

With a few exceptions, PSL policies cover both full-time and part-time and temporary workers (Al-Sabah and Ouimet, 2021). Most PSL laws apply to all firms within the mandate's jurisdiction, although in some states such as Michigan, the smallest firms are exempted from providing paid sick leave.

We conduct various tests to evaluate the relevance and potential impact of PSL laws on households. First, we follow Al-Sabah and Ouimet (2021) and use Google search volume for "sick leave" to gauge the public interest in PSL mandates. As shown in Panel A of Figure 1, we observe a spike in the internet search interest for "paid sick leave" around the time when the state passes PSL law. For example, the search interest for "paid sick leave" peaks around July 2015 when the PSL mandate becomes effective in California. Conversely, as shown in Panel B, states without PSL mandates, such as Virginia and Alabama, do not exhibit specific trends in search interest. The patterns in Figure 1 indicate that households are aware of and actively seek information about the adoption of the PSL laws in locations where mandates are in effect.

[Figure 1 around here]

Second, we validate in Appendix 3 that the enactment of PSL mandates indeed incentivizes workers to take time off from work to recover from short-term illnesses. In particular, after the adoption of PSL mandates, employees miss, on average, 54% (= 2.29/4.25) more days of work a year due to self-illness. In contrast, the adoption of PSL laws does not affect the number of days that employees miss work to take care of other family members or to take a vacation.[4] Finally, we later show in Table 3 that the adoption of PSL mandates is not driven by local socioeconomic or

[4] It is highly unlikely that survey participants provide inaccurate information about the reasons and number of days they miss work. This is because survey responses are anonymous and participants do not receive any benefits based on the answers they provide.

political factors. Instead, grassroots ballot initiatives have been instrumental to the mandates' adoption in many states in our sample (Maclean et al. 2020).

In addition to paid sick leave mandates, there are several other public safety net programs in the US designed to provide financial and social support to individuals and families in need, particularly during times of hardship. For example, the Family and Medical Leave Act of 1993 (FMLA) is a federal law requiring companies with 50 or more employees to provide up to 12 weeks of *unpaid* leave to employees who have been with the company for at least one year. As a result, the FMLA excludes many part-time employees and employees in small companies, and only covers approximately 44% of the private sector workforce (Jorgensen and Appelbaum, 2014). In response, numerous states enacts the Paid Family Leave (PFL) programs. The PFL allows employees to take time off work with partial wage replacement to provide longer-term care for seriously ill family members, undergo medical treatment, recover from a serious illness, or bond with a new child through birth, adoption, or foster care placement.

Another public safety net program is the Affordable Care Act (ACA). The ACA expands medical coverage to include individuals under the age of 65 with income at or below 138% of the federal poverty level in eligible states. Finally, Unemployment Insurance (UI) is a social insurance program designed to provide temporary income to partially replace the earnings lost by eligible workers when they become involuntarily unemployed. While the UI provision is federally mandated, eligibility criteria and benefit amounts vary by state.

Paid sick leave mandates differ from other safety net programs in several ways. First, the PSL program stands out by offering full wage replacement for the period of absence, in contrast to the UI and PFL programs, which only provide partial wage replacement. Second, PSL benefits are far more accessible compared to other programs. For instance, the ACA primarily targets low-

income households, while UI benefits are provided solely to unemployed individuals. Third, PSL mandates provide broader coverage for any medical reason requiring short-term leave, whereas PFL programs only target serious illnesses that require long-term leave. Finally, the funding for ACA, UI, and PFL is sourced from tax revenue, whereas the full costs of providing PSL are covered by the employers (Al-Sabah and Ouimet, 2021; Miller, 2022).

2.2 Conceptual framework and hypothesis development

Having laid out the institutional setting, we proceed to develop our conceptual framework and testable hypotheses. Our framework is based on theoretical models of risk aversion in the presence of multiple sources of risk. Under fairly general conditions, any source of background risk[5] such as medical risk crowds out the demand for risky assets (e.g., Gollier and Pratt, 1996; Kimball, 1993). Specifically, the "standard risk aversion" concept proposed by Kimball (1993) suggests that the presence of one undesirable risk increases sensitivity to another, even when the two risks are independent. As a result, bearing one undesirable risk should make an agent reduce optimal levels of risky investment. Similarly, under the concept of "risk vulnerability" proposed by Gollier and Pratt (1996), adding an unfair background risk can cause a utility maximizing agent to reduce their demand for risky assets and increase the demand for insurance. An important implication of this framework is that increases in multiple sources of background risk reduce the optimal financial risk that a household is willing to bear.

We hypothesize that access to PSL benefits could reduce multiple sources of a household's background risk, including income risk, health risk, medical expenditure risk, and job security risk. Specifically, without PSL, sick workers face a trade-off: either going to work with illness or taking

[5] Background risk refers to any risk that an individual cannot avoid. See Elmendorf and Kimball (2000), Gollier and Pratt (1996), Gomes et al. (2021), Kimball (1993), and Pratt and Zeckhauser (1987), among others.

unpaid leave. If they choose to take unpaid leave and stay at home, they not only lose wages but may also face the risk of job termination (Miller, 2022; Susser and Ziebarth, 2016). This is because the majority of private sector workers in the US are "at-will employees" (meaning their employment can be terminated by their employer for any legal reason at any time), missing too much work due to a non-severe illness can be grounds for termination in the absence of PSL mandates (Al-Sabah and Ouimet 2023). Consistent with this, nearly one in four workers in the US report that they have lost a job or were threatened with job termination for taking time off due to illness (Smith and Kim, 2010), and workers without paid sick leave face a 25% higher probability of job separation (Hill, 2013).

Conversely, with access to PSL, sick employees can take paid time off at home to recover while ensuring both income stability and job security. Additionally, sick employees have the option to take paid leave for necessary medical treatment (Gilleskie, 1998), preventing potentially higher medical costs and more severe health issues due to untimely treatment (Miller, 2022). Thus, PSL laws could mitigate multiple sources of background risk, particularly those associated with human capital. As the primary driver of lifetime income, human capital carries substantial idiosyncratic and uninsurable risks (Guiso and Sodini, 2013). PSL laws may help reduce these risks by reducing uncertainties related to health conditions, health-related expenses, individual productivity, job security, future career prospects, and other contingencies that could impact future earnings. As a result, these laws could increase the optimal level of household financial risk-taking and encourage them to participate in the risky stock markets.

Hypothesis 1A. *Households are more likely to participate in the stock market following the adoption of a Paid Sick Leave mandate.*

While Hypothesis 1A focuses the average impact of PSL laws, it is natural to expect that the extent to which PSL laws mitigate household background risk and promote stock market participation varies depending on individual household circumstances. In line with the idea that PSL laws can help reduce background risk related to health conditions, job security, and other contingencies affecting future earnings, we further hypothesize that the impact of PSL laws would be particularly salient among vulnerable households facing poor health, employment risks, or exposure to wealth shocks. These households are likely to benefit more from the insurance-like protection provided by PSL laws.

Hypothesis 1B. *The impact of Paid Sick Leave laws on promoting stock market participation is more salient among vulnerable households facing greater health and employment risks.*

3. Data and methodology

3.1 Data and sample selection

Our analyses use household survey data from the Panel Study of Income Dynamics (PSID). The PSID started surveying a nationally representative sample of US households in 1968 and has continuously collected information on the same family and their descendants every year until 1997, and every two years since then (Hacamo, 2021). The PSID gathers information on all household members but focuses more extensively on the household head and their spouse or long-term cohabiting partner.[6] This includes detailed information on demographic characteristics such as age, education, and marital status, as well as household-level information such as household income, wealth, consumption, and borrowings.

[6] The PSID replaces the term "household head" with "reference person" starting from 2017.

The PSID offers three primary advantages for our research purposes. First, because the PSID tracks the same household over time, we are able to include household fixed effects and thus identify the effects of PSL *within* the same household. Second, the PSID provides a rich set of information on the health conditions, both current and anticipated future health conditions, of household members. This enables us to directly link individual health conditions to the extent they would benefit from having access to PSL. Finally, we can observe household locations at the county-level[7] and the exact date in which households complete the survey questionnaire. This allows us to accurately identify the dynamic timing effects of the passage of PSL mandates on stock market participation.

Our sample period starts in 2009 and ends in 2019. The sample starts in 2009 to avoid picking up the impact of the 2008 Global Financial Crisis on household financial decisions and ends in 2019 to avoid confounding effects related to the Covid-19 pandemic when, among others, the US enacted PSL mandates at the federal-level through the Families First Coronavirus Response Act (Al-Sabah and Ouimet, 2023; Maclean et al., 2020). Our 2009-2019 sample period covers the majority of PSL enactments to date, when 11 states and 30 localities enacted PSL mandates.[8] Because PSL mandates primarily affect employed individuals, we restrict our sample to households in which at least one of the household head or their spouse is employed.[9] This leaves us with 20,998 household-year observations covering six survey waves between 2009 and 2019.

3.2 Methodology

[7] Data on county-level household location come from the restricted-use PSID county-level identifiers.
[8] San Francisco and Washington, DC enacted PSL laws before 2009.
[9] We obtain robust results focusing on households in which only the household head is employed.

To examine the effect of the staggered adoption of PSL mandates on household stock market participation, we estimate the following household-level difference-in-differences (DiD) specifications:

$$Y_{ict} = \beta \times PSL_{ct} + X_{it} + \delta_i + \gamma_c + \eta_t + safety\ nets + \varepsilon_{ict} \quad (1)$$

The dependent variable Y_{ict} is a dummy variable that equals one if household i residing in county c holds any shares in public companies, mutual funds, or investment trusts on the survey date t, and zero otherwise (Giannetti and Wang, 2016). Following the literature, this measure focuses on stock investment in non-retirement accounts only because investment in retirement accounts is often influenced by employer-selected default options (Beshears et al., 2009; Giannetti and Wang, 2016). PSL_{ct} is a dummy variable that equals one if county c has effective local or state PSL mandates on survey date t, and zero otherwise. If PSL mandates are implemented at the state-level, we consider all counties in that state to be "treated" by the law. For a county that is subject to both local and state PSL mandates, we use the earlier effective date. The main coefficient of interest is the DiD coefficient β. The first difference is between households in counties that have effective PSL mandates and households in counties that do not yet have effective PSL mandates. The second difference is between the dates before and after the PSL mandates become effective.

We include a large set of fixed effects, including household fixed effects (δ_i), county fixed effects (γ_c), and time fixed effects (η_t). By adding household fixed effects, we compare stock market participation behavior of the *same* household before and after the passage of PSL. The inclusion of household fixed effects thus absorbs all time-invariant household characteristics that may influence stock market participation, such as household cultural background or slow-moving

household preferences. We further include county fixed effects to account for time-invariant local conditions, such as proximity to major cities or county-specific laws and regulations that predate the sample period. Finally, we account for time effects by including survey year fixed effects and survey month fixed effects to take out any seasonality and macroeconomic fluctuations that could affect household investment decisions.

X_{it} refers to our control variables. Following the household finance literature, we start with a set of household characteristics, including family size (*Family size*), dummy variables indicating whether the household owns a house (*House ownership*), has a mortgage debt (*Has a Mortgage*), the natural logarithms of total annual household income (*Log (Total household income))*, total household wealth (*Log (Total household wealth)),* and the outstanding balance of the mortgage (*Log (Mortgage balance))*. We further incorporate demographic characteristics of the household head, including *Age*, Age^2, and dummy variables indicating whether the household head is married (*Married*), has a college degree *(College),* is *Non-White*, is *Hispanic*, has poor physical health conditions (*Poor physical health*), and has poor mental conditions (*Poor mental health*).

Moreover, all specifications control for other public safety net programs. In particular, following Miller (2021), we include two dummies that equal one if the household resides in a state with an effective *ACA* and an effective *PFL* mandate. We further control for state-level unemployment insurance benefits, measured as the natural logarithm of the maximum amount of weekly unemployment benefits times the maximum benefit duration (*UI benefits*) (Agrawal and Matsa, 2013). Appendix 2 presents detailed definitions of the variables used in the empirical analysis.

[Table 1 around here]

Table 1 presents summary statistics. Similar to Giannetti and Wang (2016), all variables are weighted using the population weights provided by the PSID to ensure that they are representative of the underlying population. On average, 11.8% of the household-level observations in our sample are "treated," that is, residing in a location with an effective PSL policy. The average stock market participation rate is 18.7% and is in line with those reported in the prior literature (e.g., Jiang et al., 2024). The average household earns a total annual income of $96,693, has total wealth of $393,034, and 65.8% of households in our sample own a house.

4. Main results

4.1 Baseline results

Table 2 reports our baseline regression results on the impact of PSL mandates on household stock market participation. Model specifications in Table 2 vary across columns in terms of the set of control variables and fixed effects included. We start with a basic model where only the PSL dummy and survey year and survey month fixed effects are included (Column (1)). We then include several characteristics of the household in Column (2). In Column (3), we further add household head characteristics, household and county fixed effects, and control for other public safety net programs.

Across all specifications in Table 2, the coefficients on *PSL* are positive and statistically significant below the 1% level. Thus, consistent with Hypothesis 1A, our results indicate that households are more likely to participate in the stock market following the enactment of a PSL mandate. The effect is economically meaningful. For instance, in Column (3), which includes the full set of control variables and fixed effects, the coefficient on *PSL* indicates that households are approximately 3.83% more likely to participate in the stock market after the adoption of a PSL

law. The increase corresponds to a substantial marginal effect of 20% (= 0.0383/ 0.187) from the average stockholding rate of 18.7% in our sample.

[Table 2 around here]

Moreover, the magnitude of the coefficient estimates on *PSL* is stable as we progressively include more control variables and fixed effects in the model. For example, the coefficient on *PSL* remains at 0.0383 from Columns (2) to (3) even after further controlling for household head characteristics, household and county fixed effects, and measures of other safety net programs in the model. This suggests that the estimated effects are unlikely to be driven by unobserved heterogeneity across households and locations. We also note that the coefficients on other safety net programs are not statistically significant below the 5% level. It could be because, unlike PSL law, other programs have narrower applicability and are less accessible (e.g., unemployment insurance applies only to the unemployed, and PFL is limited to serious illnesses that require long-term leave).

4.2 Identification concerns

In this section, we show various tests to further support the validity of our results. Specifically, we show that both treated and control observations share a similar pre-event trend. Moreover, our results are robust when we allow for heterogenous treatment effect and survive a large set of placebo tests.

4.2.1 Exogeneity of PSL Mandates

An important assumption of the DiD design is that the implementation of PSL mandates is exogenous to household stock market participation. We perform three tests to support the exogeneity of PSL adoption.

First, we test for the parallel trend assumption, which requires that the treated observations and the control observations exhibit no significant differences in stockholdings prior to the adoption of PSL mandates. To that end, Figure 2 displays the dynamic timing effects of PSL mandates on household stock market participation for a 6-year event window [-6, +6] with a six-month time interval, and the excluded period is six months before the date when the PSL mandate becomes effective. As shown in Figure 2, the positively significant treatment effects of PSL mandates on household stock participation take place after, and not before, the effective date. Thus, there is no evidence of pre-trends or anticipatory effects before the implementation of PSL mandates, suggesting that the parallel trends assumption is likely to hold.

[Figure 2 around here]

Second, we show that the adoption of PSL mandates is unlikely to be driven by economic trends or political conditions in the state. To do this, we aggregate the data at the state-year level and regress the PSL dummy in year *t* on various state characteristics in year $t-1$,[10] including *GDP growth*, *Income growth*, *Log (Social insurance)*,[11] *Log (Employment)*, *Unemployment rate*, and a dummy variable indicating whether the state has a *Democrat governor*. As shown in Panel A of Table 3, the coefficients on each state characteristics are statistically insignificant, indicating that the adoption of PSL mandates is plausibly exogenous to the economic or political conditions in the state.

[Table 3 around here]

Third, we show that our baseline results are robust to controlling for time-varying state-level economic and political conditions. Specifically, we re-estimate the specification in Column

[10] We obtain the same conclusion when regressing the PSL dummy in year *t* on state characteristics in year *t*.
[11] Social insurance is the annual contribution to government social insurance programs in a given state. Appendix 2 displays definitions for all variables used in the paper.

(3) of Table 2 and additionally control for *GDP growth*, *Income growth*, *Log (Social insurance)*, *Log (Employment)*, *Unemployment rate*, and a dummy variable indicating whether the state has a *Democrat governor*. As shown in Panel B of Table 3, none of the state-level control variables are statistically significant. In contrast, we continue to find the coefficients on *PSL* to be positively significant. Overall, the results in Table 3 indicate that local economic or political conditions are unlikely to drive our main findings.

4.2.2 Placebo tests

Next, we conduct various placebo tests. If the enactment of PSL mandates is plausibly random, then in placebo tests where there is no difference in the provision of PSL, we should not observe significant differences in household stockholdings. To this end, we perform three placebo tests in Panel A of Table 4. Our first placebo test keeps the treatment location correct but alters the treatment date. Specifically, we assume that treatment starts earlier and lasts until the actual date when PSL becomes effective). Columns (1)-(4) present results where placebo effective dates are set for three months, six months, nine months, and one year before the true treatment date, respectively. As shown in Columns (1)-(4), none of the placebo DiD coefficients are statistically significant.

[Table 4 around here]

Our second placebo test keeps the treatment date correct but alters the treatment location by allocating the placebo adoption of PSL mandates to households in similar but untreated counties. To do this, we use a one-to-one nearest neighbor matching with replacement to obtain placebo treated households with similar characteristics to those located in the actual treated counties.[12] We

[12] We match based on all household covariates in Column (3) of Table 2.

also drop all the actual treated households. As shown in Column (5), the placebo coefficient is statistically insignificant. Overall, we do not continue to find significant effects when using the placebo time or placebo location, suggesting that our results are not likely to be driven by omitted variables, such as differences in the economic or political conditions between locations with and without PSL mandates.

Our third placebo test exploits the unsuccessful attempt to adopt a PSL mandate in Orange County, Florida. In particular, in 2012, Citizens for a Greater Orange County gathers signatures from registered voters in Orange County to place an initiative called "Earned Sick Time" on the ballot. Although the local population strongly supports the initiative, the Florida Governor signs House Bill 655 in June 2013, which prohibits political subdivisions from mandating employers to provide specific benefits, including paid sick time. This effectively ends efforts to mandate PSL coverage in Orange County (Huang and Shu, 2024). If our baseline results are driven by local economic conditions, we should continue to observe an increase in household stockholdings in Orange County despite the unsuccessful attempt to adopt the PSL mandate. Our treated observations are households in Orange County and the control observations are households in other counties in Florida. The event year is 2013. As shown in Column (6), the placebo coefficient is statistically insignificant.

4.2.3 Alternative DiD Estimators

Recent studies show that a staggered treatment design could lead to a biased estimation of causal effects, particularly when the treatment effects evolve over time (e.g., Baker et al., 2022; Borusyak et al., 2024; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). This bias arises when the treated observations are potentially compared with

control observations that have recently been treated, causing the estimated effect to capture the treatment effect that is in the process of materializing in the control observations. In Panel B of Table 4, we allow for treatment effect heterogeneity by estimating the group-time average treatment effects (ATE) based on a set of 2x2 comparisons (i.e., pre versus post treatment and control versus treated) following Callaway and Sant'Anna (2021).

In Panel C of Table 4, we further address this concern by using the stacked regression approach by Cengiz et al. (2019) and stack the event-specific data sets over the 13-year event window [-6, +6] with PSL mandates introduced at year 0. The sample includes households that are treated (i.e., those in counties with effective PSL mandates) in the same year and all other "clean" control households (i.e., those in counties never treated by PSL mandates).[13] Our results remain robust, which suggest that potential biases due to heterogeneous treatment effects are not a major threat to our analysis.

4.3 Mandate intensity

In this subsection, we perform various cross-sectional tests exploiting variation in the potential impact of PSL mandates on households. If our baseline results are indeed driven by the expanded sick leave coverage, we should expect stronger treatment effects when the PSL mandates have a greater potential impact on households.

Our first test considers heterogeneity in PSL generosity. We use two measures of PSL generosity: (1) *Accrual rate* is the rate at which PSL is earned per hour worked. For example, in Arizona, workers are able to accrue one hour of paid sick leave for every 30 hours worked,

[13] The alternative DiD estimation methods performed in this section require regular time gaps. Thus, instead of defining treatment based on survey date, we define it based on the survey wave.

implying an accrual rate of 0.033 (= 1/30). A higher accrual rate means that employees are able to accumulate paid sick leave at a faster pace, and thus indicates a more generous PSL policy; (2) *Annual cap* is the maximum hours of PSL that employees can accrue each year. For example, the annual cap in California is 24 hours. A higher annual cap thus indicates a more generous PSL policy. We test our hypothesis by interacting the *PSL* dummy with each generosity measure.

Panel A of Table 5 presents the results. In line with our hypothesis, we find positive and statistically significant interaction coefficients between *PSL* and the accrual rate (Column (1)) and annual cap (Column (2)). The interaction coefficient indicates that household stock market participation on average increases by 2.3% (=0.02 x 1.1477) following the adoption of a PSL mandate that has an accrual rate of 0.02. The estimated treatment effect increases to 3.8% when the accrual rate is 0.033.

[Table 5 around here]

Our second test exploits heterogeneity in households' coverage of PSL mandates across different industries. To test this hypothesis, we split our sample into two subsamples of (1) *High-PSL access industries*, where the household head is employed in information, financial activities, wholesale, or education and health services; and (2) *Low-PSL access industries*, where the household head is employed in leisure and hospitality, construction, retail, or other services (Al-Sabah and Ouimet, 2021). Individuals working in high-PSL access industries typically already have, on average, higher pre-mandate coverage of voluntary paid sick leave benefits provided by their employers, making them less likely to be impacted by the new PSL mandates. Thus, we expect that the effect of PSL mandates on stock market participation is less salient when the household head is already employed in high-PSL access industries.

Consistent with our expectation, the results in Panel B indicate that the enactment of PSL mandates does not have a statistically significant effect on stockholdings when the household head works in industries that already provide high access to voluntary paid sick leave benefits prior to the mandates (Column (1)). In contrast, stockholdings increase by 5.3% following the enactment of PSL mandates when the household head is employed in low-PSL access industries (Column (2)).

5. Economic Mechanisms

In this section, we evaluate three primary economic mechanisms through which PSL mandates promote household stock market participation: (1) offering households insurance-like protection, (2) enhancing households' subjective expectations, and (3) increasing household income and wealth.

5.1 Insurance-like protection

The first mechanism—*insurance-like protection*— posits that access to PSL benefits promotes stock market participation by offering households insurance-like protection. Specifically, by enabling sick workers to take paid time off for recovery without the risk of losing their income or job (Miller, 2022), PSL laws insure households against various sources of background risk related to health conditions, job security, and other contingencies affecting future earnings. This protection could therefore reduce the need for precautionary savings and encourage households to take more financial risk (e.g., Chou et al., 2003; Engen and Gruber, 2001; Hubbard et al., 1995). We test for this for this channel by exploiting variations across household vulnerability and examining the impact of PSL laws on household precautionary motives.

5.1.1 Vulnerable households

Our first set of tests for this channel exploits variations in household vulnerability. If PSL laws promote stock market participation by offering households insurance-like protection, this protection should be more valuable for vulnerable households facing precarious health or insecure employment situations. We examine this hypothesis by analyzing three key dimensions of household vulnerability: (1) health conditions, (2) employment conditions, and (3) exposure to wealth shocks.

First, we examine heterogeneity in household health conditions using two proxies: one focuses on the current health status of the household head, and the other captures their health trend over time. Specifically, *Poor health* is a dummy variable that equals one if the self-rated health of the household head is fair or poor, and zero if it is excellent, very good, or good; and *Declining health* is a dummy variable that equals one if the self-rated health of the household head is worse compared to two years ago, and zero if it is about the same or better compared to two years ago.[14] We test our hypothesis by interacting the *PSL* dummy with each health measure. Panel A of Table 6 displays the results.

[Table 6 around here]

Consistent with the insurance-like protection of PSL mandates, we find that the effect of PSL mandates is more pronounced among households with greater health concerns. In particular, the interaction coefficients between *PSL* and *Poor Health* (Column (1)) and *Declining Health* (Column (2)) are both positive and statistically significant. The interaction coefficients indicate that households with a head in *Poor health* (*Declining Health*) are 4.2% (6.2%) more likely to

[14] Two years prior refers to a retrospective question included in the questionnaire. Similarly, the unemployment two years ago in the next test is also a retrospective question.

participate in the stock market following the adoption of PSL mandates compared to the healthier counterparts.

Second, we exploit variations across households' employment situations using two proxies: (1) *Re-enter workforce*, a dummy variable that equals one if the household head is unemployed two years ago but is currently employed. Individuals who re-enter the workforce after a period of unemployment often have obsolete skills and limited networks, leading to a heightened sense of job insecurity. Since this analysis focuses on household heads who were unemployed in the past, our sample includes both unemployed and employed households.[15] (2) *Mass Layoffs/Employment*, the percentage of workers who experience mass layoffs to total employment in a county-survey wave.[16] Workers in areas with greater incidences of mass layoffs are likely to face increased unemployment risk (Arslan et al., 2024). As before, we test our hypothesis by interacting the *PSL* dummy with each measure of job insecurity.

As shown in Panel B of Table 6, the interaction coefficients are positive and statistically significant. This indicates that the effect of PSL mandates on stock holdings is stronger for households with greater job insecurity: households where the head recently re-enters the workforce following a period of unemployment (Column (1)) and households located in counties with higher mass layoff rates (Column (2)).

Third, in Panel C of Table 6, we examine heterogeneity in households' exposure to wealth shocks. Prior studies show that rational households would not participate in the stock market when they are exposed to wealth shocks that could jeopardize their ability to meet subsistence

[15] We find similar results focusing only on employed households.
[16] Data on mass layoffs come from the Worker Adjustment and Retraining Notification (WARN) database. According to the WARN act, when firms intend to perform a mass layoff, they will need to notify local governments in advance so that suitable support systems are available to the laid off workers. Employment data come from the Bureau of Economic Analysis (BEA).

consumption needs (Gormley et al., 2010). By offering insurance-like protection against such wealth shocks, PSL laws could particularly encourage stock market participation among these vulnerable households.

We consider a household to be vulnerable to wealth shocks if its subsistence-level consumption-to-wealth ratio falls in the highest decile.[17] We define subsistence-level consumption as total expenditures on foods, clothing, telephone, and transportation. For these households, a small decline in wealth could jeopardize their ability to maintain their minimum consumption levels. Consistent with the insurance-like protection of PSL mandates, we find in Panel C that the interaction coefficient between *PSL* and *Vulnerable households* is positive and marginally statistically significant, indicating that the effect of PSL mandates is more pronounced among households that are most vulnerable to wealth shocks.

Overall, the results in Table 6 provide strong support to the insurance-like channel and Hypothesis 1B. Our findings indicate that PSL laws promote stock market participation, particularly among vulnerable households, by mitigating background risks related to health conditions, job security, and other contingencies affecting future earnings. A novel aspect of our work is that we are able to directly link a household's health, employment, and wealth conditions to the extent they would benefit from having access to PSL. Our findings have important implications for policy discussions on the economic benefits of PSL laws.

5.1.2 Precautionary motives

Our second test of the insurance-like channel examines the impact of PSL laws on households' precautionary saving motives. Households set aside precautionary savings to buffer against labor

[17] We obtain similar results using alternative cutoffs, such as the top quintile or quartile of the subsistence-level consumption-to-wealth ratio.

income risk and other sources of background risk, such as medical expenses (e.g., Engen and Gruber, 2001; Hubbard et al., 1995). Access to PSL benefits can reduce uncertainty about the future, leading households to decrease their precautionary savings and increase their risky holdings (Chou et al., 2003; Engen and Gruber, 2001; Hubbard et al., 1995). We therefore predict that, following the enactment of PSL mandates, the composition of household portfolios will shift from safe financial assets to risky financial assets.

We test this using a new dependent variable, the ratio of the value of risky assets to the value of safe assets.[18] Safe assets include checking and savings accounts, certificates of deposit, government bonds, treasury bills, and money market funds.[19] Risky assets include stocks in publicly held corporations, stock mutual funds, and investment trusts, which indicate the value of the stocks held in the household. The regression specifications mirror those in Columns (1) to (3) of Table 2.

[Table 7 around here]

Table 7 displays the results. We find a 9% increase in the ratio of risky over safe assets following the enactment of a PSL mandate, indicating that households tilt their portfolios toward riskier financial assets. The results lend further support to the insurance-like channel that the PSL mandate insures households against job and income loss due to illness. This reduces the need for precautionary savings and encourages households to invest in the risky stock market.

5.2 Subjective expectations

[18] We use a natural logarithm because this variable has a skewed distribution (Arslan et al., 2024). We find similar results otherwise.

[19] The PSID only provides the total sum of safe assets and does not include a variable specifically for savings and bank deposits. Investments in money market funds and assets issued by the US government are considered risk-free and can be categorized as safe assets.

The second channel—*subjective expectations*— states that PSL policies could enhance subjective future expectations of households, leading to increased stock market participation. Prior studies show that welfare systems and public health insurance contribute to a sense of financial security and improve life satisfaction (Sirgy, 2012). PSL laws—by reducing disease transmission in the workplace and strengthening job protection—could improve employees' future expectations and incentivize them to stay longer in the workforce. All else equal, a longer expected retirement horizon and improved optimism about the future could encourage greater financial risk-taking (Choi and Robertson, 2020; Puri and Robinson, 2007; Viceira, 2001).

To test for this channel, we examine the impact of PSL mandates on the household head's future expectations. We use two outcome variables: (1) *Plan to work for longer,* a dummy that equals one if the household head's planned retirement age in the current survey wave is higher than in the previous survey wave; and (2) *Decline in life satisfaction*, a dummy that equals one if the household head's self-reported life satisfaction worsens to "not satisfied at all" in the current survey wave compared with the previous survey wave. Table 8 displays the results. As before, we condition our tests on households' health (Panel A), employment (Panel B), and wealth situations (Panel C).

[Table 8 around here]

The results in Panel A of Table 8 indicate that PSL mandates positively influence the future subjective expectations of households with health concerns. In particular, the interaction coefficients on *PSL* x *Poor health* and *PSL* x *Declining health* suggest that after the enactment of PSL mandates, household heads with poor or declining health are more likely to extend their planned retirement age (Columns (1)-(2)) and are less likely to experience a decline in life satisfaction (Columns (3)-(4)).

We then analyze households facing unstable employment situations in Panel B and those that are vulnerable to wealth shocks in Panel C. All interaction coefficients in Panels B and C are statistically insignificant, indicating that PSL laws do not affect the subjective expectations of households facing unstable employment situations and those vulnerable to wealth shocks. Thus, the effect of PSL laws on improving subjective expectations is primarily driven by households with health concerns, consistent with the laws' intent to address health-related issues. Overall, our findings suggest that paid time off and healthier work environments introduced by PSL laws enhance the subjective future expectations of health-vulnerable households, which can, in turn, promote stock market participation (Choi and Robertson, 2020; Puri and Robinson, 2007). The results are also in line with the "horizon effect" discussed in Hugonnier et al. (2013), which shows that stock holdings and life expectancy both increase as an agent's health improves.

5.3 Household wealth

The third channel—*household wealth*— hypothesizes that PSL mandates could promote stock market participation by enhancing household income and wealth. While PSL laws do not provide immediate cash gains, it could positively affect household income and wealth accumulation in several ways. PSL mandates not only ensure that sick workers continue to receive pay while they stay at home to recover from their illness, these laws can also increase household income and wealth by encouraging workers to work longer hours, take on additional jobs, and become more productive (Al-Sabah and Ouimet, 2023; Chunyu et al., 2024). In addition, PSL laws also enable workers to seek timely medical care, allowing households to avoid potentially costly healthcare expenses that could otherwise worsen their financial situations (Angrisani et al., 2018; Miller, 2022). We perform two tests to explore this channel.

First, we examine the impact of PSL mandates on households' total income and wealth. The dependent variables are *Log (Total household income)*, the natural logarithm of total household income, *Log (Non-housing wealth),* the natural logarithm of total household wealth that excludes home equity, and *Log (Housing wealth),* the natural logarithm of the value of home equity. Panel A of Table 9 displays the results.

[Table 9 around here]

We find in Panel A that households experience an approximately 1% increase in total income (Column (1)) and a 0.9% increase in non-housing wealth (Column (2)) following the enactment of a PSL law. Our findings are consistent with those documented in Al-Sabah and Ouimet (2023) that the provision of PSL has a positive impact on household income and wealth. In contrast, the provision of PSL does not affect housing wealth (Column (3)). This is expected since housing wealth is slow-moving and tends to reflect real estate market conditions.

Second, we directly examine how income and wealth growth following the adoption of PSL mandates promotes stock market participation. We interact the *PSL* dummy with three variables: (1) income growth, (2) non-housing wealth growth, and (3) housing wealth growth.[20] The dependent variable is household stock market participation. As shown in Panel B, the coefficients on the interaction between *PSL* and *Non-housing wealth growth* is positive and statistically significant (Column (2)). In contrast, the interaction coefficients on *PSL* x *Income growth* (Column (1)) and *PSL* x *Housing wealth growth* (Column (3)) are insignificant. Thus, in line with the wealth channel, households invest some of their non-housing wealth gains after PSL laws in the stock markets.

[20] Each growth variable is calculated as the difference between the natural logarithm of its value in the current survey wave and the natural logarithm of its value in the previous survey wave.

6. Additional tests

6.1 Other household financial decisions

While our conceptual framework and main analyses focus on households' investment decisions, for completeness, we further investigate the effects of the adoption of PSL mandates on other major household financial decisions: consumption and borrowing. We start by analyzing household consumption. The effect of PSL provision on consumption is unclear ex ante. On the one hand, since PSL mandates provide an insurance-like protection against possible wealth shocks, this could encourage households to increase their spending, especially on non-essential goods and services. Additionally, the increase in household income following the implementation of a PSL mandate could lead to higher consumption. On the other hand, households may use their income and wealth gains to invest while maintaining the same consumption level.

[Table 10 around here]

We test this idea using several measures of household consumption: total expenditure, subsistence-level expenditure, discretionary expenditure (which is the difference between total expenditure and subsistence-level expenditure), and healthcare expenditure. All variables are in natural logarithms. The results are displayed in Panel A of Table 10. Across all columns, the coefficient estimates on *PSL* are statistically insignificant, suggesting that the adoption of PSL laws does not have a detectable impact on household consumption.

Panel B of Table 10 examines the impact of PSL laws on household borrowing decisions. Similar to consumption, the impact of PSL mandates on housing borrowing is also theoretically ambiguous. While improved access to sick leave benefits and increases in household income could reduce households' reliance on borrowing, this could also improve households' creditworthiness which facilitates more borrowing (Bornstein and Indarte, 2023).

Following the literature, we focus on two categories of household borrowings: (i) credit card debt (Columns (1)-(2)), and (ii) other types of debts, including student loans, medical bills, legal bills, loans from relatives (Columns (3)-(4)). The dependent variables in odd-numbered columns are dummy variables indicating whether the household has any of these loans, and the dependent variables in even-numbered columns are the natural logarithm of the dollar value of these loans. As shown in Panel B of Table 10, none of the coefficients on *PSL* are statistically significant, indicating that households do not change their borrowing behavior after the enactment of PSL laws.

6.2 Households' financial obligations

In Appendix 4, we condition our baseline results on households' financial obligations. When households are burdened with financial obligations, such as a mortgage, the opportunity costs to invest in the stock market are higher and could outweigh the potential benefits of PSL mandates. To test this, we interact the *PSL* dummy with five measures of households' financial obligations: (1) whether households have an outstanding mortgage loan, (2) the natural logarithm of the remaining mortgage amount to pay off, and (3) the natural logarithm of monthly payment of the mortgage, (4) the natural logarithm of the interest rate on the mortgage, and (5) the natural logarithm of the remaining years of the mortgage. Across all five measures, we find that the effect of PSL on stock holdings is less salient among households with greater financial obligations. This suggests that financial obligations could crowd out the positive impact of PSL laws on stock market participation.

6.3 Further robustness tests on baseline findings

Appendix 5 presents other robustness tests. We find that our baseline results are robust to the following empirical variations: (i) excluding households that move to other counties during the sample period (Column (1)); (ii) excluding households in locations that already have an effective paid sick leave mandate before 2009 (Column (2)); (ii) excluding households in the large treated states of California (Column (3)), Washington (Column (4)), Oregon (Column (5)), and all of these three states ((Column (6)), since these large states may dominate the sample due to their substantial number of treated counties; (iii) using only the state-level PSL mandates (Column (7)); (iii) using unweighted variables from the PSID (Column (8)); (iv) considering the size of the firm that employs the household head when defining the *PSL* dummy (Column (9));[21] (v) using a one-to-one propensity score matched (PSM) of households in treated counties to those in untreated counties (Column (10)); (vi) using a PSM of households in treated counties to those in untreated counties to be located in coastal states, as numerous treated states are near the coastline (Column (11));[22] (vii) controlling for a dummy that equals one if members of the household have health insurance (Column (12)), additionally including industry-year fixed effects to account for the possibility that, for example, households may invest more if their employers have strong performance (Column (13)), additionally controlling for county-specific time trends (Column (14)); and (viii) using *Fraction of risky assets,* defined as the ratio of the value of stocks to total financial assets as a dependent variable (Column (15)).

7. Conclusions

[21] In some states, the PSL accrual rates and annual caps are different for smaller firms with occasional exemptions for the smallest firms. Our baseline models do not consider firm size because this variable has many missing values.
[22] In Columns (10) and (11), we perform a one-to-one match based on all household covariates.

The US is only one of two OECD countries that do not provide a federal paid sick leave law. In this paper, we offer micro-level evidence on how the implementation of local paid sick leave laws affects households' portfolio choice. We document a 20% increase in the average stock market participation following the enactment of a paid sick leave policy. The effect is identified *within* the same household and after controlling for a large set of household demographics and financial conditions and other public safety net programs. We propose and find evidence supporting three economic channels through which PSL mandates promote household stockholdings. That is, provision of paid sick leave offers households insurance-like protection, enhances households' expectations, and improves household wealth.

Our paper is the first to investigate how paid sick leave mandates affect household financial decisions and portfolio choice. Our results indicate that in addition to providing health benefits, PSL mandates create a positive economic externality by offering job and financial security against short-term illnesses. This could, in turn, promote stock market participation and facilitate long-term wealth accumulation. Our findings have important implications for policymakers and local governments in evaluating the benefits and costs of sick leave policies.

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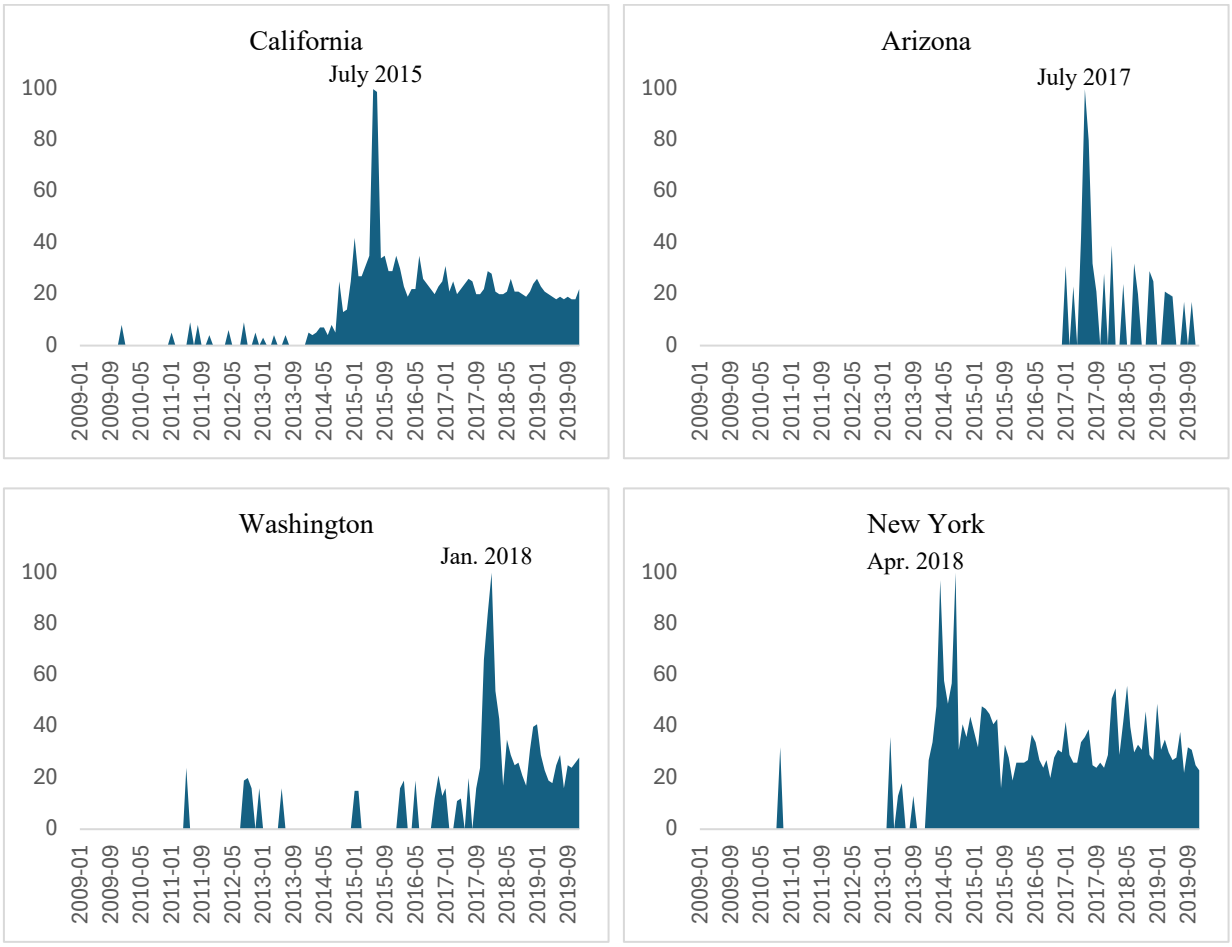
Figure 1: Google Trends for PSL

This figure displays search interest for the keyword "paid sick leave" from 2009 to 2019 on Google Trends. The search data are normalized on a scale from 1 to 100, where 100 represents the maximum search interest for the selected period and location. Panel A displays the internet search interest for "paid sick leave" in four randomly selected states that passed PSL law (California, Arizona, Washington, and New York). Panel B displays the internet search interest for "paid sick leave" in two randomly selected states that have not passed PSL law (Virginia and Alabama).

Panel A: Internet search interest for "paid sick leave" in four randomly selected states that passed PSL laws



Panel B: Internet search interest for “paid sick leave” in two randomly selected states that have not passed a PSL law

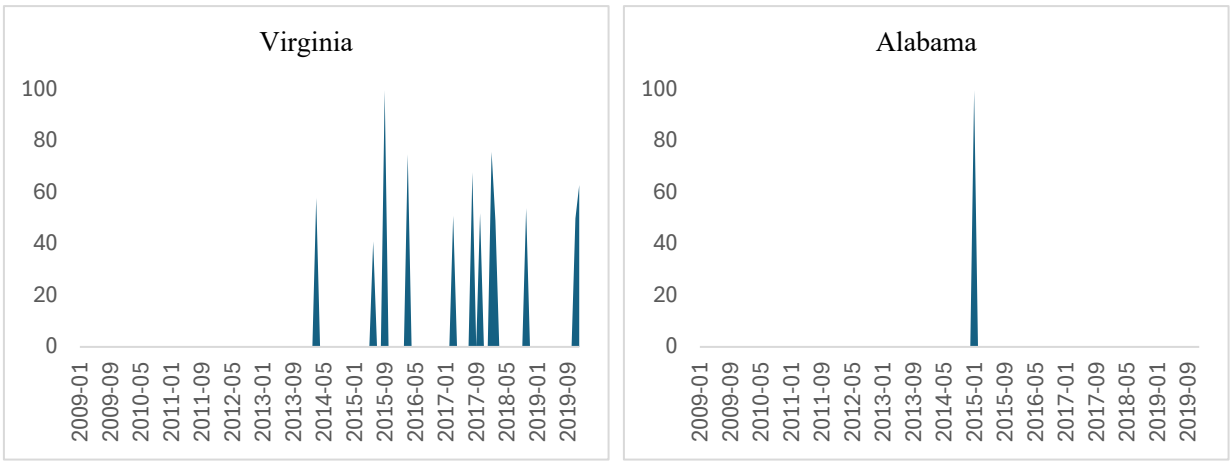


Figure 2: Dynamic Effects of PSL on Household Stock Market Participation
This figure shows the dynamic effects of the adoption of PSL on household stock market participation with point estimates and a 95% confidence interval. On the y-axis, the graph plots the coefficient estimates from Column (3) of Table 2 where we decompose the *PSL* variable into a series of dummy variables: $PSL_{\leq -6}$, PSL_{-5}, PSL_{-4}, PSL_{-3}, PSL_{-2}, PSL_{0}, PSL_{+1}, PSL_{+2}, PSL_{+3}, PSL_{+4}, PSL_{+5}, and $PSL_{\geq +6}$. The time interval is six months, and the excluded period is six months before the date when the PSL mandate becomes effective.

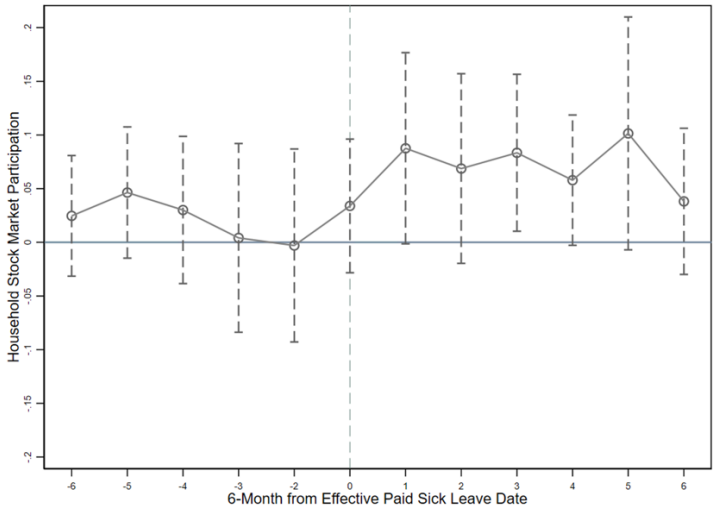


Table 1 Summary Statistics
This table reports the descriptive statistics of the variables in the paper. Appendix 2 provides descriptions of the variables.

Variable name	Observations	Mean	Std. Dev	Median	P5	P95
Key dependent variable						
Household stock market participation	20,998	0.187	0.390	0.000	0.000	1.000
Key independent variable						
PSL	20,998	0.118	0.323	0.000	0.000	1.000
Household characteristics						
Family size	20,998	2.329	1.351	2.000	1.000	5.000
House ownership	20,998	0.658	0.475	1.000	0.000	1.000
Has a mortgage	20,998	0.495	0.500	0.000	0.000	1.000
Total household wealth (in 10 thousand dollars)	20,998	39.303	144.375	8.050	-3.050	159.000
Total household income (in 10 thousand dollars)	20,998	9.669	11.894	7.220	1.630	24.011
Mortgage balance (in 10 thousand dollars)	20,998	7.741	12.074	0.000	0.000	31.000
Log (Total household wealth)	20,998	14.771	0.285	14.691	14.644	15.179
Log (Total household income)	20,998	12.780	0.215	12.737	12.557	13.138
Log (Mortgage balance)	20,998	5.751	5.844	0.000	0.000	12.644
Household head characteristics						
Age	20,998	48.500	13.349	49.000	27.000	70.000
College	20,998	0.678	0.467	1.000	0.000	1.000
Married	20,998	0.575	0.494	1.000	0.000	1.000
Non-White	20,998	0.196	0.397	0.000	0.000	1.000
Hispanic	20,998	0.094	0.291	0.000	0.000	1.000
Poor physical health	20,998	0.020	0.142	0.000	0.000	0.000
Poor mental health	20,998	0.014	0.116	0.000	0.000	0.000
Other public safety nets						
PFL	20,998	0.154	0.361	0.000	0.000	1.000
ACA	20,998	0.344	0.475	0.000	0.000	1.000
UI benefits	20,998	8.815	1.048	9.036	7.987	9.579

Table 2: PSL Mandates and Household Stock Market Participation

This table reports regressions that estimate the effect of the enactment of PSL mandates on household stock market participation. The dependent variable is *Household stock market participation*, a dummy variable that equals one if the household holds stocks, stock mutual funds, or investment trusts, and zero otherwise. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Dependent variable:	*Household stock market participation*		
	(1)	(2)	(3)
PSL	0.0609***	0.0383***	0.0383***
	(0.0150)	(0.0129)	(0.0134)
Family size		-0.0245***	-0.0030
		(0.0047)	(0.0091)
House ownership		0.1134***	0.0465**
		(0.0294)	(0.0185)
Has a mortgage		-0.2611***	0.0494
		(0.0860)	(0.0665)
Log (Total household wealth)		0.2398*	0.0514
		(0.1349)	(0.0583)
Log (Total household income)		0.4369***	0.0798**
		(0.1054)	(0.0404)
Log (Mortgage balance)		0.0177**	-0.0059
		(0.0080)	(0.0062)
Age			0.0186
			(0.0122)
Age^2			-0.0058
			(0.0043)
College			0.0071
			(0.0419)
Married			0.0296
			(0.0342)
Non-White			0.0635
			(0.0593)
Hispanic			0.0285
			(0.0213)
Poor physical health			-0.0147
			(0.0240)
Poor mental health			0.0250
			(0.0229)
PFL			-0.0417
			(0.0381)
ACA			-0.0256**
			(0.0124)
UI benefits			0.0048*
			(0.0027)
Survey year FE	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes
Household FE	No	No	Yes
County FE	No	No	Yes
N	20,998	20,998	20,998
R2	0.013	0.170	0.698

Table 3: Exogeneity of PSL Mandates

This table performs various tests to evaluate the exogeneity of the passage of PSL mandates. Panel A reports regressions that estimate the effect of prior state-level economic and political factors on the decision to adopt PSL mandates. The dependent variable is *PSL*, a dummy variable that equals one if the state has an effective PSL law in year *t*. The independent variables are *GDP growth*, *Income growth*, *Log (Social insurance)*, *Log (Employment)*, *Unemployment rate*, and a dummy variable indicating whether the state has a *Democrat governor*. All independent variables are measured in year *t*-1. Standard errors in Panel A are clustered at the state-level and are reported in parentheses. Panel B re-estimates the baseline regressions in Column (3) of Table 2 but additionally controls for time-varying state-level economic and political characteristics. The dependent variable is *Household stock market participation*, a dummy variable that equals one if the household holds stocks, stock mutual funds, or investment trusts. The independent variable of interest is *PSL*, a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Control variables in Panel B are collapsed for brevity and are identical to Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: Determinants of the passage of PSL laws

Dependent variable:	*PSL*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GDP growth	0.4366						0.0972
	(0.4331)						(0.5219)
Income growth		0.7352					-0.0400
		(0.6013)					(0.6362)
Log (Social insurance)			1.0741				0.4762
			(0.7019)				(1.2953)
Log (Employment)				1.5351			0.5545
				(0.9887)			(2.0209)
Unemployment rate					-3.7727		-2.5156
					(2.3034)		(2.6237)
Democrat governor						0.0359	0.0286
						(0.0579)	(0.0604)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	510	510	510	510	510	510	510
R2	0.489	0.489	0.500	0.503	0.499	0.489	0.508

Panel B: Controlling for economic and political variables

Dependent variable:	*Household stock market participation*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PSL	0.0387***	0.0383***	0.0421***	0.0406***	0.0384***	0.0400***	0.0435***
	(0.0134)	(0.0134)	(0.0135)	(0.0136)	(0.0136)	(0.0136)	(0.0139)
GDP growth	-0.1366						-0.2604
	(0.1690)						(0.2332)
Income growth		-0.0157					0.3043
		(0.2141)					(0.2948)
Log (Social insurance)			-0.1473				-0.2770
			(0.1208)				(0.2439)
Log (Employment)				-0.1063			0.1652
				(0.1428)			(0.3094)
Unemployment rate					0.0225		-0.1201
					(0.4282)		(0.5024)
Democrat governor						-0.0108	-0.0102
						(0.0088)	(0.0089)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	20,998	20,998	20,998	20,998	20,998	20,998	20,998
R2	0.698	0.698	0.698	0.698	0.698	0.698	0.698

Table 4: Placebo Tests and Alternative DiD

Panel A reports the results of placebo tests. Columns (1) to (4) present results where the placebo effective dates are set three months, six months, nine months, and one year before the true treatment, respectively, and the placebo treatment lasts until the actual date when the PSL law becomes effective. In Column (5), we focus on placebo households that are located in non-treated counties but share similar characteristics with the treated counties, and the regressions drop all the actual treated households. In Column (6), we exploit the unsuccessful attempt to adopt a PSL mandate in Orange County, Florida. Placebo treated observations are households in Orange County, Florida, and control observations are households in other counties in Florida. Panel B reports the DiD method proposed by Callaway and Sant'Anna (2021). Panel C reports the stacked regression approach by Cengiz et al. (2019). Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: Placebo tests

Dependent variable:	*Household stock market participation*					
Placebo types:	Placebo treatment timing				Placebo treatment location	Orange county, FL
	(1)	(2)	(3)	(4)	(5)	(6)
PSL placebo	-0.0076	-0.0132	-0.0204	-0.0213	-0.0041	0.0270
	(0.0211)	(0.0191)	(0.0170)	(0.0153)	(0.0109)	(0.1049)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
N	20,998	20,998	20,998	20,998	18,599	831
R2	0.697	0.697	0.697	0.698	0.712	0.712

Panel B: Callaway and Sant'Anna (2021) approach

Dependent variable:	*Household stock market participation*
PSL	0.0386**
	(0.0173)

Panel C: Cengiz et al. (2019) approach

Dependent variable:	*Household stock market participation*
$PSL_{\leq -6}$	0.0332
	(0.0247)
PSL_{-5}	-0.0017
	(0.0237)
PSL_{-4}	0.0313
	(0.0291)
PSL_{-3}	-0.0087
	(0.0202)
PSL_{-2}	0.0326
	(0.0293)
PSL_{0}	0.0392
	(0.0296)
PSL_{+1}	0.0482**
	(0.0220)
PSL_{+2}	0.0584*
	(0.0304)
PSL_{+3}	-0.0285
	(0.0320)
PSL_{+4}	0.0592*
	(0.0326)
PSL_{+5}	-0.0218
	(0.0487)
$PSL_{\geq +6}$	0.0125
	(0.0549)

Table 5: Mandate Intensity

This table reports regressions that estimate the effect of the PSL mandate intensity on household stock market participation. The dependent variable is *Household stock market participation*, a dummy that equals one if the household holds stocks, stock mutual funds, or investment trusts, and zero otherwise. *PSL* is a dummy that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Panel A reports results on PSL mandate generosity on household stock market participation. *Accrual rate* is the rate at which paid sick leave is earned per hour worked. *Annual cap* is the maximum hours of paid sick leave that an employee is allowed to accrue each year. Panel B splits the sample into two subsamples of *Low-PSL access industries*, where the household head is employed in leisure and hospitality, construction, retail, or other services (Column (1)) and *High-PSL access industries*, where the household head is employed in information, financial activities, wholesale, or education and health services (Column (2)). Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: PSL generosity across locations

Dependent variable:	*Household stock market participation*	
	(1)	(2)
*PSL*Accrual rate*	1.1477***	
	(0.4232)	
*PSL*Annual cap*		0.0009**
		(0.0004)
Control variables	Yes	Yes
Survey year FE	Yes	Yes
Survey month FE	Yes	Yes
Household FE	Yes	Yes
County FE	Yes	Yes
N	20,998	20,998
R2	0.698	0.698

Panel B: Heterogeneity across industries

Dependent variable:	*Household stock market participation*	
Sample:	Low-PSL access industries	High-PSL access industries
	(1)	(2)
PSL	0.0533**	0.0038
	(0.0251)	(0.0278)
Control variables	Yes	Yes
Survey year FE	Yes	Yes
Survey month FE	Yes	Yes
Household FE	Yes	Yes
County FE	Yes	Yes
N	5,529	5,524
R2	0.707	0.716

Table 6: Vulnerable Households

This table reports results on vulnerable households. The dependent variable is *Household stock market participation*, a dummy variable that equals one if the household holds stocks, stock mutual funds, or investment trusts, and zero otherwise. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. In Panel A, *Poor health* is a dummy that equals one if the self-rated health of the household head is fair or poor, and zero if it is excellent, very good, or good. *Declining health* is a dummy that equals one if self-rated health of the household head is worse compared to two years ago, and zero if it is about the same or better compared to two years ago. In Panel B, *Re-enter workforce* is a dummy variable that equals one if the household head is unemployed two years ago but is currently employed. *Mass Layoffs/Employment* is a county-level ratio of workers who experience mass layoffs to total county employment. In Panel C, *Vulnerable households* is a dummy variable that equals one if the household's subsistence-level consumption-to-wealth ratio falls in the highest decile. Subsistence-level consumption is the total expenditure on food, clothing, telephone, and transportation. Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: Households facing health concerns

Dependent variables:	*Household stock market participation*	
	(1)	(2)
PSL*Poor health	**0.0415****	
	(0.0208)	
PSL*Declining health		**0.0622***
		(0.0355)
Poor health	-0.0029	
	(0.0082)	
Declining health		-0.0159
		(0.0105)
PSL	0.0370***	0.0366***
	(0.0134)	(0.0134)
Control variables	Yes	Yes
Survey year FE	Yes	Yes
Survey month FE	Yes	Yes
Household FE	Yes	Yes
County FE	Yes	Yes
N	20,998	20,992
R2	0.698	0.698

Panel B: Households facing job security concerns

Dependent variable:	*Household stock market participation*	
	(1)	(2)
PSL*Re-enter workforce	**0.0708****	
	(0.0354)	
PSL*(Mass layoff/Employment)		**0.1049***
		(0.0614)
Re-enter workforce	-0.0027	
	(0.0103)	
Mass layoff/Employment		0.1231
		(0.1658)
PSL	0.0188	0.0329**
	(0.0121)	(0.0134)
Control variables	Yes	Yes
Survey year FE	Yes	Yes
Survey month FE	Yes	Yes
Household FE	Yes	Yes
County FE	Yes	Yes
N	29,593	20,844
R2	0.699	0.699

Panel C: Households exposed to wealth shocks

Dependent variable:	*Household stock market participation*
	(1)
PSL* Vulnerable households	**0.0562***
	(0.0328)
Vulnerable households	-0.0062
	(0.0068)
PSL	0.0418***
	(0.0137)
Control variables	Yes
Survey year FE	Yes
Survey month FE	Yes
Household FE	Yes
County FE	Yes
N	20,122
R2	0.697

Table 7: PSL Mandates and Household Portfolio Composition

This table reports regressions that estimate the effect of the enactment of PSL mandates on the composition of household portfolios. The dependent variable is *Risky asset ratio*, the ratio of the value of risky assets to the value of safe assets. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Control variables are collapsed for brevity and mirror those in Columns (1) to (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Dependent variable:	*Risky asset ratio*		
	(1)	(2)	(3)
PSL	0.0918***	0.0514**	0.0648**
	(0.0277)	(0.0260)	(0.0299)
Household characteristics	No	Yes	Yes
Household head characteristics	No	No	Yes
Other safety nets	No	No	Yes
Survey year FE	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes
Household FE	No	No	Yes
County FE	No	No	Yes
N	19,722	19,722	19,722
R2	0.007	0.086	0.626

Table 8: Subjective Expectations

This table reports regressions that estimate the effect of the enactment of PSL mandates on subjective expectations. The dependent variables are (1) *Plan to work for longer*, a dummy that equals one if the household head's planned retirement age in the current survey wave is higher than in the previous survey wave, and (2) *Decline in life satisfaction*, a dummy that equals one if the household head's self-reported life satisfaction worsens to "not satisfied at all" in the current survey wave compared with the previous survey wave. In Panel A, *Poor health* is a dummy that equals one if the self-rated health of the household head is fair or poor, and zero if it is excellent, very good, or good. *Declining health* is a dummy that equals one if self-rated health of the household head is worse compared to two years ago, and zero if it is about the same or better compared to two years ago. In Panel B, *Re-enter workforce* is a dummy variable that equals one if the household head is unemployed two years ago but is currently employed. *Mass Layoffs/Employment* is a county-level ratio of workers who experience mass layoffs to total county employment. In Panel C, *Vulnerable households* is a dummy variable that equals one if the household's subsistence-level consumption-to-wealth ratio falls in the highest decile. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: Variation in health

Dependent variables:	*Plan to work for longer*		*Decline in life satisfaction*	
	(1)	(2)	(3)	(4)
PSL*Poor health	**0.1707****		**-0.0112***	
	(0.0848)		**(0.0065)**	
PSL*Declining health		**0.4199*****		**-0.0151****
		(0.1104)		**(0.0063)**
Poor health	-0.0464		0.0033	
	(0.0404)		(0.0025)	
Declining health		-0.0124		0.0157***
		(0.0621)		(0.0060)
PSL	0.0007	-0.0015	0.0009	0.0010
	(0.0489)	(0.0484)	(0.0033)	(0.0032)
Control variables	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
N	3,230	3,230	12,852	12,848
R2	0.353	0.356	0.402	0.404

Panel B: Variation in employment stability

Dependent variables:	*Plan to work for longer*		*Decline in life satisfaction*	
	(1)	(2)	(3)	(4)
*PSL*Re-enter workforce*	0.0969		-0.0004	
	(0.1843)		(0.0061)	
PSL(Mass layoff/Employment)*		-0.2613		0.0021
		(0.2438)		(0.0081)
Re-enter workforce	-0.1470		-0.0008	
	(0.1239)		(0.0060)	
Mass layoff/Employment		-0.6994		-0.0089
		(0.9353)		(0.0146)
PSL	0.0280	0.0079	0.0067	0.0006
	(0.0494)	(0.0499)	(0.0044)	(0.0033)
Control variables	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
N	3,356	3,203	19,603	12,748
R2	0.353	0.350	0.373	0.401

Panel C: Variation in wealth shocks

Dependent variables:	*Plan to work for longer*	*Decline in life satisfaction*
	(1)	(2)
PSL Vulnerable households*	-0.3039	0.0054
	(0.1962)	(0.0062)
Vulnerable households	-0.0144	-0.0072
	(0.0883)	(0.0067)
PSL	-0.0154	0.0006
	(0.0530)	(0.0033)
Control variables	Yes	Yes
Survey year FE	Yes	Yes
Survey month FE	Yes	Yes
Household FE	Yes	Yes
County FE	Yes	Yes
N	3,181	12,438
R2	0.352	0.407

Table 9: Household wealth

This table reports regressions that estimate the effect of the enactment of PSL mandates on wealth. In Panel A, the dependent variables are *Log* (*Total household income),* the natural logarithm of the household total income (Column (1)), *Log (Non-housing wealth)*, the natural logarithm of the household wealth that excludes home equity (Column (2)), and *Log (Housing wealth),* the natural logarithm of home equity value (Column (3)). In Panels B and C, the dependent variable is *Household stock market participation*, which is a dummy variable that equals one if the household holds stocks, stock mutual funds, or investment trusts, and zero otherwise. *Income growth* is the logarithm of household income in current survey wave minus that in the prior survey wave. (*Non)-Housing wealth growth* is the logarithm of (non-)housing wealth in current survey wave minus that in the prior survey wave. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: Impact of PSL mandates on household income and wealth

Dependent variables:	*Log (Total household income)*	*Log (Non-housing wealth)*	*Log (Housing wealth)*
	(1)	(2)	(3)
PSL	0.0098**	0.0090*	0.0197
	(0.0050)	(0.0051)	(0.0214)
Control variables	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes
Household FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
N	20,998	20,998	20,779
R2	0.845	0.580	0.998

Panel B: Income and wealth growth and stock market participation

Dependent variable:	*Household stock market participation*		
	(1)	(2)	(3)
*PSL*Income growth*	-0.1163		
	(0.0942)		
*PSL*Non-housing wealth growth*		0.2960***	
		(0.1079)	
*PSL*Housing wealth growth*			-0.0045
			(0.0039)
Income growth	-0.0319		
	(0.0518)		
Non-housing wealth growth		0.0192	
		(0.0436)	
Housing wealth growth			0.0003
			(0.0015)
PSL	0.0451***	0.0425***	0.0447***
	(0.0162)	(0.0162)	(0.0164)
Control variables	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes
Household FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
N	12,931	12,931	12,772
R2	0.722	0.723	0.721

Table 10: The impact of PSL mandates on household consumption and borrowing

This table reports regressions that estimate the effect of PSL on household consumption and borrowing decisions. *PSL* is a dummy that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. In Panel A, the dependent variables are *Log (Total expenditure)* (Column (1)), *Log (Subsistence-level expenditure)* (Column (2)), *Log (Discretionary expenditure)* (Column (3)), *Log (Expenditure on health care)* (Column (4)). In Panel B, the dependent variables are *Credit card debt dummy* (Column (1)), *Log (Credit card debt)* (Column (2)), *Other loan dummy* (Column (3)), and *Log (Other loan value)* (Column (4)). Variables are defined in Appendix 2. Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Panel A: Household consumption

Dependent variables:	*Log (Total expenditure)*	*Log (Subsistence-level expenditure)*	*Log (Discretionary expenditure)*	*Log (Expenditure on health care)*
	(1)	(2)	(3)	(4)
PSL	-0.0056	0.0014	-0.0141	0.0399
	(0.0161)	(0.0200)	(0.0790)	(0.0321)
Control variables	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
N	20,998	20,998	20,998	20,998
R2	0.880	0.794	0.742	0.737

Panel B: Household borrowing

Dependent variables:	*Credit card debt dummy*	*Log (Credit card debt)*	*Other loan dummy*	*Log (Other loan value)*
	(1)	(2)	(3)	(4)
PSL	0.0231	0.2011	0.0041	0.0435
	(0.0182)	(0.1508)	(0.0182)	(0.1641)
Control variables	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
N	17,218	17,155	17,235	17,235
R2	0.703	0.733	0.707	0.749

Appendix 1: Paid Sick Leave Mandates
This table reports summary of the state and local PSL mandates.

Panel A: State paid sick leave mandates

State	Enactment Date	Effective Date	Accrual Rate	Annual Cap
Connecticut	2011-07-01	2012-01-01	>=50 employees:1 hour for every 40 hours worked	>=50 employees: 40 hours per year
California	2014-09-19	2015-07-01	1 hour for every 30 hours worked	24 hours per year
Massachusetts	2014-11-04	2015-07-01	>10 employees: 1 hour for every 30 hours worked <=10 employees: 1 hour of unpaid sick leave for every 30 hours worked	40 hours per year
Oregon	2015-06-12	2016-01-01	>=10 employees: 1 hour for every 30 hours worked <10 employees: 1 hour of unpaid sick leave for every 30 hours worked	40 hours per year
Vermont	2016-03-09	2017-01-01	1 hour for every 52 hours worked	24 hours (40 hours from 2019) per year
Arizona	2016-11-08	2017-07-01	1 hour for every 30 hours worked	>=15 employees: 40 hours per year <15 employees: 24 hours per year
Washington	2016-11-09	2018-01-01	1 hour for every 40 hours worked	No cap (no more than 40 hours carry over)
Maryland	2017-04-05	2018-02-11	>=15 employees: 1 hour for every 30 hours worked <15 employees: 1 hour of unpaid sick leave for every 30 hours worked	40 hours per year
Rhode Island	2017-09-19	2018-07-01	>=18 employees: 1 hour for every 35 hours worked <18 employees: 1 hour of unpaid sick leave for every 35 hours worked	24 hours (32 hours in 2019 and 40 hours thereafter) per year
New Jersey	2018-05-02	2018-10-29	1 hour for every 30 hours worked	40 hours per year
Michigan	2018-12-14	2019-03-29	>=50 employees: 1 hour for every 35 hours worked	>=50 employees: 40 hours per year

Panel B: Local paid sick leave mandates

City	County	Enactment Date	Effective Date	Accrual Rate	Annual Cap
San Francisco, CA	San Francisco	2006-11-07	2007-02-05	1 hour for every 30 hours worked	<10 employees: 40 hours per year >=10 employees: 72 hours per year
Washington, DC	D.C.	2008-05-13	2008-11-13	<=24 employees: 1 hour for every 87 hours worked 25-99 employees: 1 hour for every 43 hours worked >=100 employees: 1 hour for every 37 hours worked	<=24 employees: 24 hours per year 25-99 employees: 40 hours per year >=100 employees: 56 hours per year
Seattle, WA	King	2011-09-12	2012-09-01	<250 employees: 1 hour for every 40 hours worked >=250 employees: 1 hour for every 30 hours worked	40 hours per year
Portland, OR	Multnomah	2013-03-13	2014-01-01	>5 employees: 1 hour for every 30 hours worked <=5 employees: 1 hour of unpaid sick leave for every 30 hours worked	40 hours per year
Jersey City, NJ	Hudson	2013-03-13	2014-01-01	1 hour for every 30 hours worked	>=10 employees: 40 hours per year <10 employees: 24 hours per year
New York, NY	New York, Kings, Bronx, Richmond, Queens	2013-06-26	2014-04-01	>=5 employees: 1 hour for every 30 hours worked <5 employees: 1 hour of unpaid sick leave for every 30 hours worked	40 hours per year
Newark, NJ	Essex	2014-01-29	2014-06-21	1 hour for every 30 hours worked	>=10 employees: 40 hours per year <10 employees: 24 hours per year
Paterson, NJ	Passaic	2014-09-02	2015-01-01	1 hour for every 30 hours worked	>=10 employees: 40 hours per year <10 employees: 24 hours per year
Oakland, CA	Alameda	2014-11-04	2015-03-02	1 hour for every 30 hours worked	>=10 employees: 72 hours per year <10 employees: 40 hours per year
Trenton, NJ	Mercer	2014-11-04	2015-03-04	1 hour for every 30 hours worked	>=10 employees: 40 hours per year <10 employees: 24 hours per year

Philadelphia, PA	Philadelphia	2015-02-12	2015-05-13	>=10 employees: 1 hour for every 40 hours worked <10 employees: 1 hour of unpaid sick leave for every 40 hours worked	40 hours per year
New Brunswick, NJ	Middlesex	2015-12-17	2016-01-06	1 hour for every 35 hours worked	>=10 employees: 40 hours per year 5-9 employees: 24 hours per year
Tacoma, WA	Pierce	2015-01-27	2016-02-01	1 hour for every 40 hours worked	40 hours per year
Elizabeth, NJ	Union	2015-11-03	2016-03-02	1 hour for every 30 hours worked	>=10 employees: 40 hours per year <10 employees: 24 hours per year
Los Angeles, CA	Los Angeles	2016-06-01	2016-07-01	1 hour for every 30 hours worked	48 hours per year
San Diego, CA	San Diego	2016-06-07	2016-07-11	1 hour for every 30 hours worked	80 hours per year
Montgomery, MD	Montgomery	2015-06-24	2016-10-01	1 hour for every 30 hours worked	>=5 employees: 56 hours per year <5 employees: 32 hours per year
Spokane, WA	Spokane	2016-01-26	2017-01-01	1 hour for every 30 hours worked	40 hours per year
Morristown, NJ	Morris	2016-09-13	2017-01-11	1 hour for every 30 hours worked	>=10 employees: 40 hours per year <10 employees: 24 hours per year
Minneapolis, MN	Hennepin	2016-05-27	2017-07-01	>5 employees: 1 hour for every 30 hours worked <=5 employees: 1 hour of unpaid sick leave for every 30 hours worked	48 hours per year
Chicago, IL	Cook	2016-06-22	2017-07-01	1 hour for every 40 hours worked	40 hours per year
St. Paul, MN	Ramsey	2016-09-07	2018-01-01	1 hour for every 30 hours worked	48 hours per year

Note: Similar mandates have been enacted in other cities within the county, including Bloomfield, East Orange, Irvington, and Montclair in Essex County; Passaic and Paterson in Passaic County; Berkeley and Emeryville in Alameda County; Plainfield in Union County; and Santa Monica in Los Angeles County

Appendix 2: Variable Definitions
This table reports the variable definitions.

Variable name	*Definition*
Household characteristics	
Household stock market participation	=1 if the household holds stocks, stock mutual funds, or investment trusts
Family size	The number of persons in the household
House ownership	=1 if the home is owned by any household member
Has a mortgage	=1 if the household has a mortgage
Log (Total household wealth)	The natural logarithm of the household wealth that includes the value of home equity
Log (Total household income)	The natural logarithm of the household total income
Log (Mortgage balance)	The natural logarithm of the remaining value of the mortgage
Log (Mortgage monthly payment)	The natural logarithm of the monthly payment of the mortgage
Log (Mortgage interest rate)	The natural logarithm of the interest rate of the mortgage
Log (Remaining mortgage terms)	The natural logarithm of the remaining years of the mortgage
Log (Housing wealth)	The natural logarithm of the value of home equity
Log (Non-housing wealth)	The natural logarithm of the household wealth that excludes the value of home equity
Income growth	The natural logarithm of income in current survey wave minus that in the prior survey year
Housing wealth growth	The natural logarithm of housing wealth in current survey wave minus that in the prior survey year
Non-housing wealth growth	The natural logarithm of non-housing wealth in current survey wave minus that in the prior survey year
Log (Total expenditure)	The natural logarithm of the household total expenditure
Log (Expenditure on health care)	The natural logarithm of the household health care expenditure
Log (Subsistence-level expenditure)	The natural logarithm of the household food, clothing, telephone, and transportation expenditures
Log (Discretionary expenditure)	The natural logarithm of the household vacations and entertainment expenditures
Loan dummy	=1 if the household owns any form of student loans, medical bills, legal bills, loans from relatives, or other debts
Log (Loan value)	The natural logarithm of the value of student loans, medical bills, legal bills, loans from relatives, and other debts
Credit card debt dummy	=1 if the household owns credit card debt
Log (Credit card debt)	The natural logarithm of the value of credit card debt
Household head characteristics	
Age	The age of the household head
College	=1 if the household head attended college
Married	=1 if the household head is married or permanently cohabiting
Non-White	=1 if the race of household head is Non-White
Hispanic	=1 if the ethnicity of household head is Hispanic
Poor physical health	=1 if the household head's self-reported health is poor
Poor mental health	=1 if the household head's self-reported Kessler Psychological Distress Scale (K6) score if higher than 13 (which indicates a probability of serious mental illness)
Plan to work for longer	=1 if the household head’s planned retirement age in the current survey wave is higher than that in the prior survey wave.
Decline in life satisfaction	=1 if the household head’s self-reported life satisfaction worsens to “not satisfied at all” in the current survey wave compared with the prior survey wave
Days missed work due to self-illness	The number of days that the household head misses work due to self-illness
Days missed work due to other family members	The number of days that the household head misses work due to taking care of other family members
Days missed work due to vacation	The number of days that the household head misses work due to a vacation
State or county characteristics	
PSL	=1 if the household's county has an effective paid sick leave mandate on the survey date
PFL	=1 if the household's state has an effective paid family leave mandate on the survey date
ACA	=1 if the household's state provides the Affordable Care Act on the survey date

UI benefits	The generosity of unemployment insurance benefits in a state, measured as the natural logarithm of the maximum number of weeks times the maximum weekly benefit amount
GDP growth	The growth of the annual current GDP in a state. Source: BEA
Income growth	The growth of the annual personal income in a state. Source: BEA
Log (Social insurance)	The natural logarithm of the annual contributions for government social insurance in a state. Source: BEA
Log (Employment)	The natural logarithm of the annual total employment in a state. Source: BEA
Unemployment rate	Unemployment rate in a state. Source: BLS
Democrat governor	=1 if the household's state has a democrat governor

Appendix 3: Do workers take extra days off following the enactment of PSL mandates?

This table reports regressions that estimate the effect of the enactment of PSL mandates on the number of days off that workers take each year. All dependent variables are based on the number of days that the household's head misses work: *Days missed work due to self-illness* (Column (1)), *Days missed work due to other family members* (Column (2)), and *Days missed work due to vacation* (Column (3)). The independent variable of interest is *PSL*, a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. The control variables are identical to Column (3) of Table 2 and are collapsed for brevity. Standard errors in Panel B are clustered at the household-level and are reported in parentheses. Variables are defined in Appendix 2. *p <.1; **p <.05; ***p <.01.

Dependent variable:	*Days missed work due to self-illness*	*Days missed work due to other family members*	*Days missed work due to vacation*
	(1)	(2)	(3)
PSL	2.2933**	0.2214	2.3518
	(1.1217)	(0.2388)	(1.5210)
Control variables	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes
Household FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
N	20,996	20,997	20,998
R2	0.349	0.401	0.434

Appendix 4: The moderating effects of households' financial obligations

This table reports regressions that estimate the effect of the enactment of PSL mandates on household stock market participation. The dependent variable is *Household stock market participation*, a dummy variable that equals one if the household holds stocks, stock mutual funds, or investment trusts, and zero otherwise. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. *Has a mortgage* is a dummy variable that equals one if the household has a mortgage. *Log (Mortgage balance)* is the natural logarithm of the remaining value of the mortgage. *Log (Mortgage monthly payment)* is the natural logarithm of the monthly payment of the mortgage. *Log (Mortgage interest rate)* is the natural logarithm of the interest rate of the mortgage. *Log (Remaining mortgage terms)* is the natural logarithm of the remaining years of the mortgage. Control variables are collapsed for brevity and identical to those in Column (3) of Table 2. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Dependent variable:	*Household stock market participation*				
	(1)	(2)	(3)		
PSL	0.0400***	0.0408***	0.0401***	0.0371***	0.0366***
	(0.0135)	(0.0135)	(0.0135)	(0.0136)	(0.0135)
*PSL*Has a mortgage*	-0.0535**				
	(0.0238)				
*PSL*Log (Mortgage balance)*		-0.0044**			
		(0.0020)			
PSL Log (Mortgage monthly payment)*			-0.0070**		
			(0.0033)		
PSL Log (Mortgage interest rate)*				-0.0337**	
				(0.0149)	
PSL Log (Remaining mortgage terms)*					-0.0217***
					(0.0079)
Has a mortgage	0.0464	0.0411	0.0468	0.0483	0.0052
	(0.0667)	(0.0667)	(0.0782)	(0.0798)	(0.0651)
Log (Mortgage balance)	-0.0050	-0.0046	-0.0069	-0.0021	0.0014
	(0.0062)	(0.0062)	(0.0065)	(0.0067)	(0.0071)
Log (Mortgage monthly payment)			0.0032		
			(0.0116)		
Log (Mortgage interest rate)				-0.0090	
				(0.0209)	
Log (Remaining mortgage terms)					-0.0050
					(0.0113)
Control variables	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
N	20,998	20,998	20,941	20,420	20,805
R2	0.698	0.698	0.699	0.704	0.704

Appendix 5: Other robustness tests

This table reports robustness tests on our baseline estimation results. The dependent variable is *Household stock market participation*, a dummy variable that equals one if the household holds stocks, stock mutual funds, or investment trusts, and zero otherwise. *PSL* is a dummy variable that equals one if the county where the household resides has an effective local or state paid sick leave mandate on the survey date, and zero otherwise. Column (1) excludes households that move to other counties during the sample period. Column (2) excludes households in locations that already have an effective paid sick leave mandate before 2009. Columns (3) to (6) exclude the households in the large treated states of California, Washington, Oregon, and all three states, respectively. Column (7) uses only the state-level PSL mandates. Column (8) uses unweighted variables from the PSID. Column (9) considers the size of the firm that employs the household head when defining the *PSL* dummy. Column (10) uses a one-to-one propensity score matched sample of households in treated counties to those in untreated counties. Column (11) uses a one-to-one propensity score matching of households in treated counties to those in untreated counties to be located in coastal states. Column (12) controls for a dummy that equals one if any members of the household have health insurance. Column (13) controls for industry-year fixed-effects. Column (14) controls for county-specific time trends. Column (15) uses *Fraction of risky assets* as a dependent variable. *Fraction of risky assets* is defined as the ratio of the value of risky assets (i.e., stocks, stock mutual funds, or investment trusts) to total financial assets. Variables are defined in Appendix 2. Standard errors are clustered at the household-level and are reported in parentheses. *p <.1; **p <.05; ***p <.01.

Robustness tests

Dependent variable:	*Household stock market participation*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PSL	0.0421***	0.0380***	0.0517***	0.0347**	0.0393***	0.0528***	0.0421***
	(0.0141)	(0.0135)	(0.0168)	(0.0139)	(0.0139)	(0.0191)	(0.0147)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	19,339	20,836	19,016	20,576	20,610	18,224	20,998
R2	0.699	0.698	0.697	0.700	0.698	0.700	0.698

(cont.)

Dependent variable:	*Household stock market participation*						
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
PSL	0.0239**	0.0417***	0.0790*	0.1150***	0.0377***	0.0318**	0.0575***
	(0.0107)	(0.0143)	(0.0417)	(0.0369)	(0.0135)	(0.0136)	(0.0174)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	20,998	20,998	2,275	2,362	20,961	19,915	20,998
R2	0.690	0.698	0.759	0.763	0.698	0.703	0.725

(cont.)

Dependent variable:	*Fraction of risky assets*
	(15)
PSL	0.0195*
	(0.0115)
Control variables	Yes
Survey year FE	Yes
Survey month FE	Yes
Household FE	Yes
County FE	Yes
N	19,791
R2	0.624