JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS

doi:10.1017/S0022109024000449

Government Investment Stimulus and Household Balance Sheet Externalities

Abhishek Bhardwaj
Tulane University Freeman School of Business
abhardwaj@tulane.edu

Saptarshi Mukherjee
Northeastern University D'Amore-McKim School of Business
saptarshimukherjee@northeastern.edu (corresponding author)

Abstract

We document spillover effects of government policies promoting capital investment on household financial choices and wealth accumulation. Using individual-level data on employment outcomes and household balance sheets, we find that increase in accelerated depreciation limits increases the layoff probability of routine workers and reduces their stock share of liquid wealth relative to non-routine workers. Background risk due to the policy is mitigated when workers have access to generous unemployment insurance benefits. Finally, we show that such portfolio rebalancing adversely impacts investment returns and the wealth accumulation of routine workers.

I. Introduction

Governments often use tax incentives to stimulate small business investments and job growth. Starting with the Jobs and Growth Tax Reconciliation Act of 2003, federal and state governments in the United States have routinely updated special provisions for accelerated depreciation of capital investment to provide incentives for increasing investment. By shifting the timing of tax deductions, these measures increase the present value of corporate tax benefits. A growing literature focuses on the effects of such policies on investment, employment, and wages.[1] However, limited attention has been paid to the externalities these policies may impose on worker households.

In this paper, we explore the effects of such tax incentive policies on worker households' portfolio choice and wealth accumulation process. Specifically, we use

We thank Viral V. Acharya, Rajesh Aggarwal, Holger M. Mueller, Arpit Gupta, Simone Lenzu, Joseba Martinez, Tiantian Gu, Weiling Liu, Kunchen Zhang, and seminar participants at the 2022 EFA Annual Meeting, 2021 SFA Meeting, and Northeastern University for helpful comments. We would also like to thank Ran Duchin (the editor) and two anonymous referees for numerous helpful comments which significantly improved the paper. All errors are our own.

[1]Using tax return data, Zwick and Mahon (2017) shows that such policies substantially affected investment, especially for small firms. Similar results were also obtained in Ohrn (2016), (2019), and Maffini, Xing, and Devereux (2019), among others. Tuzel and Zhang (2021) finds change in labor composition among affected firms.

the staggered increases in Section 179 depreciation limits in the Internal Revenue Code as the main policy shock in our analysis. Our paper highlights a portfolio rebalancing channel whereby exposed worker households shift liquid financial wealth away from risky instruments like stocks and mutual funds toward safer interest-earning assets and cash. These reallocations have significant and persistent impact on their returns and wealth accumulation process in the medium to long term.

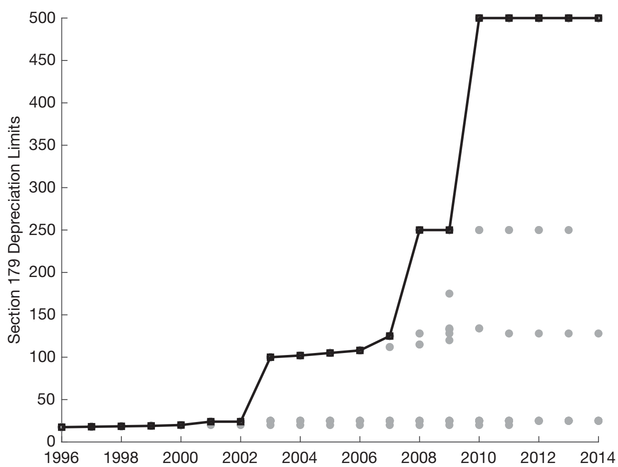
Section 179 of the Internal Revenue Service (IRS) tax code allows for full expensing of qualified capital expenditures in the year of purchase. Under this code, investment in productive equipment and software can be fully depreciated in the first year rather than following the usual schedule as specified in Modified Accelerated Cost Recovery System (MACRS). Importantly, Section 179 imposes phase-out threshold limits on expenditures to attract more small firm participation. During the sample period of 1996–2013 considered in our empirical analysis, the federal Section 179 limits increased from $24,000 to $500,000. Some states adopt the full federal depreciation limits, allowing firms to also claim similar deductions for state taxes. However, many states deviate from the federal guidelines, creating delays in the adoption of new limits. Figure 1 shows the federal and state depreciation thresholds over the sample period. Federally adopted limits are represented by the solid black line, while the gray dots represent those at the state level.[2]

In this paper, we use annual changes in the state-level Section 179 limits to investigate the effect of investment stimulus on household balance sheets of affected workers. Section 179 exemptions promote investments in labor-displacing capital and impact the employment of routine and non-routine workers differently (Tuzel and Zhang (2021)). Our baseline empirical strategy uses this heterogeneous

FIGURE 1

Changes in Section 179 Depreciation Limits

Figure 1 plots Section 179 depreciation limits ($, 000). The black solid line plots the limits at the federal level, while the gray dots represent the limits for different states.



[2]See Kitchen and Knittel (2016) for a descriptive analysis of Section 179.

exposure of routine and non-routine workers in the same state and year to estimate the causal impact of Section 179 limit changes on employment risk, household portfolio choice, and wealth accumulation.[3]

We begin our empirical analysis by investigating the effect of Section 179 changes on worker displacement risk. We show that a $100,000 increase in state-level Section 179 limit corresponds to a 0.57 percentage point increase in layoff probabilities of routine workers over the next 3 years (relative to non-routine workers)—a 9.1% increase in the relative likelihood of losing one's job over the unconditional layoff probability of 6.3% over the sample period. Our regressions employ individual-level demographic and education controls, as well as state-by-year fixed effects to absorb local labor market fluctuations. Our results on displacement risk complement those obtained by Tuzel and Zhang (2021) who, using establishment-level employment data, show that technological adoption incentivized by Section 179 disproportionately lowers the employment prospects of routine workers.

We then proceed to our main result that explores the effect of Section 179 limit changes on the portfolio choice of workers. We use the share of liquid wealth invested in stocks and mutual funds (called the stock share) as the key variable of interest. We show that workers employed in routine occupations (alternatively, routine households) respond to a $100,000 increase in limit by reducing their stock share by 0.74 percentage points in the following year. The effect is persistent, and the stock share decreases by 1.4 percentage points over a 3-year period. The economic significance of this result can be gauged from the fact that average stock share in our sample is about 15%. Thus, the stimulus program lowers the stock market exposure of routine households by 5% to 9% over a 3-year period. Our estimation controls for demographic factors, income- and wealth-related factors, and unobserved state-level fluctuations that may jointly determine the occupational choice and portfolio allocation.[4] Our results are consistent with the theoretical mechanism outlined in Cocco, Gomes, and Maenhout (2005), which shows that even a small increase in unemployment probability can generate substantial rebalancing of households' financial portfolios away from stocks and risky investments.

We run several robustness tests to support our main result. First, we verify that workers do not frequently switch across routine and non-routine occupational groups. Such switching friction is essential to our identification strategy, which relies on the differential exposure of routine and non-routine households to the state-level stimulus shocks. Additionally, we do not find any increase in the take-up of upskilling programs after the policy shock. This suggests that a likely reason behind the switching friction is the large cost of human capital investment which the workers are unable or unwilling to bear. Second, we verify that state adoption of

[3]Our classification rests on an established literature on Routine Biased Technological Change (RBTC). See, for example, Autor, Levy, and Murnane (2003), Autor, Katz, and Kearney (2006), (2008), and Acemoglu and Autor (2011), among others. Following Autor (2015), we classify an individual as a routine worker if her score falls in the top tercile of the routine task intensity (RTI) score distribution.

[4]See, Dow and da Costa Werlang (1992), Grinblatt, Keloharju, and Linnainmaa (2011), Addoum, Korniotis, and Kumar (2017), Van Rooij, Lusardi, and Alessie (2011), Christelis, Jappelli, and Padula (2010), Kaustia and Torstila (2011), and Fagereng, Gottlieb, and Guiso (2017).

Section 179 limits is not correlated with aggregate economic conditions that may affect routine and non-routine workers differently. We also address concerns with using stock share as the primary outcome variable. Since stock share is a ratio measure, we show that our results are driven by the decline in stock wealth (numerator) and not by an increase in liquid asset holdings (denominator) which may happen due to precautionary reasons. We also look separately at the extensive and intensive margin of stock market participation and find consistent results across both dimensions.

Finally, we address concerns about alternative mechanisms that may drive our results. First, it is possible that Section 179 and associated economic changes may affect beliefs about future stock returns among workers. To investigate this possibility, we exploit variation in the generosity of unemployment insurance (UI) programs across states and show that workers living in states with generous UI benefits are less responsive to the policy shocks. Apart from controlling the belief channel, these results also indicate that government-provided insurance can lower the background risk generated by the investment stimulus policy and its impact on household balance sheets. Second, Section 179 may induce firms to undertake more risky investments and our results might capture workers' response to this firm-level risk instead of their own displacement risk. We address this concern by showing that firm bankruptcies actually declined after the policy shock, and that our results are not driven by increases in firm risk following state depreciation limit changes. Third, we show that our results are not driven by changes in workers' income, wealth, or realized unemployment shocks which may affect stock market participation due to liquidity constraints or risk aversion. Finally, we show that our results are not driven by other depreciation tax policies like Section 168 that may be correlated with Section 179 policy changes.

After establishing the background risk due to potential future layoffs as the key mechanism behind our results, we show that the shift away from stocks manifests into a lower average return on liquid wealth for routine households. We use household portfolio weights across stocks, bonds, interest-bearing bank accounts, and checking deposits to estimate their realized return on liquid wealth. We show that following a \$100,000 increase in Section 179 limit, routine households earn 61 basis points lower return in the following year compared to non-routine households. We find that the return differential does not close, but rather diverges to 80 basis points in 3 years. We also use annual change in liquid wealth as an alternative outcome variable, since it provides direct evidence on the wealth accumulation process of households. We find that, after a \$100,000 increase in limits, the growth rate of liquid wealth of routine households is 6.2% lower relative to that of non-routine households. Overall, our results indicate that investment stimulus provided by the government can have unintended consequences on the portfolio choice of affected workers and has implications for aggregate stock market participation and wealth inequality.

Our paper makes three important contributions. First, it contributes to a growing literature on the effects of government investment stimulus policies. Existing studies look at the effect on corporate investment (Zwick and Mahon (2017), Ohrn (2016), (2019)) and labor outcomes (Tuzel and Zhang (2021), Gaggl and Wright (2017), Garrett, Ohrn, and Suárez Serrato (2020), Acemoglu, Manera,

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

and Restrepo (2020)). Ours is the first study on the spillover effects of such tax incentives on household finance outcomes, especially for workers facing increased displacement risk.

Second, our paper contributes to the literature on background risk and portfolio choice.[5] Fagereng, Guiso, and Pistaferri (2018) highlight the difficulty in establishing the role of uninsurable income risk in portfolio choice because of the challenges in measuring large exogenous variation in the uninsurable income risk in the data. Earlier papers on this topic used subjective expectations data (Guiso, Jappelli, and Terlizzese (1996), Hochguertel (2003)), which might suffer from measurement error (Manski (2004)), or used residual variation in earnings (Angerer and Lam (2009), Betermier, Jansson, Parlour, and Walden (2012)), a large part of which might reflect individual choice rather than risk (Low, Meghir, and Pistaferri (2010), and Guvenen and Smith (2014)). A key contribution of our paper is providing an empirical setting where the background risk is i) plausibly exogenous so as to overcome this measurement problem, and ii) large enough to overcome widely documented household portfolio inertia (Bilias, Georgarakos, and Haliassos (2010), Ameriks and Zeldes (2011)). Accordingly, we find economically large effects on portfolio choice. Another important contribution of our paper is to document a novel and important source of background risk: countercyclical fiscal policy. Such policies are an important part of the government toolkit, and understanding their spillovers on the workers is important for policymakers.

Our paper is closely related to Gomes, Jansson, and Karabulut (2018)), who also focus on capital-labor substitution. However, there are several key differences. First, they document the effects of industrial robot penetration at the industry level on wealth dispersion, whereas our paper focuses on firm investment driven by government tax policy. Second, while industrial robots reflect a specific form of technological change, our paper documents the effect of general investment in labor-substituting capital on affected workers across all types of occupations and industries. Finally, they use education as a proxy for displacement risk. Since education may independently affect portfolio choice, we use detailed occupation data at the worker level and measure displacement risk and portfolio choice of workers at similar education levels.

Third, our paper is connected to the growing literature that studies the effects of technological investments on wealth inequality. A large literature in macroeconomics and labor economics has attributed the rise in wealth inequality to wage premiums commanded by high-skilled workers who are better poised to complement the technological-innovation-driven demand for capital (Katz and Murphy (1992), Acemoglu (1998), (2002), Krusell, Ohanian, Ríos-Rull, and Violante (2000), Piketty and Saez (2003), Autor et al. (2003), Goldin and Katz (2009)). While most of the existing literature focuses on the importance of heterogeneity in returns to human capital in explaining the rapidly increasing wealth inequality, a

[5]See Merton (1971), Kimball (1993), and Heaton and Lucas (1997) for theoretical contributions. Other important papers include Koo (1995), Massa and Simonov (2006), Benzoni, Collin-Dufresne, and Goldstein (2007), Lynch and Tan (2011), and Catherine (2019). The empirical evidence on the importance of background risk, however, is mixed. See, for instance, Angerer and Lam (2009), Betermier, Jansson, Parlour, and Walden (2012), Bonaparte, Korniotis, and Kumar (2014), Fagereng, Guiso, and Pistaferri (2018), and Dimmock (2012).

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

recent strand of papers focus on the importance of capital gains (Fagereng, Guiso, Malacrino, and Pistaferri (2020), Moll, Rachel, and Restrepo (2022)). We contribute to this literature by showing that increasing layoff risk and associated portfolio rebalancing, especially among households that are susceptible to displacement, can also affect wealth inequality over medium and long runs.

II. Data and Summary Statistics

In this section, we describe the data used in our empirical analysis and explain the construction of key variables.[6]

A. Household Demographics and Balance Sheet

Our primary data come from the Survey of Income and Program Participation (SIPP), a nationally representative longitudinal survey conducted by the U.S. Census Bureau. Each SIPP panel tracks about 45,000 households over a 4-year period. Core interviews, referred to as "waves" in the data, collect information on demographics, unemployment, income, and program participation. In addition, supplemental interviews are conducted annually to gather information on household assets and liabilities. Our study covers data from four SIPP panels covering the period 1996 through 2013.[7] The panel structure of SIPP data provides key advantages over other widely used surveys like the Survey of Consumer Finances (SCF).[8]

Table 1 provides a summary of the key variables used in the empirical analysis. The final sample contains detailed demographic information about 186,934 unique households with 792,136 members who were employed during at least one wave over the sample period. The median (mean) individual in the sample is a 38 (39)-year-old white male who has about one year of college education and earns approximately \$1,900 (\$2,700) per month. Our sample's median household has 2 working members with a total monthly income of \$4,700. Turning to household balance sheets, we find that a median household has a total wealth of \$141,000 out of which \$68,000 is invested in durable assets (housing and vehicles) while only \$1,600 is kept in liquid assets. The average liquid wealth holding in the entire sample is only \$23,000. On average, 21% of liquid wealth is held in the form of money, which we proxy with checking account holdings. 80% is held in various interest-earning accounts in banks, while 4% is allocated toward holdings of federal and municipal bonds. Consistent with a substantial literature on the retail investor stock market participation puzzle, we find only 20% of households participate in the stock market, and the total stock and mutual fund holdings account for approximately 15% of the total liquid wealth.

[6]Table A1 in the Appendix shows the definition of key variables used in the empirical analysis.

[7]SIPP made substantial changes to their survey methodology starting with the 1996 panel. To preserve homogeneity, we start our analysis with 1996. Also, data were not collected during the global financial crisis, and the balance sheet information of households is missing for 2007, 2008, and 2009.To preserve homogeneity, we start our analysis with 1996. Also, data were not collected during the global financial crisis, and the balance sheet information of households is missing for 2007, 2008, and 2009.

[8]SCF is a series of cross-sectional surveys, which precludes the possibility of tracking changes in household asset positions over time. See Chetty, Sándor, and Szeidl (2017) for a discussion on the benefit of using SIPP for studying portfolio choices of U.S. households.

TABLE 1

Summary Statistics

Table 1 shows the summary statistics for key variables used in the empirical analysis.

	Mean	Median	SD	P10	P90
Panel A. Demographic Variables					
RTI Score	−0.04	−0.34	1.06	−1.42	1.56
Age (years)	38.85	38.00	13.90	21.00	58.00
Male	0.50	0.00	0.50	0.00	1.00
Female	0.50	1.00	0.50	0.00	1.00
White	0.82	1.00	0.39	0.00	1.00
Black	0.12	0.00	0.33	0.00	1.00
Asian or Pacific Islander	0.03	0.00	0.16	0.00	0.00
American Indian, Aleut, or Eskimo	0.04	0.00	0.18	0.00	0.00
Less than HS	0.12	0.00	0.32	0.00	1.00
High school or GED	0.29	0.00	0.45	0.00	1.00
Some college	0.25	0.00	0.43	0.00	1.00
College graduate	0.08	0.00	0.27	0.00	0.00
Graduate degree	0.26	0.00	0.44	0.00	1.00
Panel B. Employment and Income					
Layoff (%)	4.47	0.00	20.66	0.00	0.00
Switch (%)	5.38	0.00	22.56	0.00	0.00
Wage income ($, monthly)	2,654.93	1,944.00	3,074.63	277.00	5,442.00
Dividend income ($, monthly)	12.21	0.00	90.89	0.00	1.00
Interest income ($, monthly)	12.90	0.00	83.65	0.00	20.00
Total household income ($, monthly)	5,922.12	4,703.00	5,260.79	1,574.00	11,084.00
Employer: $<$ 100 employees	0.06	0.00	0.24	0.00	0.00
Employer: $\geq$ 100 employees	0.54	1.00	0.50	0.00	1.00
Panel C. Household Wealth ($, 000)					
Total wealth	235.50	141.79	275.72	3.65	595.33
Retirement wealth	97.25	25.42	160.28	0.00	285.70
Durable wealth	94.62	68.49	108.13	0.30	233.15
Illiquid wealth	20.48	0.04	67.28	0.00	48.81
Liquid wealth	23.14	1.60	60.39	0.00	67.51
Total debt	46.57	13.20	67.71	0.00	138.19
Panel D. Household Portfolio (%)					
Stock market participation	20.29	0.00	40.22	0.00	100.00
Stock share	15.12	0.00	30.63	0.00	77.22
Bond share	4.40	0.00	14.68	0.00	10.71
Money share	21.38	0.00	37.16	0.00	100.00
Interest earning share	79.58	100.00	34.38	11.47	100.00
Return on liquid wealth	2.02	1.05	2.77	0.00	5.60

B. Routine Versus Non-Routine Occupations

SIPP collects detailed information on the occupation and industry codes of all employed individuals within a household. Using the methodology outlined in Goos, Manning, and Salomons (2014), we match occupation codes in SIPP to Standardized Occupational Codes (SOC) provided by the U.S. Department of Labor's Dictionary of Occupational Titles (DOT). Our sample covers 980 occupations spanning 88 2-digit NAICS industries.

Following the Routine-Biased Technological Change (RBTC) literature, we construct the routine tax intensity (RTI) measure for each occupation in our sample by matching the occupational classification code with the data on occupational requirements from the fourth edition of the DOT.[9] Occupations having a higher

[9]For details on the creation of the RTI measure, see Autor et al. (2003), (2006), (2008) and Acemoglu (1998).

level of routine content are termed routine occupations. We follow the directions in Autor and Dorn (2013) to convert the RTI Score into a $\text{HighRTI}_{i,s,t}$ indicator at the individual level, which assumes a value of 1 if the RTI score for the occupation of individual i living in state s in year t lies in the upper tercile of the RTI score distribution.[10] Finally, to facilitate comparison across households with multiple employed members, we define a household-level RTI variable as the average RTI score for each employed member in the household. Correspondingly, households that lie in the upper tercile of household-level RTI score distribution are termed as routine households.

Table A2 in the Appendix provides examples of routine and non-routine occupations for every income decile. Jobs with the highest RTI scores include data entry professionals, customer service representatives, adjusters and calibrators, cashiers, tailors, or dressmakers. Such occupations with high RTI scores are partially or completely replaceable by labor-displacing capital. At the other extreme, occupations with the lowest RTI scores include teachers, managers, machine and vehicle operators, teaching aides, and lodging managers. All these occupations require a higher level of cognitive skills, active problem-solving, or interpersonal skills. These occupations are complementary to labor-substituting capital.[11] Figure 2 shows the distribution of RTI scores across routine and non-routine occupations in our individual-level SIPP sample. The orange bars in the histogram refer to the individuals employed in non-routine occupations (the routine dummy assumes a value of 0 for all these individuals). In contrast, the gray bars correspond to the individuals in routine occupations. The mean RTI score in our sample is -0.04.

As Autor et al. (2006) have noted, routine occupations form the middle part of income distributions, while non-routine occupations primarily capture the 2 tails. A potential threat to our analysis stems from the concern that disparities in income profiles may bias the results. For instance, individuals working in a non-routine occupation may have a higher income relative to those working in routine jobs. Figure 3 shows that this concern is small, at least in our sample. The figure plots the kernel densities of high and low RTI households over labor income and wealth. While non-routine individuals earn a higher wage and are wealthier on average, there is considerable overlap in the kernel densities. We residualize earnings and wealth variables using observable characteristics of the individuals and their households.[12]

C. Comparing Routine and Non-Routine Households

We discuss differences between routine and non-routine households before proceeding with our main identification strategy. This analysis is important for two

[10]The RTI score is calculated each year according to the occupation reported by the panelists for that year. Thus, occupational switches do not cause a major issue with our analysis.

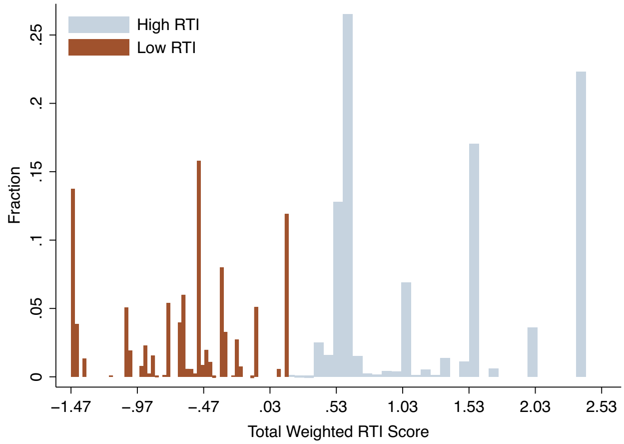
[11]More than one occupation can have the same RTI score. This is because some occupations are very close to each other. For example, science teachers and art teachers are both grouped under the category "Teachers."

[12]Controls include education, age, sex, race, marital and home-ownership status, and employer size. We also include state-year fixed effects to account for aggregate factors.

FIGURE 2

RTI Score Distribution

Figure 2 plots the distribution of Routine Task Intensity (RTI) score in the individual-level SIPP sample. The construction process of RTI score is described in the main text. We define an occupation to lie in the high RTI group (denoted as routine occupation) if it lies in the top tercile of the RTI distribution.



reasons. First, it uncovers underlying differences between these two groups, providing a baseline for calculating the economic magnitudes of changes induced by the policy shock we study. Second, it helps us create a vector of control variables for our main regressions to effectively rule out alternative explanations that may bias our results.

Univariate analysis of key variables for routine and non-routine households are presented in Appendix Table A3. While routine and non-routine households are similar in terms of age and other demographic characteristics, they differ significantly in terms of their educational qualifications, income potential, and wealth distributions. 39% of routine workers have a maximum education of high school, compared to 24% of non-routine workers. On the other hand, 10% of routine workers have a graduate degree, compared to 33% of non-routine workers. Moreover, non-routine workers experience a lower risk of layoffs and earn about 50% more than their routine counterparts. There are significant differences in their accumulated wealth as well. Routine households are considerably poorer relative to their non-routine counterparts and tend to hold a more conservative asset portfolio. Routine households are less leveraged and hold a larger fraction of liquid wealth in safe assets like checking accounts or interest-earning accounts in banks. They are also much less likely to invest in the stock market. On average, only 18% of routine households hold any form of direct or indirect equity, compared to 23% of non-routine households.

These cross-sectional patterns show that routine and non-routine households differ across several key observable dimensions. We take cognizance of these differences in designing our empirical strategy. Specifically, we control for all these observable factors when we study how government stimulus may impact the

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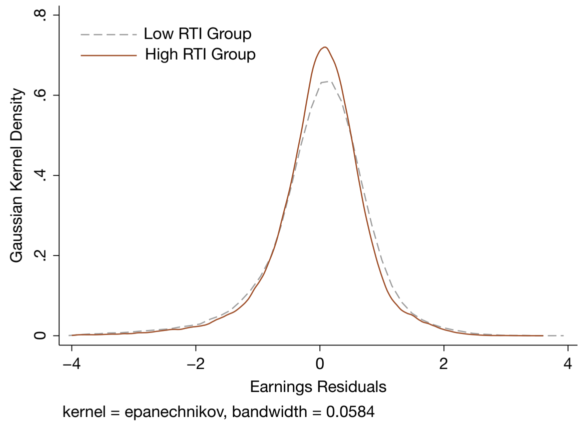
FIGURE 3

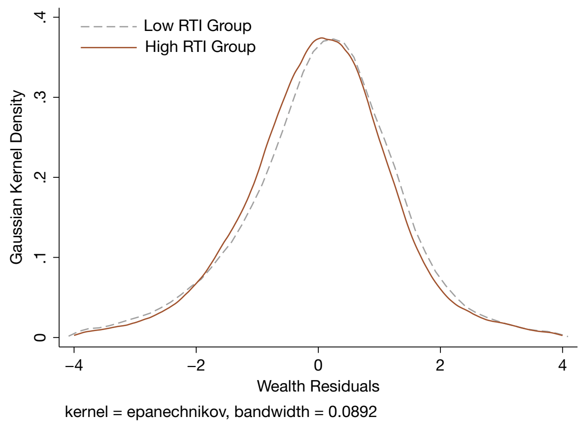
Wage Earnings and Wealth of Routine and Non-Routine Households

Figure 3 plots the distribution of wage earnings residuals in Graph A and wealth residuals in Graph B for routine and non-routine households. Routine households refer to the group which lies in the top tercile of the household-level RTI score. To calculate wage earnings and wealth residuals, we regress the wage earnings and wealth of a household on key demographic characteristics, including age, sex, race, education, marital and home ownership status, and employer size of the main respondent. We also employ state-year fixed effects to absorb state-level economic factors which may influence wage earnings and wealth.

Graph A. Wage Earnings Residuals



Graph B. Wealth Residuals



portfolio choice of worker households.[13] Additionally, we control for state-year fixed effects to absorb the effect of local fluctuations on individuals' financial decisions. In our robustness exercise, we further employ state-industry-year fixed effects to additionally control for industry fluctuations that may impact our outcomes of interest.

[13]We exclude non-routine unskilled workers from our analysis because, even though they are non-routine, they are mainly engaged in manual work and do not benefit from capital investments. Although, our results are not sensitive to this exclusion.

D. Section 179 Depreciation Allowances

Businesses in the United States follow the Modified Accelerated Cost Recovery System (MACRS) to decide the life and depreciation schedule for each type of capital investment. Sections 168 and 179 of the Internal Revenue Code allow further depreciation on certain types of investments. Section 168 is about bonus depreciation, in which businesses can depreciate an extra (bonus) percentage of the equipment cost in the first year of purchase. Section 179 allows for expending (i.e., depreciating 100% of the cost of equipment in the year of purchase) of investments that have an active life of less than 20 years. Because of this, most structure investments, like real estate and buildings, do not qualify for Section 179 deductions.

Depreciation under Section 179 is subject to two limits: the *deduction limit* and *phaseout threshold*, which dictate the total amount of investment that can be depreciated under the accelerated schedule. Assume, as was the case in 2010, that the deduction limit is \$500,000 and the phaseout threshold is \$2,000,000. Under these restrictions, all businesses with eligible investments less than the deduction limit can expense all their capital expenditure using an accelerated schedule. However, if the investment amount is between the deduction limit and the phaseout threshold, the maximum allowable investment amount is capped at \$500,000. Finally, once the investment amount exceeds the phaseout threshold, a claimable deduction under Section 179 reduces dollar-for-dollar, becoming 0 at \$2,500,000.

The level and structure of Section 179 have evolved over time. Tax incentives were small before 2003 and have been significantly increased since (Guenther (2015)). Starting with \$24,000 in 2002, the deduction limits were subsequently increased to \$100,000 in 2003, \$250,000 in 2008, and further to \$500,000 in 2010. The deduction limit and the phaseout threshold are both determined at the federal level. However, individual states may choose to follow the federal limits, ignore them, or partially implement them. If the state follows the federal limit, then the firms in that state would enjoy the same benefits for both federal and state-level taxes. Ohrn (2016) shows that while almost all the states followed the federal limits before 2003, the conformity has fallen steadily with more states opting out or only partially implementing the subsequent increases in Section 179 provisions. For instance, in 2013, only 60% of states had the same deduction limit and phaseout threshold as the federal equivalents. Figure 1 shows the federal and state depreciation thresholds over the sample period. Federally adopted limits are represented by the solid black line, while the dots represent variations in state mandates.

To understand how Section 179 incentivizes investments in technology and labor-substituting capital, consider the following example. With few exceptions, investments eligible for accelerated depreciation through Section 179 are limited to depreciable tangible capital. Assuming a 5-year MACRS depreciation schedule[14], the marginal tax rate of 6.08% and an interest rate of 10%, the present value of

[14]Under MACRS, capital with 5-year economic life depreciates by 20% in the year of purchase, 32% in the second year, and 19.20%, 11.52%, 11.52% and finally 5.76% respectively in the next 4 years.

all the depreciation tax shields amount to an additional 1% of the purchase price of capital under Section 179. Unsurprisingly, Tuzel and Zhang (2021) find a 1.7% additional increase in computers in eligible establishments following a \$250,000 increase in state Section 179 limit.

We use variation in state adoption of Section 179 deduction limits to identify the effect of investment in capital by a company on its workers' asset allocations. There are several reasons why such variation is useful. First, Section 179 benefits only accrue to firms investing in eligible capital, which are substitutes for labor, instead of plants and buildings, which can potentially increase future labor demand.

Second, cross-sectional variation in state-level Section 179 deduction limits allows us to create a plausibly exogenous shock to displacement risk across households in different states. However, for our identification strategy to be valid, changes in state adoption of Section 179 limits, $\Delta\text{Limit}_{s,t}$ should not be correlated with any factors that directly affect the portfolio choice of routine and non-routine workers. We verify this assumption in Appendix Table A4. We show that changes in the deduction limits are not correlated with state-level macroeconomic variables including the unemployment rate, per-capita GDP, wage levels, and UI benefits.

Third, while the increase in the present value of depreciation tax benefits appears small, Tuzel and Zhang (2021) show that the routine workers' employment declines by 6% over a 3-year window after a \$250,000 limit increase. During the same period, employment of non-routine skilled workers increases by 3.5%. Our estimates on the relative layoff probability of routine workers are in line with these estimates. Thus, the impact of Section 179 limit changes is not only economically significant but is also sharply heterogeneous across routine and non-routine workers, which is crucial for our paper. It allows us to compare the changes in portfolio allocation of routine and non-routine workers, and to isolate the role of labor-substituting capital investment induced by government stimulus from other determinants of portfolio choice.

III. Empirical Analysis

A. Layoff Risk

We start by assessing whether state-level changes in Section 179 depreciation limits lead to a change in layoff risk for routine and non-routine workers. Specifically, we consider a difference-in-difference specification given by:

$$\text{(1)} \quad \text{Layoff}_{i,s,t\rightarrow t+k} = \begin{aligned}[t] &\beta \times \text{HighRTI}_{i,s,t-1} \times \Delta\text{Limit}_{s,t-1\rightarrow t} + \beta_1 \times \text{HighRTI}_{i,s,t-1} \\ &+ \psi_{s,t} + \gamma \mathbf{X}_{i,s,t} + \epsilon_{i,s,t}, \end{aligned}$$

where $\text{Layoff}_{i,s,t\rightarrow t+k}$ is a dummy variable which assumes a value of 1 if an individual i in state s was laid off from their job at any time between year t and $t+k$. $\text{HighRTI}_{i,s,t-1}$ denotes a dummy variable that is 1 if the individual's RTI score in year $t-1$ lies in the top tercile of the distribution, and 0 otherwise. Individuals with $\text{HighRTI}_{i,s,t-1} = 1$ are denoted as routine workers and those with $\text{HighRTI}_{i,s,t-1} = 0$ are denoted as non-routine workers. $\Delta\text{Limit}_{s,t-1\rightarrow t}$ denote the

TABLE 2

Layoffs, Routine Jobs, and Section 179

Table 2 summarizes the results from linear probability regressions of layoffs on the dummy variable for high RTI occupation (top tercile of RTI score), annual change in state-level investment tax benefits (Section 179 of the IRS tax code), their interaction, and a set of controls. Specifically, we estimate the regression specification in equation (1), where $\text{Layoff}_{i,s,t \to t+k}$ is a dummy variable which assumes a value of 1 if an individual i in state s was laid off from their job between year t and $t+k$. $\text{HighRTI}_{i,s,t-1}$ denotes a dummy variable that is 1 if the individual's RTI score in year $t-1$ lies in the top tercile of the sample distribution, and 0 otherwise. Individuals with $\text{HighRTI}_{i,s,t-1}=1$ are denoted as routine workers and those with $\text{HighRTI}_{i,s,t-1}=0$ are denoted as non-routine workers. $\Delta\text{Limit}_{s,t-1 \to t}$ denote the change in Section 179 depreciation limit for state s from year $t-1$ to t. $\mathbf{X}_{i,s,t}$ denote the set of demographic control variables that include an individual's education, age, sex, race, marital and home-ownership status, and employer size. We use state-year fixed effects ($\psi_{s,t}$) and cluster standard errors at the state level. *** indicates significance at the 1% level.

	Dependent Variable: $\text{Layoff}_{i,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × High RTI	0.012	0.407***	0.607***	0.573***
	(0.116)	(0.124)	(0.139)	(0.152)
High RTI	1.026***	0.953***	1.206***	1.285***
	(0.087)	(0.097)	(0.137)	(0.148)
Obs.	357,466	357,466	357,466	357,466
R^2	0.02	0.02	0.03	0.03
$\bar{y}$	4.08	4.08	5.67	6.3
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes

change in Section 179 depreciation limit for state s from year $t-1$ to t. We control for several variables (denoted as a vector $\mathbf{X}_{i,s,t}$) that may impact an individual's employment condition, including education, age, and other demographic characteristics. We absorb the effect of local economic shocks using state-year fixed effects $(\psi_{s,t})$ and cluster standard errors at the state level. The coefficient of interest (β) captures the differential impact of the policy change across routine and non-routine workers.[15]

Table 2 shows the effect of changes in state adoption limits in year t on layoff probabilities across routine and non-routine workers from year t to $t+k$. Routine workers experience a 0.41 percentage point increase in their layoff probability (relative to non-routine workers) in the year following a \$100,000 increase in state deduction limits. This effect is economically significant relative to the unconditional layoff probability of 4.08% in our sample. The effect is persistent and lasts for at least 2 more years. These results are consistent with Tuzel and Zhang (2021), who using establishment-level data, show that employment in routine occupations reduce significantly over 3 years following a state-level increase in Section 179

[15]SIPP also contains information on the industry in which an individual works. In our main analysis, we do not absorb industry-specific fluctuations in layoff risk because certain routine-intensive industries might be more sensitive to changes in tax policy, and we want to capture the across-industry variation in our outcome of interest. However, industry-level fluctuations correlated with our policy shock may impact routine and non-routine workers differently. For example, the global financial crisis, which disproportionately impacted the employment of routine workers (Jaimovich and Siu (2020)), was followed by the largest increase in depreciation limits in our sample. Thus, for robustness, we rerun all our specifications with state-by-industry-by-year fixed effects. Our results remain statistically and economically significant and are reported in Appendix Table A5. In other words, our results are not explained by industry shocks correlated with the change in depreciation limits.

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

deduction limits. Our results obtained using individual employment data also show similar patterns. For example, our results suggest that after several states increased their limits from \$250,000 to \$500,000 in 2010, the layoff probability of routine workers increased by 1.4 percentage points relative to non-routine workers in those states over the course of 3 years. This represents around 22% increase relative to the unconditional average.[16]

Each occupation requires a separate skill set, and it may be difficult for routine workers to switch to non-routine occupations in order to avoid losing their jobs in response to the policy shock. We find evidence of friction in occupational switching in our data and discuss the results in Section IV.A. Overall, the results presented in this section, and the evidence provided by Tuzel and Zhang (2021), suggest that employment risk for individuals engaged in routine occupations increases significantly after their states adopt higher deduction limits. Enhanced tax incentives induced by higher limits push firms to substitute routine labor with technological capital and high-skilled labor who possess the know-how to operate them.

B. Portfolio Allocations

Next, we investigate how employment risk associated with Section 179 depreciation limits affects household portfolio choice. In the presence of switching frictions, employment risk emerges as a key source of background risk (Cocco et al. (2005)), and influences the amount of financial risk a household is willing to bear (Heaton and Lucas (2000)). Since all wealth variables are defined in SIPP only at household level, we calculate household-level RTI score as the weighted average of the RTI scores of the employed members, scaled by their wage income. We analogously define a household h residing in state s at time t as routine-intensive if its RTI score $\text{HHRTI}_{h,s,t}$ lies in the top tercile of household-level RTI score distribution. Our baseline specification is given by:

$$\begin{aligned}(2)\qquad \Delta\text{Stock Share}_{h,s,t\to t+k} = {} & \beta\times\text{HHHighRTI}_{h,s,t-1}\times\Delta\text{Limit}_{s,t-1\to t}\\ & +\beta_1\times\text{HHHighRTI}_{h,s,t-1}+\psi_{s,t}+\gamma\mathbf{X}_{h,s,t}\\ & +\rho\Delta W_{h,s,t}+\epsilon_{h,s,t},\end{aligned}$$

where the dependent variable $\Delta\text{Stock Share}_{h,s,t\to t+k}$ denotes change in stock share of liquid wealth for household h from year t to $t+k$.[17]

To address the potential concern that stock wealth changes may mechanically stem from price movements and not reflect active reallocations, we calculate the

[16] Tuzel and Zhang (2021) find that a \$250,000 increase in state limits lower routine employment by 6% and increase non-routine employment by 3.4%, giving a relative decline of 9.4%. Thus, our results on the increase in layoffs of routine workers relative to non-routine workers are in line with their results.

[17] We use the change in portfolio composition (instead of its level) as the dependent variable because the difference specification removes household fixed effects and controls for spurious correlations due of differences across households. With level regressions, one may argue that differences in outcomes stem from innate skills which may also affect sorting into non-routine cognitive labor, creating a simultaneity bias (Fagereng et al. (2020)). In other words, using a difference specification helps us quantify the response of a given household to state level changes in Section 179 limits.

TABLE 3

Changes in Household Stock Share

Table 3 summarizes the results from a regression on annual change in stock share of liquid wealth on the dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. Our baseline specification is given by equation (2), where the dependent variable $\Delta\text{Stock Share}_{h,s,t\to t+k}$ denotes change in stock share of liquid wealth for household h, residing in state s from year t to $t+k$. The dummy variable $\text{HHHighRTI}_{h,s,t-1}$ assumes the value one if the household h living in state s falls in the top tercile of the household-level RTI score distribution. The vector $\mathbf{X}_{h,s,t}$ denotes controls for household's demographic and wealth-related factors. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual changes in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: $\Delta\text{Stock Share}_{h,s,t\to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	0.208	−0.741**	−1.228***	−1.402***
	(0.319)	(0.315)	(0.297)	(0.298)
HH High RTI	0.452*	0.211	0.656	0.733*
	(0.241)	(0.237)	(0.392)	(0.416)
No. of obs.	79,753	77,723	77,723	77,723
R^2	0.05	0.05	0.06	0.07
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

change in household stock portfolio after netting out price movement related changes as:

$$\Delta\text{Stock Share}_{h,s,t\to t+k} = \text{Stock Share}_{h,s,t+k} - \frac{P_{t+k}}{P_t}\text{Stock Share}_{h,s,t}.$$

Here P_t is the price of the household stock portfolio at time t. Since we do not observe individual portfolios, we use the S&P 500 index as a proxy for the average stock price. Second, allocations of liquid wealth may depend on the current household wealth (Fagereng et al. (2020)). To control for this standard wealth effect, we include annual changes in total household income and wealth. We also control for demographic variables like the household head's education, age, sex, race, and marital status. Finally, we control for the impact of state-level fluctuations using state-year fixed effects $(\psi_{s,t})$ and cluster standard errors at the state level.[18]

Table 3 reports our main results. Each column k corresponds to a version of the equation (2) investigating the effect of change in state deduction limit from year $t-1$ to t on changes in household stock share between years t and $t+k$. The coefficient β captures the relative response of routine households when their employment risk increases due to their employers adapting to the policy shocks. Column 2 shows that routine households respond to a \$100,000 increase in depreciation tax limit by reducing their stock share of liquid wealth by 74.1 basis points. This is not a one-off shift and the decline is persistent. We find that stock share of routine households declines by a total of 140.2 basis points over a 3-year

[18]Using state-year fixed effect absorbs the impact of policy on non-routine households. We re-estimate the equation using state and year fixed effects separately and report the results in Appendix Table A6. The results highlight no impact on tax policy on the stock share of non-routine households.

period following the policy shock. These results suggest that when several states adopted a \$250,000 increase in their depreciation limits in 2010, their routine households lowered their stock share by 3.51%. The average value of stock share in our sample is 15%, indicating the decline due to our policy shock is economically significant.

As with the layoff result, we do not use industry information in our baseline specification as we want to capture the disproportionate effect of the policy across different industries. However, to mitigate the concern that our results may be driven by differing time-varying risks at industry level, we estimate an alternate specification that uses state-industry-year fixed effects. This ensures that our key estimate β is identified off cross-sectional differences in portfolio adjustments of routine and non-routine respondents residing in the same state and employed in the same census 4-digit industry. Results reported in Appendix Table A7 show that the estimated coefficients are similar to those in the baseline specification, implying that our main results are not driven by between-industry fluctuations that may be potentially correlated with the policy shocks.

Finally, we address an important concern related to our main outcome variable. Since stock share is a ratio measure, the post-policy decline in stock share may arise mechanically if households increase their non-stock liquid wealth (by consuming less or by selling their illiquid assets) after the policy shock. To analyze this concern, we re-estimate equation (2) using changes in the dollar value of stock and mutual fund holdings as the dependent variable. Appendix Table A8 shows that, after an increase in depreciation limits, there is a persistent decline in the size of the stock portfolio for routine households. At the same time, Appendix Table A9 shows that the fraction of household wealth held in liquid assets declines, albeit marginally, after the policy shock. Overall, these results indicate that households respond to an increase in their layoff risk by substituting their stock holdings with other liquid assets and not by increasing their liquid asset holdings.

1. Extensive and Intensive Margin of Stock Market Participation

We further decompose the main effect into two parts: i) the extensive margin where households exit the stock market altogether, and ii) the intensive margin where portfolio rebalancing occurs without full exit. For the extensive margin, we define the exit as an indicator variable which assumes a value of 1 if the household had a positive stock balance previously, but that dwindles to 0 in the current period. That is,

$$\text{Exit}_{h,s,t} = \begin{cases} 1 & \text{if Stock Share}_{h,s,t-1} > 0 \ \& \ \text{Stock Share}_{h,s,t} = 0 \\ 0 & \text{otherwise.} \end{cases}$$

On the other hand, for the intensive margin, we consider the effect on households who reallocate wealth between the stock market and safe assets, but do not exit the stock market altogether. The results of this decomposition are reported in Table 4. In Panel A, we consider the effect of investment tax stimulus on stock market exit. Households with high exposure to such labor-adjusting tax stimuli gradually withdraw from risky investments. This gradual reallocation manifests in a

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

TABLE 4

Changes in Household Stock Share: Extensive and Intensive Margin

Table 4 summarizes the results from a regression on annual change in stock share of liquid wealth on the dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. In Panel A, we consider the extensive margin (i.e., the probability that the household exits the stock market altogether). The dependent variable $\text{Exit}_{h,s,t\rightarrow t+k}$ is a dummy which assumes a value one if the household h residing in state s has a positive stock holding at time t, but 0 wealth in stocks and mutual funds at time $t+k$. In Panel B, we focus on the intensive margin by considering the stock reallocation decisions of households who do not exit the stock market. That is we impose the condition that $\text{Exit}_{h,s,t\rightarrow t+k}=0$. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-industry-by-year fixed effects and cluster standard errors at state level. *** and * indicate significance at the 1% and 5% levels, respectively.

Panel A. Probability of Stock Market Exit

	Dependent Variable: $P(\text{StockMarket Exit}_{h,s,t\rightarrow t+k})$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI	−0.435*	−0.096	0.127*	0.203***
	(0.205)	(0.116)	(0.065)	(0.040)
HHHighRTI	0.151	0.286	0.075	−0.079
	(0.271)	(0.202)	(0.249)	(0.189)
No. of obs.	80,612	80,612	80,612	80,612
R^2	0.25	0.25	0.27	0.27
$\bar{y}$	2.48	2.48	5.26	6.26
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

Panel B. Changes in Stock Share Without Exit

	Dependent Variable: $\Delta\text{Stock Share}_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI	−0.057	−0.862***	−1.265***	−1.440***
	(0.201)	(0.318)	(0.311)	(0.315)
HHHighRTI	0.235	0.215	0.348	0.422
	(0.191)	(0.224)	(0.370)	(0.408)
No. of obs.	75,084	73,054	70,954	70,572
R^2	0.05	0.05	0.06	0.07
$\bar{y}$	−0.66	−0.66	−3.75	−5.07
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

higher probability of stock market withdrawals, especially in a couple of years following an increase in state investment limits. Specifically, we find that the probability of stock market exit increases significantly by 20.3 basis points by the third year following a \$100,000 increase in state depreciation limits. For perspective, the corresponding average exit rate in our sample is 6%, suggesting that the impact along the extensive margin is economically large.

On the other hand, households that do not completely exit the market also significantly reduce their holdings of stocks and mutual funds. In Panel B of Table 4, we report the results of equation (2) where the dependent variable is the change in stock share of liquid wealth for households who do not completely exit the market. Our results are very similar to those obtained in the baseline specification in Table 3. Overall, our results portray a consistent rebalancing away from financial risk along the extensive and intensive margin for households exposed to heightened unemployment risk.

2. Economic Magnitude of the Policy Impact

The effect of Section 179 policy shock on stock market participation has a similar magnitude to that of other factors studied in the household finance literature. For example, Agarwal, Aslan, Huang, and Ren (2022) show that households experiencing political uncertainty due to gubernatorial elections reduced their stock allocation by 3.8%. Similarly, a 1-standard-deviation increase in exposure to local fraud decreases households' participation rate by 4% and equity-wealth ratio by 10% (Giannetti and Wang (2016)). Other related studies are Ilhan (2020) that shows a 1-standard-deviation increase in exposure to sea-level rise can lower the share of financial wealth invested in risky assets by 9%. Finally, Gomes et al. (2018) document that a one standard deviation rise in robot adoption lowers stock share by a similar order of magnitude.[19] Our results that a \$100,000 increase in the Section 179 limit adversely affects routine households by lowering their stock share by 5% to 9% and increasing their exit rate by 3% relative to the unconditional average over a 3-year period aligns with previous studies in terms of the magnitude of the effect.

C. Robustness

In this section, we provide several robustness checks to the main results presented above.

1. Anticipated Versus Realized Unemployment

We first highlight an important distinction between the set of workers that are still employed but facing a higher probability of layoff in the future and another set of workers who have already lost their jobs. The first set of workers are the main focus of our paper and their response sheds light on the impact of heightened background risk (i.e., the increase in the probability of losing *future* wage income) on current portfolio choice. In contrast, the second set of workers have already lost their income stream and their response highlights how the realization of layoff event (and the associated decline in *contemporaneous* wage income) may affect portfolio choice.

We first address the concern that our main results might be driven by individuals who have already lost their jobs in response to the policy shock. To show that

[19]Other studies have used wage volatility as a proxy for background risk. Betermier et al. (2012) demonstrated that a 3% rise in wage volatility resulted in a 1% decrease in the allocation to risky assets. Fagereng et al. (2017) found that a one standard deviation increase in residual earnings risk led to a 3% reduction in such allocation. Beyond background risk, there are other factors that affect stock market participation. A 1-standard deviation increase in advanced literacy raises stock market participation by about 9 percentage points (Van Rooij et al. (2011)). Similarly, Grinblatt et al. (2011) show that the lowest IQ individuals have a participation rate that is 20.5 percentage points less than that of the highest IQ individuals. Formative beliefs also affect households' investment behavior. For example, Bharath and Cho (2023) show that 1-standard-deviation increase in disaster experiences lowers the risky asset share by 6.8% through their impact on risk aversion and beliefs. Knüpfer, Rantapuska, and Sarvimäki (2017) use the Finnish great depression as a setting and show that the stock market participation rate, more than a decade after the depression, was 2.8 to 3.6 percentage points lower for workers who experienced a 1-standard-deviation deterioration in labor market conditions during the depression. These studies highlight a multitude of factors whose economic impact on household behavior is similar to that of Section 179 policy shock in terms of economic magnitude.

our results are not explained by such contemporaneous (or recent) unemployment, we create an indicator variable $\text{Unemployment}_{h,s,t\rightarrow t+k}$ which assumes a value of 1 if there is an increase in the number of unemployed members in household h in state s between years t and $t+k$. We estimate a triple-difference specification with the key independent variables of equation (2) interacted with the unemployment indicator variable. Specifically, we estimate the regression

$$(3)\quad \begin{aligned}\Delta y_{h,s,t\rightarrow t+k} = {} & \beta \text{HHHighRTI}_{h,s,t-1} \times \Delta\text{Limit}_{s,t-1\rightarrow t} \\ & \times \text{Unemployment}_{h,s,t\rightarrow t+k} + \beta_1 \text{HHHighRTI}_{h,s,t-1} \\ & \times \Delta\text{Limit}_{s,t-1\rightarrow t} + \beta_2 \text{Unemployment}_{h,s,t\rightarrow t+k} \times \Delta\text{Limit}_{s,t-1\rightarrow t} \\ & + \beta_3 \text{HHHighRTI}_{h,s,t-1} \times \text{Unemployment}_{h,s,t\rightarrow t+k} \\ & + \beta_4 \text{HHHighRTI}_{h,s,t-1} + \beta_5 \text{Unemployment}_{h,s,t\rightarrow t+k} \\ & + \psi_{s,t} + \gamma \mathbf{X}_{h,s,t} + \rho \Delta W_{h,s,t} + \epsilon_{h,s,t}.\end{aligned}$$

In this augmented specification, portfolio recompositions of routine households which have experienced a layoff in the current year are captured by the coefficient β of the triple interaction term. If our results are principally driven by the subset of households who experienced unemployment from firm labor-capital rebalancing, we should expect $\beta < 0$, while the coefficient β_1, which now estimates the effect on routine households who face heightened income uncertainty but have not observed any actual wage loss, should not be significantly different from zero. Results in Table 5 indicate that this is not the case, and our results are mainly driven

TABLE 5

Robustness: Role of Realized Unemployment

Table 5 shows the regressions of annual changes in stock share of liquid wealth on the dummy variable for routine household, annual change in state-level investment tax exemption limits, an indicator of whether the household experienced an increase in the number of unemployed members, and their respective interaction terms. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth and wage income. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: $\Delta\text{Stock Share}_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI × Unemployment	−1.709	0.619	0.228	−0.122
	(1.036)	(0.788)	(0.800)	(0.767)
ΔLimit × HHHighRTI	0.400	−0.811***	−1.256***	−1.383***
	(0.366)	(0.301)	(0.299)	(0.300)
HHHighRTI × Unemployment	0.980	−0.387	−0.017	0.177
	(0.855)	(0.671)	(0.614)	(0.615)
ΔLimit × Unemployment	0.682	−0.428	−0.215	0.728
	(0.811)	(0.685)	(0.839)	(0.829)
HHHighRTI	0.277	0.135	0.463	0.375
	(0.247)	(0.272)	(0.436)	(0.457)
Unemployment	−0.550	0.106	−0.219	−0.624
	(0.547)	(0.441)	(0.616)	(0.636)
No. of obs.	79,915	77,878	77,878	77,878
R^2	0.05	0.05	0.06	0.07
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

by a much larger subset of routine households that did not experience a layoff incidence.

While our estimates suggest similar financial de-risking by routine households with or without recent unemployment shocks, we provide evidence that the underlying mechanisms at play across these two groups of respondents vary. For instance, households who receive a negative income shock stemming from recent unemployment may draw down on their savings in order to smooth consumption.[20] In turn, this decline in liquid wealth exacerbates future liquidity constraints, potentially lowering stock market participation (see Chetty et al. (2017), Calvet, Campbell, and Sodini (2009)). However, note that the uncertainty associated with layoff has just been resolved for these workers.[21] Thus, the portfolio reallocation of these households is more likely driven by the income shock and less likely driven by an increase in background risk associated with a future layoff event which is the primary focus of our paper. On the other hand, households that do not face an immediate unemployment realization have a lesser urgency to divert liquid or illiquid assets toward current consumption. However, the precautionary savings motive may still induce these households to divert wealth from risky toward safer investments (Carroll (1994)).

To examine the above argument, we study if routine households experiencing realized unemployment exhibit different saving draw-down behavior compared to other routine households. We re-estimate equation (3) using the change in liquid wealth as the outcome variable. Results in Appendix Table A10 show that households experiencing unemployment reduce their liquid wealth by 8.1% over a 4-year period whereas still-employed households do not exhibit such behavior. These results suggest that any reduction in stock share of unemployed household is likely driven by liquidity constraints of such households. More importantly, these results highlight that portfolio rebalancing that we find among still-employed routine households is driven by background risk associated with future layoffs and not by the incidence of actual layoffs.

2. Role of Unemployment Insurance

To further isolate the causal effect of income risk channel on household portfolio choice, we exploit the variation in state-level UI programs. State UI benefits vary both in terms of the maximum amount and duration and this variation creates heterogeneity in income losses conditional on unemployment. Ceteris paribus, routine workers in states with higher UI benefits face a lower labor income risk from an increase in Section 179 limits. This difference in background risk, in turn, may translate into difference in portfolio allocations of routine households across states.

To test this hypothesis, we employ a version of equation (2) which includes interactions between the routine household dummy, the increase in Section 179

[20]Stevens (1997) document an annual household income drop of 7%–8% following new unemployments. Hurst and Stafford (2004) provide evidence of home refinancing and equity extraction as one channel for consumption smoothing.

[21]These households may still be uncertain about the length of their unemployment spell, but we abstract away from those factors as the employment spell may be driven by job search effort, morale, and other factors that are beyond the scope of our study.

limits, and the maximum level of state-level UI benefits. We follow the methodology of Hsu, Matsa, and Melzer (2018) and Agrawal and Matsa (2013), and measure the generosity of states' UI program as the product of maximum weekly benefit amount and the maximum benefit duration in weeks. The coefficient of interest is the triple interaction term, which captures the difference between average responses of routine households residing in states with high levels of UI benefits to state-level changes in Section 179 limits.

The results are presented in Table 6. The coefficient of $\Delta\text{Limit}_{s,t-1\rightarrow t} \times \text{HHHighRTI}_{h,s,t-1}$ is negative, which indicates that routine households in states with no UI benefits would drastically reduce their stock share in response to the policy shock. Concomitantly, the coefficient of the triple interaction term $\Delta\text{Limit}_{s,t-1\rightarrow t} \times \text{HHHighRTI}_{h,s,t-1} \times \text{MaxBenefit}_{s,t-1}$ is positive, indicating a lower response to the policy shock by routine households with access to more generous UI benefits. These results are consistent with our argument that the background risk of losing employment is the key channel driving our results.

Similar to external insurance, households with accumulated financial and non-financial assets can self-insure themselves against unanticipated adverse shocks through private channels. In other words, such internal financial markets can act as a buffer stock against unanticipated income shocks. While we control for household wealth in all our regressions, we now examine if our main channel varies across wealthy and poor households. We define wealthy (poor) households as those having above (below) median wealth level. Appendix Table A11 presents the results. We do not find evidence that wealthy and poor households respond differently to

TABLE 6

Robustness: Role of Unemployment Insurance

Table 6 shows the regressions of annual changes in stock share of liquid wealth on the dummy variable for routine household, annual change in state-level investment tax exemption limits, maximum UI benefits (Max Benefit) at the state level, and their respective interaction terms. Maximum UI benefits are the product of maximum weekly benefit amount and maximum benefit duration. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: ΔStock Share$_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI × MaxBenefit	−0.089	0.232	0.354**	0.413***
	(0.178)	(0.146)	(0.148)	(0.150)
ΔLimit × HHHighRTI	1.087	−3.057*	−4.633***	−5.363***
	(2.018)	(1.686)	(1.665)	(1.670)
ΔLimit × MaxBenefit	−0.250***	−0.155*	−0.525***	−0.658***
	(0.083)	(0.088)	(0.136)	(0.155)
HH High RTI	2.262**	1.551*	5.289***	6.545***
	(0.922)	(0.797)	(1.208)	(1.353)
No. of obs.	65,014	76,210	76,210	76,210
R^2	0.21	0.05	0.06	0.07
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

TABLE 7

Robustness: Role of House Price Fluctuations

Table 7 shows the regressions of annual changes in stock shares of liquid wealth on the dummy variable for routine households, annual change in state-level investment tax exemption limits implemented during the past year, their interaction, and a set of controls. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level. Each column corresponds to a sub-sample of data mentioned in the last row of the table. *** indicates significance at the 1% level.

	Dependent Variable: ΔStock Share$_{h,s,t\rightarrow t+2}$				
	1	2	3	4	5
ΔLimit × HH High RTI	−1.223***	−1.831***	−1.419***	−1.359***	−1.374***
	(0.355)	(0.259)	(0.429)	(0.321)	(0.336)
HH High RTI	0.507	0.482	0.600	0.633	0.656
	(0.442)	(0.453)	(0.518)	(0.490)	(0.479)
No. of obs.	69,699	64,963	50,137	64,540	65,334
R^2	0.08	0.08	0.09	0.08	0.08
$\overline{y}$	−5.16	−5.16	−5.16	−5.16	−5.16
State–year FE	Yes	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes
Robustness	$V(\Delta HP)$ <median	$V(\Delta HP)$ <P25	LTV ratios <0.80	Except sand states	Except NY, CA

changes in state depreciation limits. Thus, it seems that the background risk of losing one's employment is important for households across the wealth spectrum.

3. Role of House Price Fluctuations

Unsurprisingly, the largest changes in state and federal depreciation allowances coincide with a period of large house price swings during the housing bubble burst and the global financial crisis. While we control for state-year shocks in our main regressions, it does not preclude the possibility of within-state differential price swings from driving our results. For instance, within-state geographical clustering of routine and non-routine households may expose them to differential house price shocks.

Results presented in Table 7 provide evidence that the possibility of house prices driving our key estimates is small. In columns 1 and 2, we focus on sub-samples where the within-state variance in house prices lies below the median, or in the bottom quartile. Such states experienced similar trends in house prices across different zip codes. Our results are robust to this analysis. Next, we analyze the robustness of our results to the mortgage channel (Chetty et al. (2017)). In column 3, we focus on a sub-sample of routine and non-routine households with relatively safe mortgage debt.[22] Our results remain qualitatively similar to the baseline specification. Finally, our results are also robust to the exclusion of the sand states (California, Nevada, Arizona, and Florida), that is, states with the largest house price swings (column 4). Finally, our results are also robust to the exclusion of the financial centers—New York and California (column 5).

[22]Federal Housing Finance Association (FHFA) characterizes mortgages with loan-to-value ratios less than 0.80 as safe.

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

4. Role of Firm-Level Risk

Another potential channel that can explain our results is firm-level risk. Specifically, increased depreciation benefits accruing to firms may incentivize them to pursue a higher risk-return investment strategy. For instance, firms may increase investments in labor replacing technologies like robots. If routine workers are more exposed to such firm risks given job switching frictions, they might offset them by de-risking their financial portfolios. In other words, our portfolio rebalancing result might arise due to higher exposure to firm risk instead of heightened layoff likelihood. To address this concern, we proceed in two steps.

First, we examine if firm-level risk increased following changes in Section 179 limits. We use data from the Business Dynamics Statistics (BDS) database and proxy the increase in firm-level risk using firms' ex-post death rate within each state-industry-year cell. We then estimate the following specification:

$$\text{Firm Deaths}_{j,s,t\rightarrow t+k} = \beta \Delta \text{Limit}_{s,t-1\rightarrow t} + \phi_s + \nu_j + \delta_t + \epsilon_{j,s,t}, \tag{4}$$

where $\text{Firm Deaths}_{j,s,t\rightarrow t+k}$ is the total number of firm deaths in industry j and state s from year t and $t+k$ normalized by the total number of firms in year $t-1$. We include state, industry, and year fixed effects (ϕ_s, ν_j, and δ_t, respectively) to absorb average differences across these groups and cluster standard errors at the state level. Appendix Table A12 shows that the death rate among firms declined after the policy shock. While the ex-post death rate is an imperfect proxy for ex-ante risk-taking by firms, these results suggest that the tax benefits of Section 179 lowered the risk of firm failure and are in line with the existing literature that shows the positive impact of Section 179 on firm investments and growth.

Second, we address the concern that while firm-level risk declined for an average firm, it is plausible that certain firms took on more risks and their workers are driving our main results. To address this concern, we calculate the death rate of firms over a 5-year horizon and divide industries into groups with above and below-median values of death rates within each state-year combination. We then estimate a triple-difference specification where we regress the annual change in stock share on the dummy variable for routine household, annual change in state-level investment tax exemption limits, an indicator of whether the household head worked in an industry with more ex-post deaths, and their interaction terms. We include the same set of controls as in our baseline specification and include state-by-year fixed effects and cluster standard errors at the state level.

Appendix Table A13 reports the results of this regression. We offer two key observations. First, the baseline interaction between routine household indicator (HH High RTI) and ΔLimit is negative and significant, implying that our results are not solely driven by heightened firm risk post state-level depreciation limit changes. Second, routine workers employed in industries with high ex-post death rate after the policy shock exhibit a 1.9% lower reduction in stock share relative to their counterparts in other industries over a 4-year horizon. Thus, it is unlikely that our results are driven by workers' concern about the risk-return profile of their firms. Overall, these two results highlight that our portfolio rebalancing result is driven by the background risk channel and is not due to workers' response to an increase in firm-level risk.

5. Comparisons with Section 168 Accelerated Depreciation Policy

Finally, we assess the robustness of our results to the inclusion of alternative tax incentive policies. A key policy in this regard is the accelerated bonus depreciation, detailed under Section 168(k) of the IRS tax code, which allows firms to deduct, from their annual taxable income, a "bonus" percentage of the capital expenditure cost in the year of purchase. Similar to the accelerated depreciation under Section 179 that we study in the paper, this system increases firm's profits by increasing the depreciation tax shield.

Even though these accelerated depreciation policies have similar objectives, there are key differences in their structure. Most importantly, the use of Section 179 is subject to dollar limitations, guided by the maximum depreciation allowance and phaseout thresholds, and an income limitation which bars firms from claiming an allowance greater than their taxable income. These restrictions limit the types of firms which can benefit from Section 179, essentially restricting the eligibility criteria to small- and medium-sized firms. Section 168, on the other hand, does not impose any such restriction, making it more appealing to large corporations. As shown in Zwick and Mahon (2017) and Curtis, Garrett, Ohrn, Roberts, and Serrato (2021), firms that have assets with a longer depreciation schedule tend to benefit the most from a higher Section 168 bonus rate.

Section 168 limits have also changed repeatedly throughout our sample period.[23] To show that our main results are not driven by changes in Section 168 limits, we show that routine households employed in small firms exhibit stronger portfolio adjustments in response to our policy shocks compared to routine households employed in larger companies. Since we do not observe firm sizes directly in the SIPP data, we use the Survey of U.S. Businesses (SUSB) data from the Census Bureau to create a household-level exposure to small firms. More specifically, we calculate the fraction f_{ijst} of individuals employed in small firms with 100 employees or less, relative to all employees in industry j in state s at time t. A higher (smaller) value of f_{ijst} thus implies that an individual i is more (less) likely to be employed in a small firm. We then aggregate this measure at the household level by

$$f_{h,s,t} = \sum_{i \in h} f_{ijst} \frac{w_{ijst}}{\sum_{i \in h} w_{ijst}},$$

where we use individual earnings as weights w_{ijst}. Finally, we discretize the household exposure to small firms by using an indicator variable Small Firm Share$_{hst}$ which assumes a value of 1 if $f_{h,s,t}$ is greater than the median value at (state, year) level.

To test whether firm size has a material impact on household portfolio adjustments following changes in Section 179 depreciation limits, we interact the key variables in equation (2) with the Small Firm dummy. The results of this triple difference-in-difference test are presented in Appendix Table A14. The triple interaction term which estimates the effect on routine households with an above-

[23]Appendix Figure A1 shows the timeline of federal (black solid line) and state level (gray dots) Section 168 bonus schedule in the black solid line.

median exposure to small firms is strongly negative, implying a strong response of such households to changes in Section 179 limits. For instance, routine households employed in small firms reduce their stock share of liquid wealth by 1.45 percentage points in 2 years following a \$100,000 increase in Section 179 limits. This represents a 25% decline in wealth allocated to stocks and mutual funds, as a fraction of total liquid wealth. Routine households with a below-median exposure to small firms, on the other hand reduce their stock holdings by 0.52 percentage points. Thus, the response of routine households employed in small firms is almost 3 times as large as compared to that of routine households employed in large firms.

As an additional robustness test, we run a horse-race specification between Sections 179 and 168. Specifically, we re-estimate our baseline specification after including both Section 168 and 179 limit changes together as independent variables. Appendix Table A15 shows that the coefficients of interest in this horse-race specification are very similar to those in our baseline specification. These results suggest that routine households respond to state-level changes in Section 179 depreciation policies by adjusting their stock holdings, but not to changes in Section 168 depreciation adoption rules. Overall, these robustness tests show that our main results are not driven by the changes in Section 168 policy limits over our sample period.

D. Portfolio Returns and Liquid Wealth Accumulation

We next provide evidence that portfolio reallocations due to layoff risk contribute to lower levels of liquid wealth accumulation of routine households. SIPP provides detailed information on household portfolios. We decompose household liquid wealth into four categories: stock and mutual fund holdings, government bonds, interest-bearing accounts in banks, and money. Since we do not observe individual components of these allocations, we approximate household stock returns $\left(r^S_{t\to t+k}\right)$ using Standard and Poor's S&P 500 value-weighted index excluding distributions; bond returns $\left(r^M_{t\to t+k}\right)$ using municipal bond rate, and return on bank interest accounts $\left(r^f_{t\to t+k}\right)$ using the Fed funds rate. Finally, we assume that checking accounts do not accrue any interest. We calculate the total return on household liquid wealth $\left(R^P_{h,s,t}\right)$ as:

$$R^P_{h,s,t} = \frac{\text{Stock}_{h,s,t-1}}{\text{LW}_{h,s,t-1}} \times \left(r^S_t + \delta^S_t\right) + \frac{\text{Bond}_{h,s,t-1}}{\text{LW}_{h,s,t-1}} \times r^M_t + \frac{\text{Bank}_{h,s,t-1}}{\text{LW}_{h,s,t-1}} \times r^f_t,$$

where δ^S_t is total dividends collected by the households from their stock holdings, and $\text{LW}_{h,s,t}$ is the total liquid wealth of the household given by:

$$\text{LW}_{h,s,t} = \text{Stock}_{h,s,t} + \text{Bond}_{h,s,t} + \text{Bank}_{h,s,t} + \text{Money}_{h,s,t}$$

Motivated by the results on portfolio choice, we investigate whether Section 179 policy shocks affect portfolio return of routine and non-routine households using the following specification:

TABLE 8

Effect of Section 179 on Liquid Portfolio Return

Table 8 shows results from equation (5). The dependent variable is the change in portfolio return R^P between periods t and $t+k$. To calculate return from the liquid assets portfolio, we use return on the S&P index to proxy stock return, municipal and treasury yields to calculate bond returns, and fed funds rate to proxy return on the savings account. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level. *** and ** indicate significance at the 1% and 5% levels, respectively.

	Dependent Variable: $\Delta R^P_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	0.044**	−0.051***	−0.059***	−0.067***
	(0.022)	(0.017)	(0.016)	(0.016)
HH High RTI	−0.000	0.027	0.031	0.038
	(0.021)	(0.018)	(0.023)	(0.023)
No. of obs.	91,223	88,204	88,204	88,204
R^2	0.04	0.04	0.04	0.04
$\bar{y}$	0.02	0.02	−0.04	−0.06
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

$$(5)\qquad \Delta R^P_{h,s,t\rightarrow t+k} = \beta \times \text{HHHighRTI}_{h,s,t-1} \times \Delta\text{Limit}_{s,t-1\rightarrow t} + \beta_1 \times \text{HHHighRTI}_{h,s,t-1} + \psi_{s,t} + \gamma \mathbf{X}_{h,s,t} + \rho \Delta W_{h,s,t} + \epsilon_{h,s,t},$$

where the dependent variable is the total change in the return on liquid wealth of household h in state s between years t and $t+k$.

The results of equation (5) are presented in Table 8.[24] Column 2 shows that in the year following a \$100,000 increase in state depreciation limits, the portfolio return of routine households decreases by 5.1 basis points per month or 61.2 basis points annually compared to a non-routine household in the same state. More importantly, we do not observe any return reversal over the entire duration for which SIPP samples a given household. For example, the monthly return differential increases to 6.7 basis points 3 years after the state limit change. This is consistent with the persistent decline in stock shares of routine households and the fact that stocks earn a higher return rate on average compared to the other components of liquid wealth. Overall, these results suggest that increase in depreciation limits and the consequent increase in layoff risk leads to conservative portfolio choice and lower average return for routine households.

To address the concern that our results may be driven by changes in the return structure of the financial assets themselves, and not by the portfolio reallocations of the households, we conduct a counterfactual robustness analysis. Let us consider the jump in Section 179 limits of \$250,000 in 2009 as an illustrative example. Consider a routine household h residing in state s, who experiences a heightened income uncertainty shock induced by this limit change. Our portfolio results compare the rate of return earned in 2009 with the corresponding portfolio return

[24]In the main specification, we omit industry information to maintain parity with previous specifications. As before, we confirm that our primary estimates are robust to the inclusion of state-industry-year fixed effects in Appendix Table A16.

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

earned in 2008. On an average, the household earns approximately $2.5 \times 61.2 = 153$ basis points lower return from financial holdings in 2009 compared to non-routine households residing in the same state. We now ask how much return would the household have generated in 2009 if they did not change their asset allocations from 2008, before the shock hit. That is, we use their portfolio weights from 2008 and calculate a counterfactual rate of return $\widehat{R}^P_{h,s,t\to t+k}$. We operationalize this strategy by considering the weights $\widehat{W}_{h,s,t} := \left\{\widehat{s},\widehat{b},\widehat{x},\widehat{m}\right\}$ on stocks (s), bonds (b), interest-bearing accounts (x), and checking accounts (m) observed prior to the shock as a benchmark, and calculate the counterfactual portfolio return as

$$\widehat{R}^P_{h,s,t} = \frac{\widehat{s}_{h,s,t-1}}{\mathrm{LW}_{h,s,t-1}} \times \left(r^S_t + \delta^S_t\right) + \frac{\widehat{b}_{h,s,t-1}}{\mathrm{LW}_{h,s,t-1}} \times r^M_t + \frac{\widehat{x}_{h,s,t-1}}{\mathrm{LW}_{h,s,t-1}} \times r^f_t$$

We then define the change in household portfolio return relative to the counterfactual as

$$\Delta R^P_{h,s,t\to t+k} = R^P_{h,s,t\to t+k} - \widehat{R}^P_{h,s,t\to t+k}.$$

Note that in calculating the return differential relative to the counterfactual benchmark, we have eliminated the return dynamics of the financial instruments themselves, since both the original return and the counterfactual baseline returns are calculated using the same stock, bond, and municipal bond rates. This differential directly stems from the active rebalancing decisions of households following the Section 179 limit changes.

We rerun equation (5) using this alternate return definition. The results of this counterfactual exercise are presented in Appendix Table A17. The estimates we uncover are qualitatively similar to those presented before, further underscoring the effects of active portfolio rebalancing on returns generated.

Finally, one may have concerns with the construction of our portfolio return measure. For example, consider a household that shifts away from safe stocks toward riskier high-yield bonds. For such a household, a reduction in stock share may not necessarily imply a lower growth in liquid wealth. More generally, the lack of detailed security-level information at the household level can introduce noise in our return variable. To address this concern, we directly measure the annual growth of liquid wealth and see how it varies across routine and non-routine households after the policy shock. Appendix Table A18 shows that the liquid wealth of routine households grows at a lower rate than that of non-routine households. These results provide direct evidence of the policy's effect on liquid wealth accumulation process and assuage the concerns that our results on portfolio returns are not driven by the way we construct the return measure.

IV. Discussion

A. Occupation Switching

Routine workers experiencing higher layoffs after their states adopt higher depreciation limits suggests that they face frictions in switching to non-routine occupations in response to such policy shocks. Since we observe occupational switching in the SIPP data, we now present direct evidence of the frictions faced by workers when switching between routine and non-routine occupational groups. The average likelihood of an individual switching between routine and non-routine occupational groups in our sample is 5.38%.[25] The majority of these switches happen between high-routine and low-routine *manual* occupations. For example, a person previously employed as a janitor gets a job as a store clerk. Another example that frequently appears is when an individual, after losing their job, gets employed in a manual occupation (e.g., burger flipper) and then switches back to their previous occupation. Such a transition between high-RTI to low-RTI manual occupations may not mitigate income risk stemming from state-level depreciation tax credit changes to a large extent. However, we perform a formal test to show that households are unlikely to escape the income risk by quickly switching to non-routine occupations.

To perform this analysis, we define intra-group mobility as a dummy that takes a value of 1 if an individual switches across routine and non-routine groups in a given year, and 0 otherwise. To understand the dynamics of occupational mobility around Section 179 changes, we consider a difference-in-difference model with the mobility measure as the dependent variable and the interaction between state-level changes of Section 179 and routine indicator as the key independent variable. We control for respondents' demographic characteristics, as well as state-industry-year fixed effects.

Results from this specification are presented in Table 9. Each column presents the results from a specification where the dependent variable represents a switch between high- and low-routine occupations in year $t+k$ in response to state increases in depreciation tax limits in year t. The absence of any significant job mobility after the policy shock confirms our prior that individuals in our sample cannot abstract away from the layoff risk by switching occupations.

B. Retraining

While switching occupations to non-routine groups might be difficult, routine workers can try to upskill themselves in response to the possibility of a future layoff event. SIPP includes questions on recent training programs individuals invested in during the past year. To investigate the effect of human capital decline due to Section 179 related layoffs on investment in training programs, we estimate a version of our equation (1) with the skills-related training dummy as the dependent variable. It assumes a value of 1 if the respondent attended any training session during the past year with the aim of learning new skills which may improve their employment opportunities in the future, but are not necessarily linked to their current employment and occupation. The results from this regression are presented in Table 10. We do not find any significant increase in skill

[25]A major sub-literature in labor and macro-economics studies the evolution of occupational mobility in the United States over the last 5 decades. The consistent finding of this literature is that the degree of switching between occupations has increased over time. See, for example, Kambourov and Manovskii (2008), (2009a), (2009b), Xiong (2008), and Gruber and Madrian (2002). However, typically, occupational mobility is defined as switching between 3 digit occupation levels. In this paper, we consider a more aggregate measure of mobility between low and high routine occupations.

TABLE 9

Switch Between Routine and Non-Routine Occupations

Table 9 summarizes the results from linear regression of respondent-level intra-group job switching dummy on the routine worker indicator, annual change in state-level investment tax benefits, their interaction, and a set of demographic controls. The dependent variable is a dummy which takes a value of 1 if the respondent switches to a low (high) RTI occupation from a high (low) RTI occupation, and 0 otherwise. Demographic controls include individual's education, age, sex, race, marital and home-ownership status, and employer size. We include state-by-industry-by-year fixed effects and cluster standard errors at state level. *** indicates significance at the 1% level.

	Dependent Variable: $\text{Switch}_{i,s,t\rightarrow t+k}$			
	$k = -1$	$k = 1$	$k = 2$	$k = 3$
	1	2	3	4
ΔLimit × High RTI	−0.336***	0.126	0.237	0.126
	(0.124)	(0.161)	(0.189)	(0.228)
High RTI	5.435***	5.357***	7.620***	8.559***
	(0.174)	(0.166)	(0.230)	(0.279)
No. of obs.	326,767	326,767	326,767	326,767
R^2	0.18	0.18	0.19	0.19
$\bar{y}$	3.72	3.72	5.75	6.83
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes

TABLE 10

Section 179 and Retraining

Table 10 shows results from regression where the dependent variable is the probability that an individual enrolls in a skills-based training program which enhances the chance of future employment. The corresponding question in SIPP questionnaire states that the specific purpose of this question is to gauge the respondents' willingness to join various training programs which enhances their skill sets and is not directly linked to their current occupation. The independent variables are indicator for routine worker, change in state-level depreciation limit, and their interaction. Demographic controls include individual's education, age, sex, race, marital and home-ownership status, and employer size. We include state-by-year fixed effects and cluster standard errors at state level. * indicates significance at the 10% level.

	Dependent Variable: $\text{Skill Training}_{i,s,t\rightarrow t+k}$			
	$k = 0$	$k = 1$	$k = 2$	$k = 3$
	1	2	3	4
ΔLimit × High RTI	0.003	0.012	0.002	−0.004
	(0.010)	(0.009)	(0.009)	(0.010)
High RTI	−0.037*	−0.040*	−0.030	−0.023
	(0.020)	(0.020)	(0.021)	(0.023)
No. of obs.	14,645	14,645	12,923	8,698
R^2	0.09	0.09	0.08	0.08
$\bar{y}$	0.77	0.77	0.78	0.79
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes

training among routine workers. This suggests that a likely reason behind the switching friction is the large cost of human capital investment which the workers are unable or unwilling to bear. However, we note that SIPP data on skill training are very sparsely populated. Thus, these findings may be driven by the lack of statistical power in the data.

C. Lower Lifetime Earnings

So far, we have shown that an increase in background risk represented by the probability of layoff affects the portfolio choice of routine workers. However, background risk is not the only channel through which labor income risk can affect investment decisions. If labor income cannot be capitalized, but it is riskless, then the optimal allocation in risky assets can decrease with the decline in expected lifetime earnings (Merton (1971)). In other words, it is possible that in addition to increasing the background risk, an increase in depreciation limits lowers the average path of future income for routine workers (Calvet and Sodini (2014)).

While such a mechanism is not antithetical to our paper's primary idea, it does imply a separate set of policy recommendations. For example, if state-level tax policy increases the background risk of workers, then more generous insurance through government-sponsored schemes would moderate the impact on stock market participation as we documented in Section III.C. Policies like increasing the unemployment benefits and unemployment duration that lower the loss given unemployment would directly lower the variance of the income stream for susceptible workers. On the other hand, if technology lowers the average wage a routine worker can earn in a lifetime, then the optimal policy should not be conditional on unemployment. Rather, facilitating investment in human capital either through government-sponsored training programs or by requiring the firms to educate and upskill their workers (as in the case of the CHIPS Act) would be beneficial.

It is natural to imagine that when workers' human capital becomes partially or fully redundant, they might rationally anticipate a lower average return on human capital in terms of lower future wage growth. While it is difficult to quantify the decline in the present value of a routine worker's future wages, we explore whether the wage growth of routine workers was lower than that of non-routine workers in the 4 years following the increase in Section 179 limits at the state level.

Results presented in Table 11 indicate that there is no significant decline in the wages of employed routine workers in the 3-year period following the policy shock. The result may reflect the effect of sticky wages, which do not change often. Second, the wage growth in the reported data is noisy, possibly leading to statistical insignificance. Third, while we follow Aladangady (2017) and assume that workers' expectations about future wages would be roughly consistent with

TABLE 11

Effect on Wage Income

Table 11 shows the regressions of annual changes in total household wage income on the routine worker indicator, annual change in state-level investment tax exemption limits implemented during the past year, their interaction, and a set of controls. Demographic controls include individual's education, age, sex, race, marital and home-ownership status, and employer size. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: $\Delta \text{WageIncome}_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × High RTI	0.000	−0.016	−0.019	−0.019
	(0.007)	(0.013)	(0.016)	(0.015)
High RTI	−0.022***	−0.019**	−0.010	−0.003
	(0.008)	(0.008)	(0.010)	(0.012)
No. of obs.	204,926	204,926	204,926	204,926
R^2	0.01	0.01	0.02	0.02
$\bar{y}$	−.02	−.02	−.05	−.06
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes

realized wage growth in years following the policy shock, the short span of panel data limits our interpretation of these results. Due to these reasons, we do not take a stand that this channel is not operating independently of our proposed channel. However, the results presented in this section provide strong evidence in favor of the background risk channel.

V. Conclusion

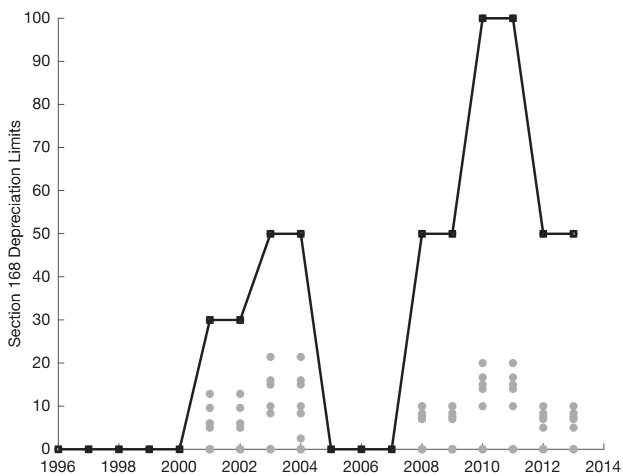
As a large literature in labor economics argues, labor-substituting capital has heterogeneous effects on routine and non-routine workers. We explore firm investment driven by government stimulus and explore if workers respond to these policies by modifying their asset holdings. Specifically, we show that the workers who face heightened unemployment risk due to capital investment reduce the risk in their asset portfolio by reducing their exposure to the equity market. We argue that such conservative portfolio choice by routine workers depresses their wealth accumulation process and may have important implications for aggregate stock market participation and wealth inequality across different types of workers.

Appendix. Additional Tables and Figures

FIGURE A1

Changes in Section 168 Depreciation Limits

Figure A1 plots Section 168 depreciation limits (in %). The black solid line plots the limits at the federal level, while the gray dots represent the limits for different states.



https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

TABLE A1

Variable Definition

Table A1 shows the definition of all key variables used in the empirical analysis.

Variable	Definition
Panel A. Individual-Level Employment Variables	
RTI Score	Routine task intensity score for each occupation
High RTI	Top tercile of the individual–level RTI score distribution
HH High RTI	Top tercile of the household–level RTI score distribution
Layoff	Indicator for worker on layoff during the SIPP reference period
Unemployment	Indicator for increase in the number of unemployed members in household
Switch	Indicator for worker switching from high RTI occupation to low RTI occupation (or vice versa)
Skill Training	Indicator for worker enrolling in a skills-based training program
Panel B. Policy Variables	
Section 179 Limit	State–level Section 179 deduction limit
ΔLimit/ΔLimit$_{179}$	Annual change in state–level Section 179 deduction limit
ΔLimit$_{168}$	Annual change in state–level Section 168 bonus depreciation rate
Panel C. Household Wealth Variables	
P(Stock Market Exit)	Indicator for household reducing stock holdings to 0
ΔStock Share	Annual change in the stock share of liquid wealth
ΔR^P	Annual change in the return on liquid wealth
ΔWage Income	Annual change in household wage income
ΔStock Wealth	Annual change in household stock wealth
ΔLiquid Wealth Share	Annual change in liquid share of total household wealth
ΔLog Liquid Wealth	Annual change in log of household liquid wealth
Panel D. Other Variables	
$V(\Delta HP)$	Variance in house prices at the state level
LTV ratios	Average loan–to–value ratio at the state level
Max Benefit	Product of maximum weekly UI benefit amount and the maximum UI benefit duration in weeks
Wealthy	Households with above–median total household wealth
Unemployment Rate	State–level unemployment rate
Per Capita GDP	State–level per capita GDP
State Wage Level	State–level average wage rate
State Max Unemployment Benefits	State–level average UI Max Benefit
Small Firm Share	Fraction of workers employed in firms with 100 or fewer employees
Firm Deaths	Death rate of firms at the industry–state–year level
More Firm Deaths	Indicator for above–median firm death rate over a 5–year horizon

TABLE A2

Examples of Routine and Non-Routine Occupations

Table A2 shows examples of routine and non-routine occupations within each wage decile.

Income Decile	Median Annual Wage ($)	Occupation Examples	
		Routine	Non-Routine
1	17,189	Cashiers, receptionists	Dishwashers
2	21,759	Retail salespersons	Cooks
3	25,877	Tellers	Medical assistants
4	29,929	Data entry keyers	Pharmacy technicians
5	34,281	Bookkeeping and accounting clerks	Retail store supervisors
6	39,137	Auto mechanics, HR assistants	Editors
7	45,255	Industrial machinery mechanics	Librarians, electricians
8	53,003	Postal service clerks	Accountants and auditors
9	64,285	Electrical and electronic technicians	Computer programmers, engineers
10	93,887	Administrative judges, avionics technicians	Managers

TABLE A3

Univariate Analysis

Table A3 shows univariate analysis of the key variables used in the empirical analysis across routine and non-routine occupations.

	Routine	Non-Routine	Difference
Panel A. Demographic Variables			
RTI Score	1.27	−0.63	1.90***
Age (years)	37.64	39.41	−1.77***
Male	0.49	0.50	−0.01***
Female	0.51	0.50	0.01***
White	0.81	0.82	−0.01***
Black	0.13	0.11	0.02***
Asian or Pacific Islander	0.02	0.03	−0.01***
American Indian, Aleut, or Eskimo	0.04	0.03	0.00***
Less than HS	0.15	0.10	0.05***
High school or GED	0.39	0.24	0.14***
Some college	0.29	0.24	0.05***
College graduate	0.07	0.08	−0.01***
Graduate degree	0.10	0.33	−0.24***
Panel B. Employment and Income			
Layoff (%)	5.59	3.96	1.64***
Switch (%)	8.73	3.86	4.87***
Wage income ($, monthly)	1,910.16	2,992.88	−1,082.71***
Dividend income ($, monthly)	6.39	14.85	−8.46***
Interest income ($, monthly)	7.93	15.16	−7.24***
Total household income ($, monthly)	4,966.07	6,355.95	−1,389.88***
Employer: < 100 employees	0.06	0.07	−0.01***
Employer: > = 100 employees	0.54	0.53	0.01***
Panel C. Household Wealth ($, 000)			
Total wealth	212.78	270.46	−57.67***
Retirement wealth	74.55	132.18	−57.63***
Durable wealth	90.23	101.39	−11.16***
Illiquid wealth	23.88	15.25	8.63***
Liquid wealth	24.12	21.63	2.49***
Total debt	36.92	61.43	−24.51***
Panel D. Household Portfolio (%)			
Stock market participation	18.27	23.40	−5.13***
Stock share	14.19	16.44	−2.25***
Bond share	4.23	4.63	−0.40***
Money share	22.16	20.27	1.89***
Interest earning share	80.53	78.25	2.28***
Return on liquid wealth	1.90	2.12	−0.22***

TABLE A4

Section 179 and State Economy

Table A4 shows the results from regressing state-level deduction limits on various state-level economic indicators. We include state and year fixed effects and cluster standard errors at state level.

	Log Section 179 Limit				
	1	2	3	4	5
Unemployment rate (%)	−0.077				−0.073
	(0.047)				(0.048)
Per capita GDP (log)		1.014			1.104
		(0.942)			(0.866)
State wage level			−0.023		−0.019
			(0.035)		(0.035)
State max unemployment benefits				−0.083	−0.087
				(0.073)	(0.076)
No. of obs.	920	920	920	920	920
R^2	0.800	0.798	0.797	0.801	0.807
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

TABLE A5

Layoffs, Routine Jobs, and Section 179: State-Industry-Year FE

Table A5 summarizes the results from linear probability regressions of layoffs on dummy variable for high RTI occupation (top tercile of RTI score), annual change in state-level investment tax benefits (Section 179 of the IRS tax code), their interaction, and a set of controls. Demographic control variables include individual's education, age, sex, race, marital and home-ownership status, and employer size. We use state-by-industry-by-year fixed effects and cluster standard errors at the state level.

	Dependent Variable: $\text{Layoff}_{i,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × High RTI	−0.005	0.276	0.410**	0.393**
	(0.126)	(0.172)	(0.181)	(0.169)
High RTI	1.005***	0.954***	1.259***	1.323***
	(0.076)	(0.084)	(0.129)	(0.139)
No. of obs.	326,767	326,767	326,767	326,767
R^2	0.19	0.19	0.19	0.19
$\bar{y}$	4.08	4.08	5.67	6.3
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes

TABLE A6

Changes in Household Stock Share: Routine and Non-Routine Households

Table A6 summarizes the results from a regression on annual change in stock share of liquid wealth on dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. The interaction term captures the additional effect of investment tax incentives (Section 179 of the IRS tax code) on the change in stock share for routine households. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state and year fixed effects to capture the differential impact on routine and non-routine households. We cluster standard errors at state level.

	Dependent Variable: $\Delta\text{Stock Share}_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit	0.121	−0.292	−0.033	0.039
	(0.329)	(0.305)	(0.309)	(0.318)
ΔLimit × HH High RTI	0.250	−0.794**	−1.281***	−1.459***
	(0.330)	(0.307)	(0.295)	(0.297)
HH High RTI	0.459*	0.246	0.658	0.743*
	(0.240)	(0.239)	(0.392)	(0.413)
No. of obs.	79,753	77,723	77,723	77,723
R^2	0.04	0.04	0.05	0.07
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State and year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A7

Changes in Household Stock Share: State-Industry-Year FE

Table A7 summarizes the results from a regression on annual change in stock share of liquid wealth on dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-industry-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔStock Share$_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	−0.012	−0.724*	−1.228***	−1.464***
	(0.332)	(0.364)	(0.342)	(0.336)
HH High RTI	−0.069	−0.151	0.498	0.771
	(0.309)	(0.339)	(0.490)	(0.536)
No. of obs.	66,330	64,451	64,451	64,451
R^2	0.21	0.21	0.22	0.24
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A8

Changes in Household Stock Wealth

Table A8 summarizes the results from a regression on annual change in stock wealth (in $ thousand) on dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in wage income and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔStock Wealth$_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	0.271	−0.406	−0.782*	−0.838*
	(0.250)	(0.411)	(0.407)	(0.423)
HH High RTI	0.378	0.310	0.746*	0.725
	(0.267)	(0.275)	(0.378)	(0.434)
No. of obs.	94,786	94,786	94,786	94,786
R^2	0.01	0.01	0.02	0.02
$\bar{y}$	13.37	13.37	13.37	13.37
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

https://doi.org/10.1017/S0022109024000449 Published online by Cambridge University Press

TABLE A9

Changes in Liquid Wealth Share of Total Wealth

Table A9 summarizes the results from a regression on annual change in the liquid wealth share of total wealth on dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wage income and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔLiquidWealth Share$_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	0.001	−0.004*	−0.004*	−0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
HH High RTI	−0.000	0.001	0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.002)
No. of obs.	62,935	61,259	61,259	61,259
R^2	0.18	0.19	0.19	0.19
$\overline{y}$	0	0	0	0
State–year–industry FE	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes

TABLE A10

Liquid Wealth Changes Under Unemployment

Table A10 shows the regressions of annual changes in liquid wealth share of households on the dummy variable for routine household, annual change in state-level investment tax exemption limits, an indicator of whether the household experienced an increase in the number of unemployed members, and their respective interaction terms. We include demographic controls at the household level. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-industry-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔLiquidWealth Share$_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI × Unemployment	4.541**	−7.661***	−7.996***	−8.131***
	(1.961)	(2.425)	(2.577)	(2.635)
ΔLimit × HHHighRTI	−0.049	0.268	0.220	0.230
	(1.265)	(1.363)	(1.427)	(1.441)
HHHighRTI × Unemployment	−0.679	4.085**	4.768***	4.794**
	(1.680)	(1.650)	(1.759)	(1.804)
ΔLimit × Unemployment	−1.852	−0.996	−0.640	−0.331
	(2.031)	(1.558)	(2.605)	(2.689)
HHHighRTI	−1.051	−0.828	−0.622	−0.604
	(0.855)	(0.883)	(1.324)	(1.414)
Unemployment	0.333	0.209	−1.409	−1.502
	(1.737)	(1.234)	(1.825)	(1.885)
No. of obs.	78,870	78,870	78,870	78,870
R^2	0.22	0.22	0.21	0.21
$\overline{y}$	−10.49	−10.49	−13.52	−13.52
Test: ΔLiquid Wealth(Unemployed) = 0	1.24	−4.92***	−5.68***	−5.54***
T–Stat(linear comb)	0.48	−3.39	−2.87	−2.74
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A11

Role of Household Wealth

Table A11 shows the regressions of annual changes in stock share of liquid wealth on the dummy variable for routine household, annual change in state-level investment tax exemption limits, dummy variable for wealthy households, and their respective interaction terms. Wealthy households are ones with above-median level of wealth in a given year. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wage income and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔStock Share$_{h,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI × Wealthy	0.438	−0.465	−0.679	−0.726
	(0.585)	(0.532)	(0.548)	(0.568)
ΔLimit × HHHighRTI	−0.019	−0.262	−0.480*	−0.564**
	(0.409)	(0.234)	(0.250)	(0.271)
HHHighRTI × Wealthy	0.521	1.118***	2.606***	3.184***
	(0.380)	(0.311)	(0.346)	(0.372)
ΔLimit × Wealthy	0.220	0.862**	1.148	1.354*
	(0.456)	(0.417)	(0.686)	(0.762)
HH High RTI	0.222	−0.549*	−0.375	−0.468
	(0.411)	(0.287)	(0.492)	(0.595)
Wealthy	−1.769***	−4.464***	−7.557***	−8.747***
	(0.379)	(0.284)	(0.412)	(0.452)
No. of obs.	80,811	78,726	78,726	78,726
R^2	0.04	0.04	0.06	0.08
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A12

Firm Deaths After Section 179

Table A12 summarizes the results of the following specification:

$$\text{Firm Deaths}_{j,s,t\rightarrow t+k} = \beta \Delta \text{Limit}_{s,t-1\rightarrow t} + \phi_s + \nu_j + \delta_t + \epsilon_{j,s,t}$$

where FirmDeaths$_{j,s,t\rightarrow t+k}$ is the total number of firm deaths in industry j and state s from year t and $t+k$ normalized by the total number of firms in year $t-1$. We include state, industry, and year fixed effects (ϕ_s, ν_j, and δ_t, respectively) to absorb average differences across these groups and cluster standard errors at the state level.

	Dependent Variable: FirmDeaths$_{i,s,t\rightarrow t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit	−0.040	−0.040*	−0.064	−0.052
	(0.028)	(0.023)	(0.051)	(0.074)
Obs.	16,606	16,606	16,606	16,606
R^2	0.65	0.65	0.73	0.77
$\bar{y}$	7.38	7.38	14.72	22.02
State FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

TABLE A13

Role of Firm-Level Risk

Table A13 summarizes the results from regressing the annual change in stock share on on the dummy variable for routine household, annual change in state-level investment tax exemption limits, and an indicator of whether the household head worked in an industry with more ex-post deaths over a 5-year horizon along with their respective interaction terms. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔStock Share$_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI × More Firm Deaths	0.421	0.940	1.554**	1.902**
	(1.011)	(0.771)	(0.750)	(0.773)
ΔLimit × HHHighRTI	−0.015	−1.460*	−2.395***	−2.842***
	(0.961)	(0.800)	(0.776)	(0.796)
HHHighRTI × More Firm Deaths	−0.264	0.172	−0.181	−0.305
	(0.736)	(0.657)	(0.643)	(0.666)
ΔLimit × More Firm Deaths	−2.648***	−2.241***	−4.031***	−4.883***
	(0.519)	(0.535)	(0.787)	(0.926)
HHHighRTI	2.460***	1.865***	3.636***	4.378***
	(0.428)	(0.509)	(0.749)	(0.858)
More Firm Deaths	1.035***	0.816**	1.695***	2.077***
	(0.384)	(0.365)	(0.528)	(0.648)
No. of obs.	78,556	76,375	76,375	76,375
R^2	0.05	0.05	0.06	0.07
$\bar{y}$	−4.49	−4.49	−8.26	−9.82
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A14

Section 179 and Household Stock Share: Firm-Employment Heterogeneity

Table A14 shows the regressions of annual changes in stock share of liquid wealth of households on the dummy variable for routine household, annual change in state-level investment tax exemption limits, an indicator of whether the household was employed in a small firm, and their respective interaction terms. Since firm size is not directly observable in the survey data, we proxy for firm sizes by creating a household exposure measure to small firms. Using the Statistics of U.S. Business (SUSB) released by the Census Bureau, we calculate the share of firms employing 100 or less people, as a share of all firms at state-industry-year level. We then aggregate this measure at the household level, by taking an average, weighted by earnings, of all the working members in the household. Finally, we classify a household to be more exposed to small firm share if this weighted measure is higher than the median value for that state in that year. The Small Firm Share variable used in the table is an indicator if the household has an above-median exposure to small firms. We also include demographic controls at the household level. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wage income and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔStock Share$_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HHHighRTI × Small Firm Share	−0.155	−1.198**	−1.372***	−1.453***
	(0.420)	(0.470)	(0.470)	(0.473)
ΔLimit × HHHighRTI	0.342	−0.082	−0.418	−0.519
	(0.454)	(0.456)	(0.430)	(0.426)
HHHighRTI × Small Firm Share	−0.030	0.371	0.758**	1.047***
	(0.318)	(0.354)	(0.326)	(0.330)
ΔLimit × Small Firm Share	1.736***	1.496***	2.111***	2.358***
	(0.503)	(0.492)	(0.605)	(0.680)
HHHighRTI	−0.706	−0.837*	−0.869	−1.004
	(0.446)	(0.427)	(0.583)	(0.646)
Small Firm Share	−0.873*	−0.730*	−2.017***	−2.931***
	(0.434)	(0.382)	(0.561)	(0.597)
No. of obs.	79,915	77,878	77,878	77,878
R^2	0.05	0.05	0.06	0.07
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A15

Robustness: Controlling for Section 168 State Limit Changes

Table A15 shows the regressions of annual changes in stock share of liquid wealth on the dummy variable for routine household, annual change in state-level investment tax exemption limits, dummy variable for wealthy households, and their respective interaction terms. Wealthy households are ones with above-median level of wealth in a given year. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wage income and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: ΔStock Share$_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit$_{179}$ × HH High RTI	−0.384	−0.810**	−1.246***	−1.419***
	(0.366)	(0.353)	(0.351)	(0.342)
ΔLimit$_{168}$ × HH High RTI	−0.603	1.341	0.267	−0.176
	(1.263)	(1.659)	(1.201)	(1.274)
HH High RTI	0.365	0.259	0.627	0.902*
	(0.368)	(0.360)	(0.521)	(0.506)
No. of obs.	65,656	59,873	59,873	59,873
R^2	0.24	0.24	0.25	0.26
$\bar{y}$	−4.47	−4.47	−8.24	−9.81
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A16

Effect of Section 179 on Liquid Portfolio Return: State-Industry-Year FE

Table A16 shows results from equation (5). The dependent variable is the change in return of the portfolio R^P between periods t and $t+k$. To calculate return from the liquid assets portfolio, we use return on the S&P index to proxy stock return, municipal and treasury yields to calculate bond returns, and fed funds rate to proxy return on the savings account. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-industry-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: $\Delta R^P_{h,s,t \to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	0.054*	−0.057**	−0.064***	−0.068***
	(0.027)	(0.021)	(0.021)	(0.021)
HH High RTI	−0.012	0.022	0.016	0.011
	(0.026)	(0.023)	(0.030)	(0.031)
No. of obs.	76,920	74,074	74,074	74,074
R^2	0.21	0.21	0.22	0.21
$\bar{y}$	0.02	0.02	−0.04	−0.06
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A17

Liquid Portfolio Return Relative to Counterfactual

Table A17 shows results from equation (5). The dependent variable $\left(\Delta R^P_{h,s,t-1\to t}\right)$ is the difference between actual portfolio return $\left(R^P_{h,s,t\to t+k}\right)$ and the counterfactual return calculated using previous periods' portfolio weights $\left(\widehat{R}^P_{h,s,t\to t+k}\right)$. between periods t and $t+k$. To calculate return from the liquid assets portfolio, we use return on the S&P index to proxy stock return, municipal and treasury yields to calculate bond returns, and fed funds rate to proxy return on the savings account. The benchmark is created by considering household portfolio weights before the change in state Section 179 depreciation limits. The counterfactual return is then calculated as the return the household would have earned on the financial portfolio had they made no alterations to the relative allocations. Then the change in return, which we use as the dependent variable in these regressions, captures the return forwent due to households scaling back on the risky financial assets. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: $\Delta R^P_{h,s,t\to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	0.038*	−0.056***	−0.048***	−0.047***
	(0.019)	(0.019)	(0.018)	(0.017)
HH High RTI	0.026	0.031	0.001	−0.007
	(0.022)	(0.022)	(0.028)	(0.027)
No. of obs.	78,740	75,860	75,860	75,860
R^2	0.18	0.18	0.19	0.19
$\bar{y}$	−0.1	−0.1	−0.18	−0.2
State–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

TABLE A18

Household Wealth Growth Under Section 179

Table A18 summarizes the results from a regression on annual change in log of liquid wealth on dummy variable for routine household (top tercile of household-level RTI score), annual change in state-level investment tax benefits, their interaction, and a set of controls. Demographic controls include household head's education, age, sex, race, and marital status. Wealth controls include annual change in households' wealth, wage income, and number of unemployed members. We include state-by-year fixed effects and cluster standard errors at state level.

	Dependent Variable: $\Delta\text{Log Liquid Wealth}_{h,s,t\to t+k}$			
	$k=-1$	$k=1$	$k=2$	$k=3$
	1	2	3	4
ΔLimit × HH High RTI	−0.666	−7.485***	−6.326***	−6.181***
	(2.344)	(2.476)	(2.221)	(2.195)
HH High RTI	−2.790	−1.443	−2.698	−2.819
	(1.892)	(2.162)	(2.158)	(2.136)
No. of obs.	65,406	65,406	65,406	65,406
R^2	0.18	0.18	0.18	0.18
$\bar{y}$	−6.28	0.3	−1.01	−0.66
State–industry–year FE	Yes	Yes	Yes	Yes
Dem. controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes

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