Financial Education, Employee Financial Stress, and Employer Productivity

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Abstract: Personal financial concerns impact employees' level of financial stress and make it harder for them to be productive at work. Financial education can potentially alleviate these challenges and allow employees to apply their cognitive capacity towards work. We document increased total factor productivity and labor productivity for employers headquartered in states that have adopted financial education mandates relative to employers headquartered in states without such mandates. We use survey data to provide corroborating evidence of financial education mandates reducing employee financial stress. These findings deepen our understanding of the impact of financial education on employee financial stress as well as employer productivity.

Keywords: financial education; employee financial stress; employer productivity
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"Few organizations can afford to have employees check out mentally and emotionally and not be engaged on the job due to financial distractions. When employees are not at the top of their game, they are neither productive nor innovative."-Dina Pyron & R. Lynn Pettus (Ernst & Young 2019)

1. Introduction

Financial stress is a key challenge for adults, surpassing stressors from work and family. Financial stress not only affects adults at home but can spill over to the workplace. According to the PwC Employee Financial Wellness Survey (2023), 57% of full-time employees say that finances are the top cause of stress in their lives. Moreover, 56% of financially stressed employees spend three hours or more per week at work dealing with or thinking about their personal finances.

The accounting profession has taken a lead role in raising awareness about the importance of financial education in alleviating these challenges. The American Institute of CPAs works with lawmakers to advocate for financial education (AICPA 2024). The AICPA maintains a resource center to equip CPAs with the tools to promote financial education through volunteer efforts at schools. State CPA societies organize volunteer efforts, advocate for legislation mandating high school financial education, and are involved in program design for the courses. For example, in response to California mandating financial education in high school in 2024, the California Society of CPAs (2024) noted, "California is set to add personal financial wellness education as a high school graduation requirement, and we're thrilled that CalCPA played a role in making this happen! This includes advocating to enhance personal finance knowledge. We're eager to collaborate with California leaders and financial education stakeholders to implement this critical new policy."

Executives and policymakers echo these calls for financial education. Doug McMillion, CEO of Walmart, notes (2022), "Financial education could help millions of families. And the added benefit—less stressed-out citizens and workers—translates into more societal health." The Federal Reserve Bank of St. Louis (2022) notes, "Managing personal finances is an important life skill that, used properly, can result in a more comfortable life and avoid much grief."

Some states have mandated a high school personal finance course, which has led to objectively better household financial decisions (Kaiser et al. 2022) and subjectively greater self-perceptions of financial well-being (Burke et al. 2024). We examine whether these household improvements spill over to the workplace. Extant research posits that general education (Becker 1962; Call et al. 2017; Beck et al. 2018) and specific coursework (Lusardi and Mitchell 2014; Abernathy et al. 2023) shape employee human capital and employer outcomes. We predict that financial education reduces employee financial stress and thereby improves employer productivity.

This prediction is not without tension. A fierce debate exists about whether high school is an opportune time to provide financial education. On the one hand, Lusardi (2013) notes, "The reason high school is a powerful place to teach financial knowledge is a simple one: Young people need to understand how to make wise financial decisions before—not after—they are faced with life-changing decisions". Consistent with short-term effectiveness, Stoddard and Urban (2020) document improved student loan outcomes a year or two after high school graduation. On the other hand, Fernbach (2024) notes, "If you take a class in high school which gives you specific information ... the idea that you would still have mastery of that in a way that would help you make better financial decisions a couple years down the line is definitely debatable." Consistent with doubts about long-term effectiveness (Leana 2019), Lusardi and Mitchell (2014) posit that the impact of financial education decays over time, as some knowledge gained becomes obsolete as new financial products develop. Brown et al. (2016) find evidence of decay for the impact of financial education on auto and credit card debt outcomes between ages 22 to 27. This raises doubts about whether and to what extent high school financial education will decrease employee financial stress and spur employer productivity over the longer term (i.e., several years after high school).

We follow the tradition in the economics literature on the effects of schooling and focus

on total factor productivity (TFP) (Black and Lynch 1996; Moretti 2004). TFP captures a firm's efficiency in converting inputs into outputs (i.e., capital and labor into revenues) (İmrohoroğlu and Tüzel 2014). TFP is a summary measure of firm productivity and is of interest in the management accounting literature (Banker et al. 1989; 2005; 2021; Callen et al. 2005; Berger et al. 2024).[1]

An empirical challenge we face is that financial education and productivity may be jointly determined by local economic conditions and unobservable time-varying factors. To overcome this challenge, we exploit the staggered adoption of 31 state laws that mandate a personal finance course for high school students in their junior or senior year. The typical coursework covers topics related to management accounting (e.g., budgeting), financial accounting (e.g., personal balance sheets), taxation (e.g., income and payroll taxes), as well as household finance (e.g., borrowing, saving, and investing).[2] Importantly, the decision by U.S. states to mandate high school financial education coursework is largely due to concerns over young adults' financial capability and is expected to be orthogonal to employer productivity. Further, prior work carefully documents that financial education mandates are not endogenous to other state policy changes nor economic conditions (Stoddard and Urban 2020; Mangrum 2022), a result we validate again for our sample.

Our empirical analyses employ a sample of 7,165 U.S. firms from 1988 to 2019. Our difference-in-differences (DID) tests compare the post-mandate change in the productivity of firms headquartered in states with mandates ("treated firms") with that of firms headquartered in states without mandates ("control firms"). We include firm and census region-by-year fixed effects as well as a host of control variables in our model. We find that after financial education mandates

[1] Profitability specifically captures rents to capital owners, whereas productivity captures value created by the firm more generally, with value gains flowing to various stakeholders, including employees, customers, and shareholders.
[2] The accounting literature describes this as "personal accounting" and/or "household accounting" (Fagerberg 1954). Fennema and Koonce (2011) highlight that, "Individuals and households record, summarize, analyze, and report the results of transactions and other financial events for reasons similar to those that motivate organizations to use accounting: to keep track of where their money is going, and to keep spending under control.".

are adopted in a state, treated firms experience an on average increase in productivity of 1.7%.

We observe no differences in productivity between treated and control firms in the five years leading up to the mandates. We observe increases in productivity for treated firms five years after mandates take effect (but not in years 1 to 4). The post-treatment trends are in line with public firm employees entering the workforce a few years after high school (Call et al. 2017; Beck et al. 2018). Our dynamic analysis is consistent with Che and Zhang (2008), who find greater college access for high school students has large effects on firm-level TFP, but only after five years. While younger employees represent a proportion of all firm employees, human capital acquisition by these employees through schooling can and does have sizeable effects on employer productivity.

Not all financial education mandates are expected to be effective in spurring productivity. Lusardi and Mitchell (2023) note, "financial education is most effective when there is a rigorous curriculum, a specific course devoted to personal finance, and trained teachers." We decompose mandates into two subtypes: rigorous (non-rigorous) mandates involve (do not involve) a standalone course, specific content requirements, teacher training, or curriculum support (Urban et al. 2020). We document evidence that rigorous mandates increase productivity while non-rigorous mandates do not impact productivity, underscoring the importance of program design.

We assess whether our results are sensitive to alternative measures of productivity. We focus on a measure often used in the management accounting literature, labor productivity, which comprises of output produced per employee (Banker et al. 2021). Berger et al. (2024) note that TFP and labor productivity measures are strongly correlated, suggesting they capture overlapping constructs. We find that after financial education mandates are adopted, treated firms experience an average increase in labor productivity of 2.2%. We find similar results for a simpler measure of labor productivity - revenue per employee (Caskey and Ozel 2017). We also find similar TFP

results for a data envelopment analysis based firm efficiency measure (Demerjian et al. 2012).

To shed light on the private returns to schooling (i.e., whether employees reap some of the benefits of financial education), we examine the effects of financial education mandates on annual employee income. We examine state residents that are a) 18-24 years old, b) employed, and c) have graduated high school. We use a DID framework. Following Madsen (2015), we use wage, salary, and other income data from the Integrated Public Use Microdata Series dataset (IPUMS-USA). We find that after financial education mandates are adopted, treated employees experience an average increase in annual income of 1.0%, consistent with some private returns to schooling.

We provide corroborating evidence of financial stress being a mechanism through which financial education affects employer productivity. Mechanism tests of mental health outcomes are new to financial economics research and prior work uses either a longitudinal (Hu et al. 2024) or a cross-sectional (Gao et al. 2020) survey approach. We improve upon this approach by examining three surveys, one longitudinal and two cross-sectional. We examine University of Michigan's Panel Study of Income Dynamics (PSID) survey and show that employees' well-being improves after mandates. We also examine a Center for Disease Control (CDC) health survey and show that employees' level of anxiety, worry, and sadness improves after mandates. These specific outcomes are precisely how employee financial stress impacts productivity (Kaur et al. 2024). We take our analysis of the CDC survey a step further and show that employees' level of stress about having enough money to pay for housing improves after mandates. Third, we examine a Consumer Financial Protection Bureau (CFPB) financial wellbeing survey. We find that employees' financial wellbeing improves after mandates. In all models, we control for employee income, suggesting increased income per se is not driving our findings. The economic magnitude of the financial stress improvements ranges from 6% to 13% across the surveys, suggesting sizeable improvements.

In cross-sectional tests, we find that our TFP results are concentrated among local firms reliant on the local labor supply (Call et al. 2017; Beck et al. 2018). We also find that our results are concentrated among younger firms reliant on younger employees (Ouimet and Zarutskie 2014).

We also analyze whether our results are sensitive to the biases of DID research designs using two-way fixed effects models with staggered treatment timing (Baker et al. 2022). We first estimate our model for each of the three waves of multiple state adoption in the same year and compare treated firms in each wave to control firms in never treated states. Here, there is no variation in treatment timing that could potentially bias the estimator (Mangrum 2022). Next, we use an alternative estimator that imputes counterfactuals and compares treatment effects using only untreated observations (Borusyak et al. 2024). We find similar results in these specifications.

We find no reliable evidence that state economic (e.g., GDP, unemployment, household income), investment (e.g., health, education, and R&D spending), or population (e.g., vote share and density) conditions in the prior five years predict mandate adoption. We find few concurrent changes to other high school graduation requirements and find similar results after controlling for these changes. We conduct a brief case study of Texas's 2007 adoption, finding it was prompted by bi-partisan political support and was championed by the Texas Society of CPAs (TSCPA). No additional spending was allocated to the course, and it represented Texas's first course addition in 20 years. TSCPA advocated for financial education by testifying at a state congressional hearing and supported its implementation by facilitating school district use of AICPA curriculum material.

Our findings contribute to the interdisciplinary literature on the impact of employees' financial concerns on workplace productivity (Meuris and Leana 2018; Bernstein et al. 2021; Aslan 2022; Kaur et al. 2024). Our study suggests financial education mandates that target young adults in a developed economy decrease financial stress and spur employer productivity. Mandates

are not without costs (Willis 2011) and may have different effects in developing economies (Carpena et al. 2019). We are not able to speak to the social welfare effects of mandates, only to mandates' effects on employer productivity. Nonetheless, documenting a benefit to employers establishes that financial education mandates can have real effects beyond the household context. These real effects are longer term in nature, which extends prior research on shorter term effects.

Our study contributes to the human capital literature in accounting.[3] Employees' general education level, and specific exposure to information technology courses, improves employers' auditing and financial reporting outcomes (Call et al. 2017; Beck et al. 2018; Abernathy et al. 2023). However, evidence that employee education impacts management accounting outcomes, such as employer productivity, is scant. We provide such evidence, in line with Call et al. (2017)'s call for future research, "employing a high-quality workforce may be associated with other benefits for the firm, beyond the benefits we document, such as more efficient and profitable investments."[4]

Our study helps bridge the gap between research and education (Madsen 2015; Clor-Porell et al. 2024). The AAA, AICPA, state CPA societies, and Big 4 firms promote financial education to a broad set of stakeholders, including undergraduate accounting students (Rakow 2019; EYARC 2024). Accounting research on financial education emphasizes investors and borrowers within capital markets (Lawrence 2013; Li et al. 2024; Chen et al. 2024; Rennekamp et al. 2024). Our study on financial education emphasizes employees and employers within labor markets. Our evidence suggests that both employers and employees reap some benefits from financial education.

[3] This literature examines accounting education or CPA licensing of the audit committee (McDaniel et al. 2002), executives (Hoitash et al. 2016), controllers (Vafeas 2009), internal auditors (Ege 2015), and investors (Lawrence 2013; Krische 2019). As such, its emphasis is undergraduate, graduate, and post-graduate, not high school, education.
[4] Shroff et al. (2014) describe total factor productivity as a "measure of investment efficiency", noting that TFP "measures how efficiently and intensely the inputs (capital and labor) are utilized in the production process."

2. Hypothesis development

Financial concerns can create a scarcity of cognitive resources and push people into a state of tunneling (i.e., neglecting activities outside of the "financial concerns tunnel"). This can result in financial stress for employees (i.e., anxiety, worry, sadness) and reduced productivity at work (Kaur et al. 2024). For example, Bernstein et al. (2021) and Aslan (2022) find that financial stress arising from negative housing wealth shocks decrease the productivity of inventor employees and sell-side equity analysts. Meuris and Leana (2018) find that employee financial stress in the U.S. trucking industry result in greater preventable workplace accidents for rank-and-file employees. While there is a consensus in the literature that employee financial stress impairs workplace productivity, there is a paucity of evidence on possible interventions that may alleviate this effect.

We propose that financial education interventions, through personal finance coursework in the formative years prior to entering the workforce, decreases employee financial stress and spurs employer productivity over the longer term.[5] We posit two non-mutually exclusive mechanisms (i.e., objective and subjective financial wellbeing) which may explain, at least in part, why financial education mandates decrease employee financial stress and spur employer productivity.[6]

First, by encouraging objectively better personal financial decisions that help individuals avoid personal financial concerns, financial education mandates can reduce employee financial stress and thereby allow employees to apply their cognitive capacity towards productive activities. Sergeyev et al. (2024) model the economics of financial stress and show that sophisticated households internalize the fact that undertaking financial decisions to avoid future stress (i.e.,

[5] Formative life periods (i.e., high school) have lasting effects on individuals' behavior within firms (Hanlon et al. 2022). In 2013, Annamaria Lusardi, Professor of Economics and Accountancy, advocated for financial education in high school during a U.S. Senate subcommittee hearing. See: https://www.annamarialusardi.com/policy-work/

[6] Financial education in the workplace can also reduce financial stress (Bannon et al. 2014). The Big 4 accounting firms sell, as part of tax or consulting services, employee financial wellness programs to clients (Deloitte 2024). Program effectiveness is challenging to assess as employees self-select into participation (Lusardi and Mitchell 2014).

savings) can free up cognitive capacity for productive work and increase their earnings. Consistent with this line of reasoning, extant research documents that financial education mandates encourage individuals to make better financial decisions as reflected in lower levels of debt, credit delinquencies, and payday borrowing, as well as higher levels of saving and rainy-day account ownership (Brown et al. 2016; Stoddard and Urban 2020; Mangrum 2022; Burke et al. 2024).

Second, by encouraging greater subjective self-perceptions of financial wellbeing that help individuals avoid personal financial concerns, financial education mandates can reduce financial stress, allowing employees to apply their cognitive capacity towards productive activities.[7] The behavioral economics literature maintains that subjective beliefs are an important mechanism through which financial capability and decision-making are connected (Anderson et al. 2017).[8] Consistent with this line of reasoning, Burke et al. (2024) document that financial education mandates encourage individuals to have greater subjective perceptions of financial well-being.

We expect financial education mandates to reduce employee financial stress, thereby facilitating productivity at work for employers.[9] Our hypothesis can be summarized as follows:

H1: Financial education decreases employee financial stress and increases employer productivity.

Extant research finds employer productivity improvements of between 10%-20% when all employees have an additional year of schooling (Black and Lynch 1996), with quantitative courses such as a high school math course, accounting for a large portion of these gains (Goodman 2019). 10%-25% productivity improvements are also observed when a subset of employees have greater access to a college education (Che and Zhang 2018; Kong et al. 2022). An open question that we

[7] Chen et al. (2024) similarly explores the impact of digital lending on objective and subjective financial well-being.
[8] Management accounting research studies interventions to increase worker productivity (Hales and Williamson 2010).
[9] Employees with financial education could indirectly impact the productivity of other employees without financial education. For example, Narayan (2024) finds evidence that financial concerns increase workplace harassment and discrimination of co-workers, suggesting that mental health challenges of one worker can spillover to other workers. This evidence is in line with schooling generating externalities that accrue to co-workers and others (Beck et al. 2018).

shed light on is the economic magnitude of employer productivity improvements arising from financial education mandates, in light of coursework being quantitative and affecting a subset of employees. Knowledge decay over time raises the distinct possibility of immaterial longer term effects. Given the fierce debate on efficacy (Lusardi and Mitchell 2014; Kaiser et al. 2022), or lack thereof (Willis 2011; Fernandes et al. 2024), of financial education, it is an empirical question whether and to what extent financial education mandates, as implemented in U.S. high schools, have an economically meaningful impact on employee financial stress and employer productivity.

3. Institutional background

The first wave of financial education mandates occurred from the 1950s to 1980s. Fourteen states imposed the requirement that high school students take a consumer education course with some personal finance content, although only one of these states required a rigorous inclusion of financial education content (i.e., Illinois, effective for students graduating in 1970). During this period, states rarely mandated coursework on even general economics topics so specialized topics such as personal finance rarely received rigorous attention. While Bernheim et al. (2001) find evidence that this first wave of mandates improved financial behavior among young adults, Cole et al (2016) demonstrate that early and later adopting states were trending differently and results are not robust to including state and year fixed effects. Thus, the literature views this first wave of mandates to be ineffective as states did not require schools to offer well-defined coursework and only required that schools do something remotely related to financial education (Burke et al. 2024). Often, this involved a representative from a bank giving a one-day lecture (Kaiser et al. 2022).

The second wave of financial education mandates began in the 1990s with three states (i.e., New Hampshire, New York, and Michigan) incorporating financial education material into an economics or career skills course. This continued in the 2000s with eleven states adopting mandates. This wave has continued to present day with Kentucky being the most recent state to

pass mandates that will be effective for students graduating in 2024. Research examining this more recent wave finds consistent evidence that mandating financial education in high school leads to positive outcomes for young adults (Kaiser et al. 2022).[10] We use the state high school financial education database to identify which states have adopted mandates and in which year these are effective for graduating high school students. In Figure 1, we present this data for states that adopt mandates during our sample period (i.e., 1988 to 2019) but not for states that adopt mandates prior to (i.e., Illinois 1970) or after (i.e., West Virginia 2020, Nevada 2022, and Kentucky 2024).

The primary purpose of requiring financial education coursework in high school is to increase financial capability among young adults and in turn reduce personal financial concerns. The underlying assumption is that financial education has the potential to provide young adults with financial knowledge that will equip them with the skills to develop future positive financial behaviors (Rakow 2019). Course topics include budgeting, income and payroll taxes, personal balance sheets emphasizing assets and liabilities, mortgages, auto and student loans, stock market, checking and savings accounts, insurance, credit scores and cards (Stoddard and Urban 2020).

Some states require a standalone course in financial education (e.g., Utah) while others integrate coursework with social studies (e.g., Oregon), mathematics (e.g., South Carolina), and/or economics (e.g., Arizona). States also vary in the number of course credits of financial education required, with New Jersey (2.5 credits) requiring the highest number of course credits.

Georgia, which implemented a mandate for students graduating in 2007, has one of the most rigorous financial education mandates. It requires students to take a one-semester integrated (with economics) financial education course that covers complex topics such as mortgages, credit scores, interest rates, and risk. The state provides formal training and certification to its teachers

[10] The third, and most rigorous, wave with a one-semester standalone course is currently underway (NGPF 2022).

and provides sample evaluations to teachers. On the other end of the spectrum, Wyoming, which implemented a mandate for students graduating in 2002, simply requires financial education topics to be covered in a social studies course but does not have specific content requirements, teacher training, or curriculum support. Both states are considered as having mandates in our tests.[11]

Using Texas as an example of one state mandate, Texas House Bill 492, signed into law on September 1, 2005, requires financial education for high school graduates starting in the 2006-2007 school year. The bill was introduced, in part, due to the high number of young adults filing for personal bankruptcy (Woolley et al. 2005). Championed by Beverly Woolley, a Republican Representative, and receiving bipartisan support from the state legislature, the financial education mandate aimed to help prevent young people from making poor financial decisions. The bill received widespread support from consumer groups, financial institutions, teachers, and the Texas Society of CPAs. This legislation integrated a financial education component within the existing economics curriculum, equipping Texas students with access to essential financial information at no additional cost to the state. It was the first time in over 20 years that the Texas legislature had mandated that a specific high school course be taught. The Texas Society of CPAs advocated for the legislation by testifying at a state congressional hearing and supporting its implementation by facilitating school district use of AICPA curriculum material (Accounting Today, 2005).

To assess if financial education mandates impact employer productivity, outcomes in states without mandates should serve as an appropriate counterfactual for states with mandates. Prior research presents compelling evidence that state adoption of mandates is not correlated with economic (e.g., unemployment rate, poverty rate, GDP growth) or political (e.g., political affiliation of state government) factors in the state (Stoddard and Urban 2020; Urban et al. 2020;

[11] We follow a conservative classification approach as any state requiring personal finance coursework is considered to have adopted a mandate. In Sections 5.4 and 5.11, we examine the sensitivity of our results to different definitions.

Mangrum 2022). These findings suggest that states with and without mandates were otherwise similar. We validate these findings in Section 5.10 of the manuscript. In addition, Stoddard and Urban (2020) explore whether concurrent changes to high school graduation requirements were put in place at the same time as mandates.[12] They identify four major policy changes to high school education requirements: number of Carnegie units required for graduation, number and level of math courses required for graduation, as well as a requirement that students take college placement exams. These changes occurred prior to the year 2000 and had minimal overlap with financial education mandates (Goodman 2019; Stoddard and Urban 2020). We validate in Section 5.11 of the manuscript that our findings are robust to including only mandates adopted after the year 2000. Finally, Stoddard and Urban (2020) and Mangrum (2022) explore whether mandates cause a shift in college selection decisions. Young adults in states with mandates were no more likely to attend college, a private university, a university with lower tuition, an in-state university, or a four-year (versus two-year) university, inconsistent with mandates impacting college selection decisions.[13]

As mandates do not focus on the corporate sector, employer effects are likely unintended. It is unlikely firms lobbied for such changes. Financial firms, who would have the highest incentive to lobby given that mandates may increase demand for their services, are specifically excluded from our sample (Rennekamp et al. 2024). Furthermore, productive firms could implement workplace financial education without relying on state mandates, which would bias towards zero.

4. Sample, variable measurement, and research design

4.1. Data and sample

[12] In untabulated analysis, we control for increases in state high school computer science, english, economics, math, science, social science courses. We find productivity increases for financial education and math (Brown et al. 2016).
[13] Mandates can include career research (i.e., requirements, expected income, and risks of careers). It is possible that career research promotes improved employee career selection, which in turn drives productivity increases (Leiby and Madsen 2017). In an untabulated analysis, we find productivity increases even for financial education mandates that do not involve career research, inconsistent with better career selection being the sole drive of productivity increases.

We obtain financial data from CRSP/Compustat and TFP data from İmrohoroğlu and Tüzel (2014). Our sample period starts from 1988, which is five years before the first state (i.e., New Hampshire 1993) adopted a financial education mandate for graduating high school students. Our sample period ends in 2019 to avoid confounding events arising from the global pandemic. Our initial sample consists of all firms in the merged CRSP/Compustat database. We exclude firms headquartered outside the U.S.[14] We exclude firms in the financial industry and the utility industry because of the differences in regulation for these industries. Our final sample consists of 68,771 firm-year observations (7,165 unique firms) for which all financial data is available.

4.2. Variable measurement

4.2.1 Identifying treated and control firms

To identify firm-year observations impacted by adoption of financial education mandates, we construct an indicator variable *PF*. *PF* equals 1 if a firm's headquarter state adopts a mandate in the adoption year and all the subsequent years. *PF* equals 0 in years prior to a firm's headquarter state adopting a mandate or in all years if a firm's headquarter state does not adopt a mandate.[15,16]

4.2.2 Measuring productivity

Total factor productivity (TFP) represents the overall efficiency with which firms use capital and labor in production. TFP is studied in the management accounting literature (Banker et al. 1989; Banker et al. 2005; Callen et al. 2005; Banker et al. 2021; Berger et al. 2024; Liu and Zhang 2024) and is also studied in the economics literature on the effects of schooling (Black and Lynch 1996; Moretti 2004; Che and Zhang 2018; Kong et al. 2022). Schooling can be a valuable investment in human capital for individuals as it increases the returns to hours worked (Becker

[14] We obtain data on historical state of firms' headquarters from: https://www3.nd.edu/~mcdonald/
[15]We obtain state-year financial education adoption data from: https://www.carlyurban.com/home/financial-education
[16] Illinois adopted a mandate in 1970. PF equals 1 (in all years) if a firm's headquarter state is Illinois.

1962). As more educated workers can promote the development and adoption of superior technologies, schooling can generate externalities for the productivity of all factors of production.

TFP captures variation in production output (i.e., revenue) beyond what can be explained by observable production inputs (i.e., capital and labor). As such, it is well suited to capture the impact of financial concerns on outcomes reflecting employee productivity that are difficult to observe and quantify (e.g., absenteeism, creativity, presenteeism, satisfaction, and turnover).

We measure TFP following the approach of İmrohoroğlu and Tüzel (2014). This approach begins with the Cobb-Douglas production function:

$$Y_{it} = A_{it} * K_{it}^{\beta_K} * L_{it}^{B_L}$$

where Y_{it} represents value added for firm i in period t, K_{it} is capital, L_{it} is labor, and A_{it} is productivity. Taking the natural logarithm of both sides, we have

$$\ln(Y_{it}) = \beta_K * \ln(K_{it}) + \beta_L * \ln(L_{it}) + \ln(A_{it}).$$

The parameters can then be estimated based on the following specification:

$$y_{it} = \beta_0 + \beta_k * k_{it} + \beta_l * l_{it} + \omega_{it} + \epsilon_{it}$$

where y, k, and l represent the logs of value added, capital, and labor, respectively, and $\ln(A_{it})$ is replaced by $\beta_0 + \omega_{it} + \epsilon_{it}$. The regression intercept, β_0, is the mean productivity across firm-years; ω_{it} and ϵ_{it} capture the difference from the mean and thus the firm-specific productivity. ω_{it} is the portion of that difference that is observable to the firm and ϵ_{it} is the error term. We follow İmrohoroğlu and Tüzel (2014) and recover ω_{it} using the Olley and Pakes (1996) estimator. Using hats to represent estimates, our firm-year (log) TFP is then measured as follows:

$$TFP_{it} = y_{it} - \hat{\beta}_0 - \hat{\beta}_l l_{kt} - \hat{\beta}_k k_{it}$$

This approach also uses industry-specific time dummies in the estimation and thus the resulting firm TFP measures are free of the effects of industry or aggregate TFP in any given year.

To calculate firm-level *TFP*, we use data from Compustat, supplemented with the price index for gross domestic product (GDP) as deflator for value added, the price index for private fixed investment as deflator for investment and capital (both from the Bureau of Economic Analysis), and the national average wage index from the Social Security Administration.

Value added (Y_{it}) is net sales (Compustat SALE) minus materials, deflated by the GDP price deflator. Materials is total expenses minus labor expenses. Total expenses is net sales minus operating income before depreciation and amortization (OIBDP). Labor expenses is estimated by multiplying the number of employees (EMP) by average wages from the Social Security Administration.[17] Capital stock (K_{it}) is gross property, plant, and equipment (PPEGT), deflated by the price deflator for investment. Labor stock (L_{it}) is the number of employees.

This approach results in a firm-year measure of the efficiency with which capital and labor are deployed to produce value added. Higher values of *TFP* indicate greater productivity.

4.2.3. Control variables

We control for several firm and industry characteristics that can affect productivity. Firm-level controls include firm size (*FSIZE)*, cash holdings (*CASH)*, leverage (*LEV)*, capital expenditures (*CAPEX)*, return on assets (*ROA*), market-to-book (*MTB)*, R&D (*RD)*, fixed assets (*FIXED)*, and stock price non-synchronicity *(PSI*). Productive firms are usually larger, profitable firms with available cash, higher growth opportunities, leverage and stock price non-synchronicity, and lower asset tangibility and R&D spending. Industry-level controls include industry concentration (*HI)* and squared industry concentration (*HISQ)* to account for nonlinear effects of product market competition. To minimize the effect of outliers, we winsorize all continuous variables at the 1st and 99th percentiles. Detailed variable definitions are provided in Appendix A.

[17] Firm-level wage data is not required to be disclosed by most non-regulated firms and is thus unavailable (Hall 2016).

4.3. Research Design

We test our hypothesis using the following OLS model. All the subscripts are suppressed, as all independent variables are measured in year *t*. *TFP* is measured in year t+1.

$$TFP = b_0 + b_1PF + b_2CASH + b_3FSIZE + b_4LEV + b_5CAPEX + b_6ROA + b_7MTB + b_8FIXED + b_9HI + b_{10}HISQ + b_{11}RD + b_{12}PSI + FIRMFE + REGION\text{-}BY\text{-}YEARFE \quad (1)$$

The coefficient on *PF* indicates how employer productivity of treated firms changes after mandates relative to control firms. Our hypothesis predicts a positive coefficient on *PF*.

To estimate generalized DID regressions, our models need to include a set of group and time fixed effects. We include firm and region-by-year fixed effects. Firm fixed effects allow us to control for time-invariant differences in productivity across firms. Region-by-year fixed effects allow us to control for mandates that may be correlated across census regions and time. These fixed effects lead to b_1 being estimated as the within-state differences before and after the mandate change as opposed to similar before–after differences in states that did not experience a change during the same period. Despite having a directional prediction, we utilize two-tailed t-tests. Given our treatment is defined at the headquarter state level, we cluster standard errors by state.

5. Results

5.1. Descriptive statistics

The descriptive statistics are presented in Table 1. 25% of firm-year observations in the sample have *PF* = 1. *TFP* has substantial variation, with a median of -0.317 and an interquartile range of -0.543 to -0.090. Sample firms have a median cash to total assets ratio of 8.08%. Sample firms are moderately leveraged with a median book leverage ratio of 20.88% and fixed assets (capital expenditures) account for 21.34% (4.01%) of total assets. Sample firms perform well with a median *ROA* of 12.32% and have moderate growth opportunities with a median *MTB* of 1.43%.

5.2. Primary analysis

Table 2, column (1) reports the results of our primary analysis without control variables or fixed effects. Column (2) reports the regression results of model (1). The coefficient on *PF* is 0.0206 and 0.0168 in columns (1) and (2), significant at the 10 and 5 percent levels. This evidence suggests that employers headquartered in states that adopt financial education mandates experience an increase in productivity of approximately 1.7%.[18] The results for control variables are in line with expectations. Larger firms, firms with greater profitability, growth, leverage, and capital expenditures, as well as lower asset tangibility and R&D expenditures are more productive.[19]

The magnitude of the increase in productivity of 1.7% is economically significant and in line with our expectations.[20] Quantitative coursework in high school, such as mathematics, has been demonstrated to have sizeable effects, reflecting up to 50% of the returns to one additional year of school (Goodman 2019). As prior research finds that the impact of high school financial education coursework is analogous to that of mathematics coursework (Brown et al. 2016), we also expect sizeable effects for financial education mandates. Black and Lynch (1996) estimate that one additional *year* of school among all firm employees leads to a 10% increase in productivity. Thus, a 1.7% increase in response to a financial education *course* is economically meaningful. However, not all firm employees are treated with financial education in our setting, rendering comparisons to Black and Lynch (1996), where all employees are treated, challenging.[21]

As an alternative benchmark, we use Liu and Zhang (2024), who document a 3.1% increase in productivity after the adoption of digital financial reporting (i.e., SEC EDGAR). As

[18] When a coefficient in a log-linear regression is small, it approximately corresponds to the percentage change in the dependent variable if the independent variable increases by 1. The coefficient on *PF* is 0.0168 in column (2).
[19] For brevity, the coefficients on controls are not reported in subsequent tables where *TFP* is the dependent variable.
[20] The economic significance of changes in firm-level TFP are typically measured in percentage, not dollar, terms in the literature (Che and Zhang 2018; Bennett et al. 2020; Kong et al. 2022 Berger et al. 2024; Liu and Zhang 2024).
[21] Che and Zhang (2018) find a 25% increase in productivity for firms in human capital-intensive industries after a Chinese educational reform that increased supply of college graduates. Kong et al. (2022) examine the same setting and find a 10% increase in productivity for firms in skilled industries. As with our setting, the productivity increases come from younger, not all, employees and arise five years after the educational reform impacting high school students

with our study, Liu and Zhang (2024) also employ a DID research design in a context where only a subset of firms' stakeholders (i.e., investors in Liu and Zhang (2024)) are impacted by the adoption of EDGAR. Some investors do not use financial information, others use alternative sources of financial information (e.g., company websites or Yahoo Finance), while others already had access to SEC reference rooms in New York City, Chicago, and/or Washington, D.C prior to the event. We view this as being analogous to our setting where not all employees receive financial education. Our estimated economic magnitude of 1.7% represents approximately 50% of the estimated economic magnitude of 3.1% in Liu and Zhang (2024), an arguably comparable setting.

5.3. Dynamic analysis

The validity of a DID estimation depends on the parallel trends assumption: absent the mandates, treated firms' productivity would have evolved in the same way as that of control firms. This assumption is inherently untestable because we do not observe the treated firms in the absence of treatment. However, we can obtain suggestive evidence by examining pre-treatment trends. In Table 3, we employ an event-time specification where *TFP* is measured contemporaneously and the key variables of interest are PF^{-5+}, PF^{-4}, PF^{-3}, PF^{-2}, PF^{-1}, PF^{+1}, PF^{+2}, PF^{+3}, PF^{+4}, and PF^{5+}, which are equal to one if the firm is headquartered in a state that will adopt a mandate in five or more years, four years, three years, two years, in one year, as well as whether the state adopted one year ago, two years ago, three years ago, four years ago, and five or more years ago, and zero otherwise. We find that none of the coefficients on *PF* in the pre-treatment period are statistically significant, suggesting productivity was no different among treated and control firms in this period. The coefficients on PF^{-5+} and PF^{-1} are positive and economically insignificant while the coefficients on PF^{-4}, PF^{-3}, and PF^{-2} are negative and economically insignificant. Thus, there is no clear pre-treatment trend with respect to economic significance nor the sign of the coefficients.

We also examine post-treatment trends. We expect the effects of mandates on productivity to coincide with the timing of employees' entry into the labor market. Financial education courses are typically taken in the junior or senior year of high school and employees of public firms enter the workforce, on average, two years after high school with an associate degree, with many others entering after earning a 4-year degree (Call et al. 2017; Beck et al. 2018). Thus, we do not expect the effects of mandates on productivity to manifest immediately but rather manifest over the longer term (Che and Zhang 2018). We find that the coefficients on PF^{+1} and PF^{+2} are economically and statistically insignificant. We find that the coefficients on PF^{+3} and PF^{+4} are economically significant, in that the magnitudes of 1.6% are similar to our baseline estimate of 1.7% in Table 2, but not statistically significant.[22] The coefficient on PF^{5+} is 0.0261 and is economically and statistically significant, consistent with longer term effects of mandate adoption.

5.4. Variation in financial education mandates analysis

Thirteen states adopted rigorous mandates and eighteen states adopted non-rigorous mandates (Urban et al. 2020). Rigorous (non-rigorous) mandates include those with (without) a standalone course, specific content requirements, teacher training, and/or curriculum support. We decompose financial education mandates into rigorous and non-rigorous. *PFR* equals 1 if a firm's headquarter state adopts a rigorous mandate in the adoption year and all the subsequent years. *PFNR* equals 1 if a firm's headquarter state adopts a non-rigorous mandate in the adoption year and all the subsequent years. Table 4 reports the results of our analysis of treatment strength heterogeneity. The coefficient on *PFR* is 0.0236 in column (1), significant at the 5 percent level.

[22] Che and Zhang (2018) also document economically significant, but statistically insignificant, coefficients three years after initiation of a college expansion program, noting "graduates of three-year college programs also contribute to firm productivity". In our U.S. context, we expect graduates of two-year college programs to contribute to TFP.

In contrast, the coefficient on *PFNR* is insignificant in column (1).[23] This evidence underscores the importance of careful program design and implementation of financial education mandates.

5.5. Alternative measurement of productivity

Following the tradition in the management accounting literature, we buttress our TFP findings by examining a partial factor productivity measure-labor productivity (Banker et al. 2021; Berger et al. 2024). We follow Berger et al. (2024) to measure labor productivity (*LP*) as output produced per employee for firm *i* in year *t*. Gross profit proxies for value added.

$$LP_{it} = y_{it} - l_{it}$$

Consistent with Berger et al. (2024), we find that *TFP* and *LP* are positively correlated with each other (ρ = 0.70), suggesting that they capture overlapping aspects of productivity. In Table 5, we find that the coefficient on *PF* is 0.0221 in column (1), significant at the 1 percent level. This evidence suggests that employers headquartered in states that adopt financial education mandates experience an increase in labor productivity of approximately 2.2%.

We also use a simpler accounting-based measure of labor productivity – revenue per employee (*REVENUE_EMP*). This measure captures the value added (revenue) per employee and has been extensively used in the accounting literature (Caskey and Ozel 2017). The coefficient on *PF* is 0.0240 in column (2), significant at the 10 percent level.

Finally, we employ a measure of firm efficiency (*FIRM_EFF*). Specifically, Demerjian et al. (2012) develop a measure of relative firm productivity within its industry using data envelopment analysis (DEA). This process forms an efficient frontier by measuring both the amount and mix of resources that are used to generate revenue. Firms that are on the efficient

[23] Urban et al. (2020) conduct a detailed review of all mandates up to 2014. We follow the methodology of Urban et al. (2020) and extend the rigorous/non-rigorous classification to the end of our sample period (i.e., 2019). In untabulated analysis, we find similar inferences if we use the original Urban et al. (2020) classification and timeline.

frontier receive a *FIRM_EFF* score of 1. As firms move away from the efficient frontier, the score approaches 0. The coefficient on *PF* is 0.0064 in column (3), significant at the 10 percent level.[24]

5.6. Employee income analysis

Following the tradition in the accounting and economics literature, we buttress our productivity findings for employers by examining the private returns to schooling for employees (Madsen 2015; Goodman 2019; Dai et al. 2024). We use a DID research design to examine the impact of financial education on annual employee income. As our focus is on younger employees, we restrict our sample to state residents that are a) 18-24-years-old, b) employed, and c) graduated high school.[25] We assume that young adults graduate from high school at age 18 and use variation in *PF* to examine the impact of financial education mandates on annual employee income. We control for demographic characteristics of individuals (i.e., race and gender) as well as population and economic characteristics of the state of residence (i.e., population density, vote share for the democratic party, unemployment rate, median household income, and GDP growth). We include state of residence and census region-by-year fixed effects in our models (Gao et al. 2020).

The Integrated Public Use Microdata Series dataset (IPUMS-USA) is a collection of microdata of distinct individuals and includes a rich set of variables relating to individuals' demographic characteristics, education, family relationships, and income. To capture annual employee income, we use three related variables: (1) wage and salary income (Dai et al. 2024), (2)

[24] In untabulated analysis, we employ *TFP2* computed following the estimator of Ackerberg et al. (2015) as opposed to Olley and Pakes (1996). The former is free of the functional dependance problem and provides an improved estimation of the production function (Liu and Zhang 2024). The coefficient on *PF* is 0.0155 significant at the 5 percent level. We also follow Schoar (2002) to estimate the log-linear Cobb–Douglas production function for firms in each industry-year group. The dependent variable is the natural logarithm of net income (in millions), and the independent variables include the natural logarithm of PPE (in millions), the natural logarithm of the number of employees (in thousands), and the natural logarithm of 1 plus R&D expenditures (in millions). We assign this measure (*TFP3*) to all firms in the same industry-year. The coefficient on *PF* is 0.0219, significant at the 5 percent level.

[25] Brown et al. (2016) use consumer credit data from 1998-2012 and report that 94% of 18–24-year-olds reside in the same state from which they graduate high school. We use tax return data to capture migration from 2012 -2021 and find that 88% of these young adults are non-migrants, where a migrant is a filer with a year -over -year state change. Dai et al. (2024) similarly use IPUMS data to examine employee income for younger employees (i.e., 18-25).

total personal earned income (Goodman 2009), and (3) total personal income (Madsen 2015). To facilitate comparability across time, all variables are reflected in thousands of 1999 dollars (Goodman 2019). Our estimation sample consists of 1,905,796 individual-year observations from 2001 to 2019 residing in 50 States and D.C. Following Madsen (2015), all means are calculated after weighting observations by their survey sampling probability weights, and all regressions weight observations using their survey sampling probability weight. We estimate a survey-weighted ordinary least squares regression in Table 6. The coefficient on *PF* is 0.1933 and 0.1941 in columns (1) and (2), both significant at the 10 percent level. These coefficient estimates suggest that mandates increase annual employee income by 0.97% and 0.91%, relative to their mean values of 19.851 and 21.272. The coefficient on *PF* is 0.1773 in column (3), statistically insignificant.

We aim for consistency across analyses and subsequent survey data sources we use do not have the same granular data as IPUMS-USA (e.g., we have the exact age of individuals in IPUMS-USA whereas we only have broad age ranges, 18-24, in subsequent surveys). Thus, the analysis reported in Table 6 does not take full advantage of the IPUMS-USA data. In untabulated analysis, we use the same control variables as in Madsen (2015) (e.g., breaking down race into separate variables for Black, Hispanic, Asian, American Indian, Miscellaneous). The coefficient on *PF* is 0.2340, 0.2606, and 0.2484, in columns (1) (2) and (3), all significant at the 5 percent level. These estimates reflect economic magnitudes ranging from 0.94% to 1.20%, relative to their respective mean values. Our collective evidence suggests that employees located in states that adopt financial education mandates experience private returns (i.e., income increases) of approximately 1%.[26]

5.7. Mechanism through which financial education impacts productivity – financial stress

[26] Goodman (2009) reports an economic magnitude of 2.9% for high school math, suggesting that the private returns (i.e., income increases) for financial education coursework are relatively comparable to those of math coursework.

Personal financial concerns result in financial stress, including anxiety, worry, and sadness (Kaur et al. 2024). We use a DID research design to examine the impact of financial education on financial stress. As our focus is on younger employees, we restrict our sample to state residents that are a) 18-24-years-old, b) employed, and c) graduated high school. We assume that young adults graduate from high school at age 18 and use variation in *PF* to examine the impact of financial education mandates on financial stress. We control for demographic characteristics of individuals (i.e., race and gender). We also control for individual income to ensure that our findings are incremental to any enhanced income from financial education. We control for population and economic characteristics of the state of residence (i.e., population density, vote share for the democratic party, unemployment rate, median household income, and GDP growth). We include state of residence and census region-by-year fixed effects in our models (Gao et al. 2020).

The CDC conducts annual health-related telephone surveys of approximately 400,000 U.S. residents across all 50 states and D.C. through the Behavioral Risk Factor Surveillance System (BRFSS).[27]Two questions relevant to the level of anxiety, worry, and sadness are asked: (1) During the past 30 days, for about how many days have you felt worried, tense, or anxious? (2) During the past 30 days, for about how many days have you felt sad, blue or depressed? These questions are asked as part of the optional modules, meaning that states make an election to ask the questions. Although the modules are optional, CDC standards require that if they are used, they must be used without modification. Our estimation sample consists of 10,505 and 10,496 employees from 1995 to 2017 residing in 34 states who responded to (1) and (2). To capture stress, we use *MH_ANXWORRY* and *MH_SAD*, measured as the number of days an employee reported as having

[27] The CDC changed its sampling-weighting methodology for BRFSS part way through our sample period, limiting our ability to achieve comparability. Consequently, the analyses we report using this survey data use unweighted methods, following prior research (Gao et al. 2020). The analyses should be interpreted with this limitation in mind.

felt worried, tense, or anxious and the number of days an employee reported as having felt sad, blue, or depressed. We estimate negative binomial regressions in Panel A of Table 7 as the dependent variables are counts. The coefficient on *PF* is -0.3017 and -0.2545 in columns (1) and (2), both significant at the 1 percent level. These coefficient estimates suggest that mandates decrease the number of days an employee reported as having felt worried, tense, or anxious by 5.8% and sad, blue, or depressed by 7.8%, relative to their mean values of 5.21 and 3.25.

Our next analysis aims to examine financial stress specifically using the CDC survey data. A relevant question is asked: How often in the past 12 months would you say you were worried or stressed about having enough money to pay your rent/mortgage? Our estimation sample consists of 3,563 employees from 2009 to 2015 residing in 25 states. To capture financial stress, we use *MH_FINSHEL*. We estimate an ordered logistic regression in column (3) of Panel A of Table 7 as the dependent variable is ordinal (i.e., always, usually, sometimes, rarely, never). The coefficient on *PF* is 0.5268, significant at the 1 percent level. This coefficient estimate suggests that mandates decrease the frequency an employee reported being worried or stressed about having enough money to pay for shelter by 12.9%, relative to the mean value of 4.08.

A limitation of the CDC survey is that it focuses on general stress outcomes as opposed to specifically financial stress.[28] In 2018 and 2021, the CFPB conducted financial well-being surveys of approximately 25,000 U.S. residents across all 50 states and D.C. through the National Financial Capability Study (NFCS). Five questions relevant to financial well-being are asked: (1) I am just getting by financially (2) I am convinced the money I have or will save won't last (3) Because of my money situation, I feel like I will never have the things I want in life (4) My finances control my life (5) I have money leftover at the end of the month. Questions (1) to (3) (i.e., describes me completely, very well, somewhat, very little, not at all) and (4) to (5) (i.e., never, rarely, sometimes, often, always) have ordinal values. Our estimation sample

[28] An additional limitation of the CDC study is its cross-sectional design, which limits the ability to observe changes over time. To address this, we use the Panel Study of Income Dynamics (PSID), a biennial longitudinal survey of U.S. families conducted by the University of Michigan. Using data from the Transition into Adulthood Study (2009–2019, biennially, n=2,456), we measure social and emotional well-being as the average response to eight questions about life satisfaction and societal outlook. A survey-weighted ordered logistic regression (untabulated) reveals that mandates significantly increase well-being by 7.1% (coefficient = 0.3053, $p<0.10$), relative to the mean value of 4.30.

consists of 2,142 employees from 2018 and 2021 residing in 50 States and D.C. who responded to (1) to (5). To capture financial stress, we use *FWB*, which is measured by taking the responses from (1) to (5) and scoring them using item response theory (IRT), where each item response has unique weights and contributes differently to the score (Burke et al. 2024). IRT uses a standard normal distribution. We estimate survey-weighted OLS regressions in column (1) of Panel B of Table 7 as the *FWB* score ranges from 20 to 90. The coefficient on *PF* is 4.8468 in column (1), significant at the 5 percent level. This coefficient estimate suggests that mandates increase financial well-being by 9.4%, relative to the mean value of 51.58. When we disaggregate questions (1) to (5) and estimate our models using survey-weighted ordered logistic regressions, the coefficient estimates suggest that mandates decrease the frequency an employee reported that "I have money left over at the end of the month." The results for Q5 are reported in column (2) of Panel B of Table 7. The coefficient estimates for Q1-4 are not statistically significant. For brevity, these results are not tabulated.[29]

5.8. Cross-sectional analysis

We expect the treatment effect to be concentrated among firms employing local employees, for which the local labor supply is critical. Following Garcia and Norli (2012), sample firms with below median geographical dispersion (i.e., operations in 5 or fewer states as per firms' 10-K reports) are firms with low geographical dispersion. Sample firms with above median geographical dispersion are firms with high geographical dispersion. In column (1) of Table 8, the coefficient on *PF* is 0.0370 for low geographical dispersion firms, significant at the 1 percent level. In contrast, the coefficient on *PF* is insignificant in column (2) for high dispersion firms. [30]

Next, we expect the treatment effect to be concentrated among firms employing younger employees, for which financial education mandates are more impactful. Younger firms have a greater proportion of younger employees and younger employee new hires (Ouimet and Zarutskie

[29] In untabulated analysis, we conduct falsification tests where we replicate our analysis for state residents that are a) 18-24-years-old, b) employed, and c) did not complete high school. It is unlikely that employees in this group received the treatment (i.e., personal finance course in their junior or senior year of high school). For this sample, we find no evidence that mandates decrease employee financial stress as measured by the CDC, CFPB, and PSID survey data.

[30] Financial education could encourage local investors to supply financing to local firms, an alternative channel by which mandates could impact productivity (Van Rooij et al. 2011). Local investors prefer local firms that are highly leveraged (Coval and Moskowitz 1999). In untabulated analysis, we find that the coefficient on *PF* is insignificant for high leverage firms. The coefficient on *PF* is 0.0305 for low leverage firms, significant at the 1 percent level.

2014).[31] Sample firms with below median firm age (i.e., 15 years) are firms with younger employees (*YE*). Sample firms with above median firm age are firms with older employees (*OE*). In column (3) of Table 8, the coefficient on *PF* is 0.0282 for younger employee firms, significant at the 1 percent level. In contrast, the coefficient on *PF* is insignificant for older employee firms.

5.9. Potential bias arising from staggered treatment timing and heterogeneous treatment effects

With staggered treatment timing and heterogeneous treatment effects, two-way fixed effects (TWFE) estimation can introduce a "forbidden comparisons" problem by comparing later treated firms to earlier treated firms as a control, yielding biased estimates of treatment effects. This occurs because the estimator is a weighted average of each possible two-by-two DID comparison combination, some of which use earlier treated firms as controls for later treated firms.

As our identification strategy exploits staggered adoption of financial education mandates, it is advisable to interpret estimates obtained from standard TWFE DID regressions with caution. If later adopting states learn from earlier adopting states and produce more effective mandates, staggered treatment timing will bias the estimator towards zero as our design pools earlier and later adopting states. If the impact of mandates grows over time, heterogenous treatment effects will bias the estimator towards zero as our design will use some earlier treated firms as controls for later treated firms. We conduct two analyses to test whether our results are sensitive to these biases.

First, following Mangrum (2022), we exploit the fact that multiple states adopt mandates in waves during the same year (i.e., 3 states in 2007, 2 states in 2009, and 3 states in 2011).[32] In Panel A of Table 9, we re-estimate model (1) but only include treated firms headquartered in each successive wave of adoption and firms headquartered in never treated states. In this specification,

[31] Ouimet and Zarutskie (2014) demonstrate that the youngest (oldest) public firms have approximately 50% (33%) of employees 34 years of age or younger, noting that younger employees are "especially critical to young firms".
[32] Our results are driven by rigorous mandates over the long-term We exclude 2013/2014 waves as all mandates are non-rigorous. We exclude 2015 and subsequent waves as sufficient (i.e., long-run) post-treatment data is unavailable.

there is no variation in treatment timing that could potentially bias the estimator and our research design explicitly allows for treatment to grow over time. The coefficient on *PF* is 0.0878, 0.0547, 0.0521 in columns (1) through (3), significant at the 10 percent, 1 percent, and 5 percent levels.

Next, In Panel B of Table 9, we re-estimate model (1) using an alternative estimator that imputes counterfactuals and compares treatment effects using only untreated observations (Borusyak et al. 2024). The alternative estimator estimates fixed effects among the untreated observations only, imputes untreated outcomes for treated observations, and then forms treatment-effect estimates as weighted averages over the differences between actual and imputed outcomes. Using this alternative estimator, the coefficient on *PF* is 0.0191, significant at the 5 percent level.

5.10. State economic, population, political, and government spending conditions

Our identification strategy relies on the assumption that the enactment of mandates is exogenous with respect to productivity. Given that we observe similar pre-treatment trends, reverse causality (e.g., productive firms lobby for the passage of mandates) is unlikely to drive our results. However, it is still possible that state economic or political conditions impact our tests.

The mandates could be endogenous in that the mandates are passed when states' economic conditions warrant these mandates and these economic conditions also affect productivity. We examine whether state governments' decisions to adopt mandates are related to state economic conditions, population characteristics, or government spending. To test this possibility, we estimate a state-level linear probability model with mandate adoption as the dependent variable. *PF_ADOPT* is an indicator variable that equals 1 if a state has adopted a mandate in year *t*, and 0 otherwise. Given lengthy delays in passing and implementing laws, we include several measures from year *t-5* in the model, including the natural logarithm of population density (*POP)*, vote share for the democratic party (*VOTE*), unemployment rate (*UR*), median household income (*HI*), GDP growth *(GDP*), per capita government health spending *(HEALTH*), per capita government

education spending *(EDUCATION*), and per capita government R&D spending *(RD_STATE*).[33] The model also includes state and region-by-year fixed effects.[34]As shown in Table 10, no state characteristics reliably have predictive power for explaining adoption of mandates.[35]

5.11. Alternative definitions of treated and control states

In untabulated tests, we address potential confounds in financial education mandates by re-estimating our baseline model using alternative definitions of treated and control states. Excluding states with pre-2000 mandates, adoption confounds (e.g., Hurricane Katrina in Louisiana, pilot programs in Kansas, New Jersey, and Oregon), non-rigorous mandates, and states with locally controlled mandates yields PF coefficients ranging from 0.0235 to 0.0350, all significant at least at the 5 percent level. These results suggest state-level confounds do not drive the findings, with economic impacts increasing to 2–3% compared to the baseline of 1.7% in these specifications.

6. Conclusion

Financial stress challenges make it more difficult for employees to be productive at work. Financial education has been proposed as a remedy. We predict that financial education mandates decrease the financial stress of employees and spur employer productivity. We test our hypothesis using a difference-in-differences research design that exploits staggered adoption of laws mandating personal finance coursework for graduating high school students across 31 U.S. states from 1988 to 2019. We find that for employers headquartered in affected states, total factor productivity (TFP) increases by 1.7% after mandate adoption. This increase is only observed five years after mandate adoption, when treated students are likely to have entered the workforce.

[33] As GDP growth can have negative values, we define this variable in its raw form without taking natural logarithms.
[34] State-level government education and health spending data is only available from 1993 onwards, GDP data is only available from 1998 onwards, and R&D spending data is only available from 2006 onwards. Therefore, we estimate four separate models in Table 10: for the full sample period 1988-2019, for 1993-2019, 1998-2019 and 2006-2019.
[35] In untabulated analysis, we repeat this for years *t-1*, *t-2*, *t-3*, *t-4*, and *t-6*. Again, no single state characteristic (e.g., vote share) is reliably significant across all four columns and rarely is any characteristic significant in one column.

Furthermore, mandates that include rigorous curriculum standards, a standalone financial education course, and/or teacher training drive the increase. We also find evidence that mandates specifically increase employee-related outcomes (i.e. labor productivity and employee wages).

Our cross-sectional tests indicate that the increase in TFP is concentrated among local firms and firms that employ younger employees. We use longitudinal and cross-sectional survey data to provide evidence of decreased employee financial stress being a mechanism by which financial education mandates affect employer productivity. Our study contributes to the literature on human capital, financial education, and productivity. Our findings should interest the AAA, AICPA, state CPA societies, Big 4 accounting firms, executives, and policymakers, all of whom have suggested that financial education can play a key role in reducing financial stress and spurring productivity.

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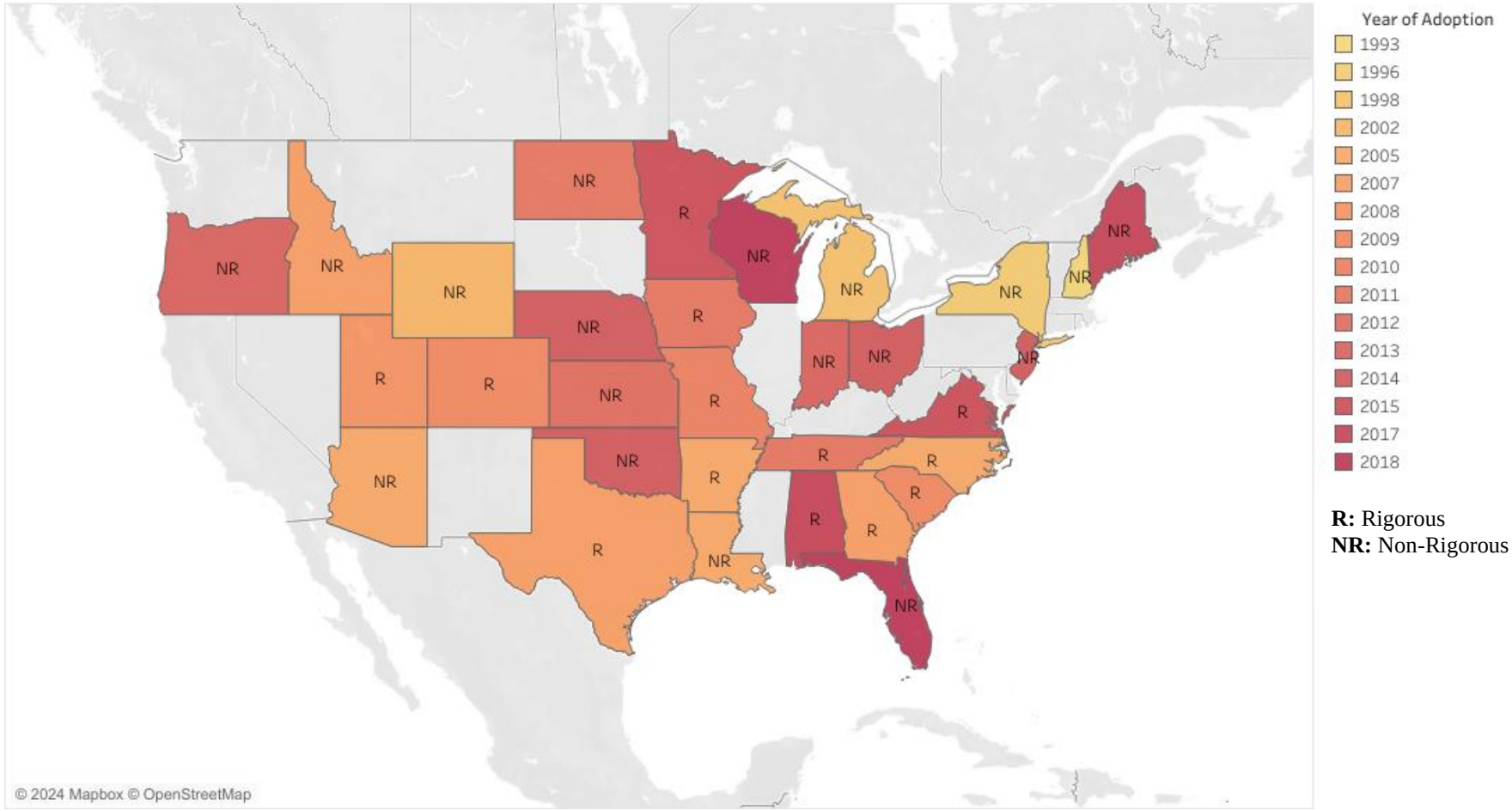
Appendix A
Variable descriptions.

Variable	Description (Compustat data items in parentheses)
Dependent Variables	
TFP	Natural logarithm of the İmrohoroğlu and Tüzel (2014) total factor productivity measure (TFP). TFP is a measure of the residual from a firm's value added regressed on capital, investment, and employee count. Residuals are calculated based on annual regressions and 3-digit SIC industry code.
LP	Natural logarithm of gross profit (gp) minus number of employees (emp).
WAGE&SALARY INCOME	Natural logarithm of the total pre-tax wage and salary income (e.g. wages, salaries, commissions, cash bonuses, tips, etc.). IPUMS-USA variable name: INCWAGE.
PERSONAL EARNED INCOME	Natural logarithm of the total personal earned income – total income earned from wages or a person's business or farm. IPUMS-USA variable name: INCEARN.
PERSONAL INCOME	Natural logarithm of the total personal income – total pre-tax income or losses from all sources (e.g., wages and salary, business income, farm income, welfare, retirement, support payments, investment income). IPUMS-USA variable name: INCTOT.
TFP2	Natural logarithm of the Liu and Zhang (2024) total factor productivity measure. This measure is computed following the estimator of Ackerberg et al. (2015).
TFP3	Natural logarithm of the Schoar (2002) total factor productivity measure. Estimated based on log-linear Cobb-Douglas production function for firms in each industry-year group. This reflects an industry-year measure of TFP.
FIRM_EFF	Demerjian et al. (2012) measure of a firm's efficiency in generating revenue using an industry adjusted mix of non-human capital and human capital inputs.
REV_EMP	Natural logarithm of sales revenue (sale) divided by the book value of assets (at).
MH_ANXWORRY	Number of days an employee reported as having felt worried, tense, or anxious in the past 30 days.
MH_SAD	Number of days an employee reported as having felt sad, blue, or depressed in the past 30 days.
MH_FINSHEL	The frequency (i.e., always, usually, sometimes, rarely, never) an employee reported being worried or stressed about having enough money to pay rent/mortgage in the past 12 months.
FWB	A measure of employee financial wellbeing constructed from responses to five questions and aggregated using item response theory (Burke et al. 2024).
Independent Variables	
PF	*PF* equals 1 if a firm's headquarter state adopts a financial education mandate in the adoption year and all the subsequent years, and 0 otherwise.
PFR	*PFR* equals 1 if a firm's headquarter state adopts a rigorous financial education mandate in the adoption year and all the subsequent years (Urban et al. 2020), and 0 otherwise.
PFNR	*PFNR* equals 1 if a firm's headquarter state adopts a non-rigorous financial education mandate in the adoption year and all the subsequent years (Urban et al. 2020), and 0 otherwise.
CASH	Cash and short-term investments (che) divided by the book value of assets (at).
FSIZE	Natural logarithm of the book value of assets (at).
LEV	Book value of long-term debt (dltt) plus debt in current liabilities (dlc) divided by the book value of assets (at).
CAPEX	Capital expenditures (capx) divided by the book value of assets (at).

ROA	Operating income before depreciation (oibdp) divided by the book value of assets (at).
MTB	Market value of assets (market value of equity (prcc_f × csho) plus book value of assets (at) minus book value of equity (ceq) minus deferred taxes (txdb)) divided by the book value of assets (at).
FIXED	Property, plant, and equipment (ppent) divided by the book value of assets (at).
HI	Sum of squared sales (sale) based on market shares of all firms in a three-digit SIC industry in a given year.
HISQ	The squared value of *HI.*
RD	Natural logarithm of one plus R&D expenditures (xrd) divided by book value of assets (at). *RD* equals 0 if its value is missing.
PSI	Stock price non-synchronicity measure based on the R^2 from two-stage asset pricing regressions, following the measurement approach of Bennett et al. (2020).
Additional Variables	
GEOGRAPHICALLY CONCENTRATED	Equals 1 if the firm has operations in 5 or fewer states as per 10-K report (Garcia and Norli 2012), and 0 otherwise.
GEOGRAPHICALY DISPERSED	Equals 1 if the firm has operations in more than 5 states as per 10-K report (Garcia and Norli 2012), and 0 otherwise.
YOUNG FIRMS	Equals 1 if firm age is below the sample median value (15 years), and 0 otherwise.
OLD FIRMS	Equals 1 if firm age is above the sample median value (15 years), and 0 otherwise.
GENDER	Equals 1 if the gender of the survey respondent is male, and 0 otherwise.
RACE	Equals 1 if the race of the survey respondent is white, and 0 otherwise.
AGE	Age of the survey respondent.
PI	Natural logarithm of total annual earnings of the survey respondent.
POP	Natural logarithm of total population divided by land area in square miles for the state. The data comes from the Census Bureau for 1980, 1990, 2000, and 2010 census.
VOTE	Natural logarithm of total votes cast for the Democratic Party presidential candidate divided by total votes cast for any presidential candidate for the state. The data comes from the MIT Election Data Lab for presidential elections in 1988, 1992, 1996, 2000, 2004, 2008, 2012, and 2016.
UR	Natural logarithm of annual unemployment rate for the state. The data comes from the Bureau of Labor Statistics, annually for the years 1988-2019.
HI	Natural logarithm of annual median household income for the state. The data comes from the Census Bureau, annually for the years 1988-2019.
GDP	Annual change in gross domestic product (GDP) for the state. The data comes from the Bureau of Economic Analysis, annually for the years 1998-2019.
HEALTH	Natural logarithm of annual per capita state government spending on health. The data comes from the Census Bureau, annually for the years 1993-2019.
EDUCATION	Natural logarithm of annual per capita state government spending on education. The data comes from the Census Bureau, annually for the years 1993-2019.
RD_STATE	Natural logarithm of annual per capita state government spending on R&D. The data comes from the National Center for Science and Engineering Statistics, annually for the years 2006-2019.
PF_ADOPT	Equals 1 if a state with a financial education mandate has adopted a financial education mandate in year *t,* and 0 otherwise.

Figure 1
Financial education mandates.



This figure shows the year when each state adopted state-level financial education mandates requiring all graduating high school students to take personal finance coursework, from 1988 to 2019. The adoption year represents the first calendar year that the financial education mandates applied to a graduating class. The figure also reports whether the mandate is considered rigorous as reflected in a standalone course, specific content requirements, teacher training, and/or curriculum support (Urban et al. 2020).

Table 1
Descriptive statistics.

Variable	*N*	Mean	Std. dev.	Median	10th	90th
TFP	68,771	-0.3311	0.4587	-0.3167	-0.8425	0.1897
PF	68,771	0.2491	0.4325	0.0000	0.0000	1.0000
CASH	68,771	0.1475	0.1679	0.0808	0.0077	0.4006
FSIZE	68,771	6.0471	1.9210	5.9092	3.6204	8.6391
LEV	68,771	0.2405	0.2266	0.2088	0.0000	0.5208
CAPEX	68,771	0.0578	0.0582	0.0401	0.0114	0.1230
ROA	68,771	0.1237	0.0980	0.1232	0.0130	0.2361
MTB	68,771	1.8658	1.5173	1.4277	0.8932	3.2253
FIXED	68,771	0.2739	0.2164	0.2134	0.0504	0.6147
HI	68,771	0.1621	0.1205	0.1312	0.0509	0.3046
HISQ	68,771	0.0408	0.0702	0.0172	0.0026	0.0928
RD	68,771	0.0344	0.0634	0.0000	0.0000	0.1136
PSI	68,771	2.2939	1.8474	2.0896	0.0728	4.8037

This table presents the descriptive statistics of variables in our primary tests. The sample period is 1988-2019. We show the mean, standard deviation, and the 10th, 50th, and 90th percentiles of the variables used in the empirical analyses. All variables are winsorized at the top and bottom 1%. Please refer to Appendix A for variable definitions.

Table 2
Primary analysis: Effects of financial education on total factor productivity.

		TFP		*TFP*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.3362	<0.001	-1.3821	<0.001
PF	+	0.0206	0.055	0.0168	0.018
CASH	+			0.0349	0.168
FSIZE	+			0.1304	<0.001
LEV	+			0.0503	<0.001
CAPEX	+			0.2526	<0.001
ROA	+			3.0485	<0.001
MTB	+			0.0055	0.001
FIXED	-			-0.4548	<0.001
HI	-			-0.0750	0.191
HISQ	+/-			0.1531	0.100
RD	-			-0.8763	<0.001
PSI	+			-0.0009	0.287
Firm Fixed Effects			NO		YES
Region-By-Year Fixed Effects			NO		YES
Adjusted R^2			0.04%		80.87%
N			68,771		68,026

This table presents the effects of financial education mandates on productivity. The dependent variable is *TFP*, the natural logarithm of total factor productivity. The key independent variable is *PF*, an indicator for firm-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 3
Dynamic analysis: Effects of financial education on total factor productivity.

		TFP	
	Predicted Sign	Coefficient	*p*-value
Intercept	+/-	-1.3858	<0.001
PF^{-5+}	+/-	0.0084	0.388
PF^{-4}	+/-	-0.0093	0.380
PF^{-3}	+/-	-0.0091	0.391
PF^{-2}	+/-	-0.0040	0.710
PF^{-1}	+/-	0.0006	0.952
PF^{+1}	+/-	0.0086	0.404
PF^{+2}	+/-	0.0056	0.570
PF^{+3}	+/-	0.0164	0.115
PF^{+4}	+/-	0.0163	0.132
PF^{5+}	+	0.0261	0.010
Controls			YES
Firm Fixed Effects			YES
Region-By-Year Fixed Effects			YES
Adjusted R^2			80.88%
N			68,026

This table presents the effects of financial education mandates on productivity. The dependent variable is *TFP*, the natural logarithm of total factor productivity. The key independent variable are PF^{-5+}, PF^{-4}, PF^{-3}, PF^{-2}, PF^{-1}, PF^{+1}, PF^{+2}, PF^{+3}, PF^{+4}, and PF^{5+} which are equal to one if the firm is headquartered in a state that will adopt a financial education mandates in five or more years, four years, three years, two years, in one year, adopted one year ago, two years ago, three years ago, four years ago, and five or more years ago, and zero otherwise. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 4
Variation in financial education mandates: Rigorous mandates.

		TFP	
	Predicted Sign	Coefficient	*p*-value
Intercept	+/-	-1.3821	<0.001
PFR	+	0.0236	0.020
PFNR	+/-	0.0119	0.197
Controls			YES
Firm Fixed Effects			YES
Region-By-Year Fixed Effects			YES
Adjusted R^2			80.87%
N			68,026

This table presents the effects of financial education mandates on productivity, exploiting variation in the type of mandate. The dependent variable is *TFP*, the natural logarithm of total factor productivity. The key independent variable is *PFR*, an indicator for firm-year observations affected by rigorous state-level financial education mandates. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 5
Alternative measurement of productivity analysis: Effects of financial education on productivity.

		LP		*REV_EMP*		*FIRM_EFF*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-1.3561	<0.001	5.0389	<0.001	0.0021	0.874
PF	+	0.0221	0.008	0.0240	0.088	0.0064	0.052
CASH	+	0.1422	<0.001	0.1031	<0.001	0.0474	<0.001
FSIZE	+	0.1014	<0.001	0.0419	0.001	0.0431	<0.001
LEV	+	0.0703	<0.001	0.0166	0.486	-0.0140	0.002
CAPEX	+	-0.1522	0.001	-0.1015	0.031	0.0345	0.045
ROA	+	3.3249	<0.001	1.0844	<0.001	0.3292	<0.001
MTB	+	-0.0013	0.586	-0.0001	0.932	0.0055	<0.001
FIXED	-	-0.1699	0.026	-0.3376	<0.001	-0.0331	0.005
HI	-	-0.0709	0.248	-0.0858	0.513	0.0217	0.405
HISQ	+/-	0.0846	0.369	0.1117	0.591	-0.0326	0.420
RD	-	-0.9894	<0.001	0.0112	0.875	0.3105	<0.001
PSI	+	0.0012	0.380	0.0014	0.447	0.0003	0.424
Firm Fixed Effects			YES		YES		YES
Region-By-Year Fixed Effects			YES		YES		YES
Adjusted R^2			88.93%		91.98%		73.43%
N			67,772		68,025		67,110

This table presents the effects of financial education mandates on productivity. The dependent variables are *LP*, labor productivity constructed following Berger et al. (2024), *REV_EMP* as per Caskey and Ozel (2017), and *FIRM_EFF* as per Demerjian et al. (2012). The key independent variable is *PF*, an indicator for firm-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 6
Private returns analysis: Effects of financial education on employee income.

		Wage & Salary Income		*Personal Earned Income*		*Personal Income*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-24.4499	<0.001	-25.6711	<0.001	-25.7627	<0.001
PF	+	0.1933	0.067	0.1941	0.074	0.1773	0.1210
GENDER		2.5644	<0.001	2.7228	<0.001	2.6947	<0.001
RACE		0.7625	<0.001	0.8594	<0.001	0.8705	<0.001
AGE		1.8314	<0.001	1.8954	<0.001	1.9080	<0.001
VOTE		-5.7583	<0.001	-6.0369	<0.001	-6.1610	0.001
UR		-0.2233	<0.001	-0.2261	<0.001	-0.2191	<0.001
POP		-0.0112	0.037	-0.0110	0.042	-0.0112	0.051
HI		0.0000	0.220	0.0000	0.161	0.0000	0.162
GDP		-4.0069	0.001	-4.3625	0.001	-3.6290	0.005
Year Fixed Effects			YES		YES		YES
State Fixed Effects			YES		YES		YES
Region-By-Year Fixed Effects			YES		YES		YES
Adjusted R^2			13.75%		14.09%		13.72%
N			1,905,796		1,905,796		1,905,796

This table presents the effects of financial education mandates on employee income based on Integrated Public Use Microdata Series (IPUMS-USA) data on American Community Survey (ACS) from 2001-2019. The dependent variables are (1) total wage & salary income – total pre-tax wage and salary income (e.g. wages, salaries, commissions, cash bonuses, tips), (2) total personal earned income – total income earned from wages or a person's business or farm, and (3) total personal income – total pre-tax income or losses from all sources (e.g., welfare, retirement, support payments, investment income). Each definition of income is additive (i.e., (2) includes (1) and additional sources of income, (3) includes (2) and additional sources of income). The income data for all three dependent variables in thousands of 1999 dollars. The key independent variable is *PF*, an indicator for individual-state-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. Standard errors are clustered by state of residence. P-values are based on two-tailed tests. This table uses survey-weighted ordinary least squares estimation.

Table 7
Mechanism analysis: Effects of financial education on employee financial stress.

Panel A: Effects of financial education on general employee financial stress.							
		MH_ANXWORRY		*MH_SAD*		*MH_FINSHEL*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
PF	-/-/+	-0.3017	<0.001	-0.2545	<0.001	0.5268	<0.001
GENDER		-0.2620	<0.001	-0.2995	<0.001	0.3428	<0.001
RACE		0.1208	0.001	-0.0556	0.171	-0.0008	0.994
PI		-0.0289	<0.001	-0.0829	<0.001	0.2912	<0.001
VOTE		0.5732	0.695	3.7085	<0.001	-35.7702	0.043
UR		-0.1047	0.216	-0.1293	0.022	0.0689	0.587
POP		0.0003	0.493	0.0006	0.034	0.0241	0.054
HI		0.0000	0.234	0.0000	0.599	0.0000	0.461
GDP		-4.6673	0.006	-4.1386	<0.001	-5.1765	0.129
State Fixed Effects			YES		YES		YES
Region-By-Year Fixed Effects			YES		YES		YES
R^2			0.42%		0.69%		5.17%
N			10,505		10,496		3,563

Panel B: Effects of financial education on employee financial wellbeing.

		FWB		"I have money left over at the end of the month"	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	32.8868	0.093		
PF	+	4.8468	0.032	0.3532	0.009
GENDER		3.6990	<0.001	0.1862	0.029
RACE		0.0357	0.957	0.0936	0.256
PI		0.6995	<0.001	0.0724	0.007
VOTE		-45.4049	0.257	0.6620	0.683
UR		1.5088	0.031	0.1251	0.025
POP		-0.0073	<0.001	0.0006	0.001
HI		0.0004	0.027	0.0000	0.617
GDP		-53.0534	0.230	-2.1386	0.376
State Fixed Effects			YES		YES
Region-By-Year Fixed Effects			YES		YES
R^2			5.63%		1.26%
N			2,142		3,662

Panel A presents the effects of financial education mandates on the level of employee financial stress based on the Behavioral Risk Factor Surveillance System (BRFSS) survey from 1995-2017. The dependent variables in Panel A are *MH_ANXWORRY*, the number of days (out of the past 30) an employee reported as having felt worried, tense or anxious, *MH_SAD*, the number of days (out of the past 30) an employee reported as having felt sad, blue, or depressed, and *MH_FINSHEL*, the frequency (i.e., always, usually, sometimes, rarely, never) an employee reported being worried or stressed about having enough money to pay rent/mortgage in the past 12 months. The key independent variable is *PF*, an indicator for individual-state-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. Standard errors are clustered by state of residence. P-values are based on two-tailed tests. Columns 1 and 2 (3) use negative binomial (ordered logistic) estimations. The BRFSS data are weighted for the probability of selection. Due to a change in the BRFSS post-stratification procedures partway through our sample period, we do not further adjust our survey. This is consistent with Gao et al. (2020).

Panel B presents the effects of financial education mandates on the level of employee financial wellbeing based on the National Financial Capability Study (NFCS) survey in 2018 and 2021. The dependent variables in Panel B are *FWB*, a measure of employee financial wellbeing constructed from responses to five questions and aggregated using item response theory, and the disaggregated responses to one question (Burke et al. 2024). Question (5) (i.e., describes me completely, very well, somewhat, very little, not at all) has ordinal values. The key independent variable is *PF*, an indicator for individual-state-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. Standard errors are clustered by state of residence. P-values are based on two-tailed tests. Panel B uses survey-weighted ordinary least squares estimation for the aggregated score and survey-weighted ordered logistic estimations for the disaggregated question.

Table 8
Cross-sectional analysis: The role of geographical concentration and firm age.

		Geographically Concentrated Firms		Geographically Dispersed Firms		Young Firms		Old Firms	
		TFP		*TFP*		*TFP*		*TFP*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-1.5144	<0.001	-1.3062	<0.001	-1.4813	<0.001	-1.4555	<0.001
PF	+	0.0370	<0.001	0.0013	0.878	0.0282	0.007	0.0057	0.451
Controls			YES		YES		YES		YES
Firm Fixed Effects			YES		YES		YES		YES
Region-By-Year Fixed Effects			YES		YES		YES		YES
Adjusted R^2			83.64%		78.18%		83.31%		82.22%
N			28,154		34,320		32,936		33,175

This table presents the effects of financial education mandates on productivity, exploiting cross-sectional variation in geographical concentration and firm age (based on median splits). The dependent variable is *TFP*, the natural logarithm of total factor productivity. The key independent variable is *PF*, an indicator for firm-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 9
Assessing potential biases associated with two-way fixed effects estimators.

Panel A: Effects of financial education on productivity with no variation in timing of treatment.							
		Only 2007 mandates		Only 2009 mandates		Only 2011 mandates	
		TFP		*TFP*		*TFP*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-1.5048	<0.001	-1.4666	<0.001	-1.4508	<0.001
PF	+	0.0878	0.087	0.0547	<0.001	0.0521	0.025
Controls			YES		YES		YES
Firm Fixed Effects			YES		YES		YES
Region-By-Year Fixed Effects			YES		YES		YES
Adjusted R^2			81.76%		82.76%		82.99%
N			31,724		24,965		24,655
Panel B: Effects of financial education on productivity using alternative estimator.							
						TFP	
	Predicted Sign					Coefficient	*p*-value
PF	+					0.0191	0.036
Controls							YES
Firm Fixed Effects							YES
Region-By-Year Fixed Effects							YES
N							61,691

Panel A presents the effects of financial education mandates on productivity, using only mandates that occur in the same year for which we have three successive waves (2007, 2009, and 2011). Panel B presents the effects of financial education mandates on productivity using an alternative estimator based on Borusyak et al. (2024). The alternative estimator estimates fixed effects among the untreated observations only, imputes untreated outcomes for treated observations, and then forms treatment-effect estimates as weighted averages over the differences between actual and imputed outcomes. The dependent variable is *TFP*, the natural logarithm of total factor productivity. The key independent variable is *PF*, an indicator for firm-year observations affected by state-level financial education mandates. Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.

Table 10

Do state economic conditions, population characteristics, government spending confound adoption of financial education mandates?

		PF_ADOPT		*PF_ADOPT*		*PF_ADOPT*		*PF_ADOPT*	
	Predicted Sign	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value	Coefficient	*p*-value
Intercept	+/-	-0.0247	0.972	-0.3663	0.737	0.4339	0.771	0.0377	0.989
POP	+/-	0.0259	0.547	0.0530	0.541	-0.0508	0.631	0.1423	0.514
VOTE	+/-	-0.0107	0.832	0.0406	0.558	0.0594	0.569	0.0865	0.701
UR	+/-	0.0343	0.124	0.0769	0.059	0.0569	0.276	0.0726	0.370
HI	+/-	-0.0112	0.857	0.0032	0.973	-0.0239	0.856	-0.0791	0.746
HEALTH	+/-			-0.0238	0.223	-0.0336	0.160	-0.0710	0.102
EDUCATION	+/-			0.0061	0.926	0.0441	0.616	-0.0094	0.943
GDP	+/-					-0.4170	0.146	0.0182	0.967
RD_STATE	+/-							0.0263	0.229
State Fixed Effects			YES		YES		YES		YES
Region-By-Year Fixed Effects			YES		YES		YES		YES
Adjusted R^2			48.26%		40.26%		34.78%		30.14%
N			1,570		1,088		839		445

This table presents the test on whether state-level economic conditions, population characteristics, and government spending predict a state's adoption of financial education mandates. The dependent variable is *PF_ADOPT*, an indicator variable that equals 1 if a state with a financial education mandate has adopted a mandate in year *t*, and 0 otherwise. We include several measures of state-level economic conditions, population characteristics, and government spending from year *t-5* in the model, including population density (*POP)*, vote share for the democratic party (*VOTE*), unemployment rate (*UR*), median household income (*HI*), GDP growth *(GDP)*, per capita state government health spending *(HEALTH*), per capita state government education spending *(EDUCATION*), and per capita state government R&D spending *(RD*). Please refer to Appendix A for variable definitions. All variables in the regressions are winsorized at the top and bottom 1%. Standard errors are clustered by headquarter state. P-values are based on two-tailed tests.