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Center for Retirement Initiatives (CRI)
McCourt School of Public Policy
Georgetown University
125 E Street NW
Washington, D.C, 20001
Email: criretirement@georgetown.edu
<http://www.cri.georgetown.edu>

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Abstract

We examine how state policies requiring firms to facilitate workplace retirement saving affect household balance sheets. Using data from the Survey of Income and Program Participation (SIPP), we compare private-sector workers likely exposed to Oregon’s Automatic-Enrollment Individual Retirement Account (Auto-IRA) policy with similar workers in not-yet-adopting states. We find that the Auto-IRA policy is associated with increases in IRA and employer-sponsored retirement plan ownership and assets. We also find increases in checking or savings account ownership and balances, as well as higher credit card debt. The results suggest that Auto-IRA policies spill over to household liquidity management and borrowing.

Adam Bloomfield

Georgetown University Center for Retirement Initiatives
125 E Street NW, Washington, DC 20001
adam.bloomfield@gmail.com

Kyung Min Lee

The World Bank
1818 H Street, NW, Washington, DC 20433
klee12@worldbank.org

Ngoc Dao

Kean University College of Business and Public Management
1000 Morris Ave, Union, NJ 07083
ndao@kean.edu

Sita Slavov

George Mason University and National Bureau of Economic Research
3351 Fairfax Drive, Arlington, VA 22201
sslavov@gmu.edu

I. Introduction

Over the past decade, a growing number of U.S. states have adopted Automatic-Enrollment Individual Retirement Account (Auto-IRA) policies to expand access to workplace retirement saving among private-sector workers. These policies have two core features. First, they establish state-facilitated *Auto-IRA programs*, through which the state facilitates the establishment of IRAs for enrolled workers. Second, they mandate that employers either offer an employer-sponsored retirement plan (ESRP) or facilitate payroll-deduction contributions to the state Auto-IRA program. Enrollment and contributions to the Auto-IRA program are automatic, but workers may opt out. Auto-IRA policies are intended to increase retirement plan coverage among workers who do not have access to traditional workplace retirement savings, particularly those who are employed by small firms or earning lower wages. Employers are not permitted to contribute to workers' Auto-IRAs. They may contribute to ESRPs but are not required to do so. Nevertheless, all covered firms face compliance obligations: they must offer an ESRP, certify exemption, or facilitate payroll deductions to the state Auto-IRA program.

A growing literature shows that Auto-IRA policies increase IRA ownership and contributions, although opt-out rates are substantial and early withdrawals are common (Quinby et al. 2020; Dao 2024; Willingham 2025; Chalmers et al. 2025). Related studies find that these policies also induce some employers to expand employer-sponsored retirement plans (ESRPs), further raising retirement plan access and participation rates (Bloomfield et al. 2025a; Bloomfield et al. 2025b). While this evidence establishes that Auto-IRA mandates raise retirement saving and employer plan availability, less is known about their effects on broader households' financial portfolios. This question is important for evaluating the implication of these policies. If workers respond to Auto-IRA exposure by changing liquid account ownership, liquid balances,

withdrawals, or borrowing, then retirement account participation and balances provide only a partial picture of the policy’s effect on household financial security.

To examine these broader balance-sheet effects, we study the rollout of Oregon’s Auto-IRA program, OregonSaves, using data from the Survey of Income and Program Participation (SIPP). Oregon provides an informative setting to study household responses over time because it was the first state to implement an Auto-IRA program and phased in covered employers by firm size. The OregonSaves program started in 2017, initially applying to firms with 100 or more employees. The program was subsequently expanded to firms with 20–99 employees in 2018, firms with 5–19 employees in 2019, and firms with 1–4 employees in 2023.¹ This staggered rollout creates quasi-experimental variation in policy exposure across state, firm size, and time. In addition, Oregon’s early adoption provides a relatively long post-policy window for studying household financial adjustment.

We estimate the effect of OregonSaves using a staggered difference-in-differences approach. Our model compares Oregon private-sector workers in treated firm-size categories to similar workers in states that eventually adopted Auto-IRA policies but had not yet done so during the study period. This design isolates changes associated with mandate exposure while accounting for broader national trends in retirement saving, bank account ownership, and consumer debt.

We find that OregonSaves substantially increases retirement saving while also reshaping household balance sheets more broadly. IRA ownership rises by 2.7 percentage points (12.7 percent), and IRA balances increase by more than \$8,500. We also find increases in 401(k) ownership and balances, consistent with employer responses through expanded ESRP provision. Beyond retirement accounts, the policy is associated with higher checking-or-savings account

¹ Because of data limitations, this study focuses on the rollout for firms with 5 or more employees.

ownership and balances. These changes may reflect accounts opened to manage the new IRAs, for example to receive withdrawals or cash outs. They may also reflect some respondents misclassifying Auto-IRAs as liquid accounts. Furthermore, we observe an increase in credit card debt: the policy is associated with a 2 percentage point increase in the incidence of credit card debt and a \$90 increase in balances. These debt responses are modest relative to the estimated increases in retirement balances, but they are consistent with some households partially offsetting higher retirement saving through short-term borrowing.

These findings connect the recent Auto-IRA literature to the broader literature on whether automatic retirement saving policies generate offsetting household balance-sheet responses. Prior evidence comes from settings that differ from OregonSaves in their policy lever, institutional context, and population studied. Beshears et al. (2022) study a natural experiment in a single federal public-sector workforce, in which newly hired U.S. Army civilian employees were automatically enrolled in the Thrift Savings Plan; they find mixed evidence of increases in auto-loan and mortgage balances, but no significant changes in credit scores, adverse credit outcomes, or debt balances outside auto loans and first mortgages. Beshears et al. (2024) document increases in unsecured and mortgage debt among workers subject to automatic enrollment at small U.K. firms. Choukhmane and Palmer (2025) use linked deposit, credit, and pension data on customers of a large U.K. financial institution who also have pensions with the institution's pension subsidiary. They find that workers affected by higher minimum retirement contributions responded by reducing spending, drawing down deposit balances, and increasing debt.

We extend this evidence by studying a state-level employer mandate that expands workplace-intermediated retirement saving access in the U.S. private sector. Additionally, the SIPP allows us to examine retirement accounts, checking-or-savings accounts, and credit card debt

within a unified household balance-sheet framework, enabling a more comprehensive test of offsetting responses more than prior work.

II. Data and Empirical Approach:

We use data from the Survey of Income and Program Participation (SIPP), which provides detailed information on individuals' demographic characteristics, labor market activity, and household balance sheets, including retirement accounts, checking and savings accounts, and consumer debt. Unlike administrative data sources, SIPP allows us to observe multiple components of household financial assets and debts within a common survey framework. We use SIPP data from 2013 to 2022. Because financial asset information is collected in the last month of each SIPP wave, we restrict the sample to individuals who responded in that month. We further restrict the sample to private-sector workers ages 18–60 who report working at least two months during the year, consistent with OregonSaves eligibility rules, and who have non-missing firm-size information.

Our treatment definition follows the rollout structure of the OregonSaves. Treated observations are Oregon residents working for private-sector employers in firm-size categories that had become subject to the mandate. Following Dao (2024), we define treatment using the worker's state of residence, firm-size category, and year relative to the OregonSaves rollout. The comparison group consists of workers in the same firm-size categories in states that eventually adopted Auto-IRA policies but had not yet implemented them during our sample period. These states are New Jersey, New York, Washington, Colorado, Delaware, Vermont, Nevada, Minnesota, Hawaii, and Rhode Island. Restricting the comparison group to not-yet treated states is intended to improve comparability by focusing the analysis on states that were also moving

toward Auto-IRA adoption. The identifying assumption is that, absent OregonSaves, outcomes for Oregon workers in treated firm-size categories would have evolved similarly to outcomes for comparable workers in these not-yet treated states, conditional on the controls and fixed effects described below.

Table 1 reports weighted summary statistics for the analysis sample. Before 2017, 21.2 percent of Oregon workers in the sample owned IRAs and 42.4 percent owned 401(k) accounts. Checking-or-savings account ownership was high but not universal: 84.9 percent of Oregon workers had a checking or savings account before 2017, implying that 15.1 percent did not report having either account type. That fraction falls to around 7.1 percent after 2017. For context, FDIC estimates show that 7.0 percent of U.S. households were unbanked in 2015, and that unbanked rates were substantially higher among households headed by younger individuals and lower among households age 65 and older (FDIC 2016). Our SIPP measure is not directly comparable to the FDIC household-level unbanked measure. Still, the higher share without reported checking-or-savings account ownership in our sample is plausible because the sample excludes older households with especially low unbanked rates and measures account ownership at the worker level rather than household level. Some individuals who do not report having a checking or savings account may live in households with such accounts. Credit card debt was also common, with 32.3 percent of Oregon workers reporting credit card debt before 2017.

We estimate a staggered difference-in-differences model comparing changes in financial outcomes for Oregon workers in treated firm-size categories before and after mandate exposure to contemporaneous changes among comparable workers in not-yet treated states. Our baseline model is as follows:

$$Y_{igst} = \beta \text{AutoIRA}_{gst} + \mathbf{X}_{igst}\theta + \mu_{st} + \pi_{gt} + \gamma_{gs} + \varepsilon_{igst} \quad (1)$$

where, Y_{igst} is an outcome for individual i working in firm-size category g , in state s , and year t . Outcomes include account ownership and balances for retirement accounts, checking-or-savings accounts, and credit card debt. The treatment variable, $AutoIRA_{gst}$, equals one when workers in firm-size category g , state s , and year t are exposed to an Auto-IRA mandate, and zero otherwise. The coefficient of interest, β , captures the effect of mandate exposure on the outcome. The vector X_{igst} includes individual demographic controls including age, gender, education, race/ethnicity, and marital status. State-by-year fixed effects, μ_{st} , absorb state-specific shocks in each year; firm-size-by-year fixed effects, π_{gt} , absorb national shocks or trends affecting workers in the same firm-size category; and state-by-firm-size fixed effects, γ_{gs} , absorb persistent differences across state-by-firm-size cells. We use sampling weights, cluster standard errors at the state level, and report monetary outcomes in 2019 dollars, winsorized at the 1st and 99th percentiles.

Because OregonSaves was implemented in staggered firm-size waves, we estimate the model using the two-stage difference-in-differences estimator of Gardner et al. (2024), which is designed to address the staggered timing of mandate exposure across firm-size categories. In the first stage, we estimate the model with the fixed effects and controls described above using untreated and not-yet-treated observations. We then use these estimates to residualize outcomes. In the second stage, the residualized outcomes are regressed on a treatment indicator. We also estimate event-study specifications in which the second-stage residualized outcomes are regressed on relative event-time indicators, with observations more than three years before or after mandate exposure binned at the endpoints. These event studies provide a diagnostic check on pre-policy trends and help assess whether estimated changes coincide with OregonSaves rollout timing.

III. Results

a. Retirement Account Ownership and Balances

Table 2 presents the estimates of the effect of Oregon’s Auto-IRA mandate on retirement account outcomes. Figure 1 presents event-study versions of these results. Column 1 in Panel A of Table 2 shows an average 2.7 percentage point (12.7 percent relative to the pre-policy mean in Table 1) increase in IRA ownership among treated individuals relative to comparable individuals in not-yet treated states following the rollout. Panel A in Figure 1 (the event study) suggests that the increase in IRA ownership occurs when the policy is implemented and persists throughout the post-treatment period. Although the event time zero coefficient appears negative, the later post-treatment coefficients are positive and statistically significant. Column 1 in Panel B of Table 2 shows a \$8,642 increase in IRA balances (roughly 78 percent relative to the pre-policy mean in Table 1). This magnitude is substantial, particularly given the combination of modest default contribution rates, lower income levels, and high opt-out and early withdrawal rates documented in prior studies of Auto-IRA programs. Panel B in Figure 1 (the event study) shows that the increase in balance coincides with the implementation of the policy. Although some pre-policy coefficients are significant, there is no clear trend until the implementation of the policy.

Column 2 in Panels A and B of Table 2 show similarly noticeable increases in 401(k) ownership and balances. These patterns are consistent with prior research suggesting that many employers adopt ESRPs in response to the state mandates (Bloomfield et al., 2025a, 2025b). Panels C and D of Figure 1 (the event studies) show that the timing of these changes is associated with the implementation of the policy. Again, although some pre-policy coefficients are significant, there is no clear trend until the implementation of the policy.

b. Liquid Savings and Credit Card Debt

Columns 3 and 4 of Table 2 report how Auto-IRA exposure affects broader household balance sheets, focusing on liquid bank account holdings and consumer credit card debt. Column 3 shows an increase in checking or savings account ownership (panel A) and balances (panel B) among treated individuals following the mandate's rollout. Overall, the policy is associated with a 2.3 percentage points (2.7 percent) increase in the probability of checking or savings account ownership. The corresponding event studies in Figure 2 are noisy, but they provide suggestive evidence that some of the measured changes emerge after the mandate exposure .

One explanation for these patterns is that some individuals may open new deposit accounts to facilitate Auto-IRA contributions or withdrawals. For example, some previously unbanked individuals may need to establish a bank account to withdraw and transfer funds from a new Auto-IRA when they opt out. Alternatively, some respondents may misclassify Auto-IRA balances as checking or savings accounts in their SIPP responses, particularly given the relatively small balances involved and the limited salience of account labels to consumers with lower-than-average financial literacy. While the SIPP data do not allow us to distinguish between these mechanisms, both interpretations imply that Auto-IRA mandates affect how households either organize or report their liquid finances.

Column 4 of Table 2 reveals that the policy is associated with an average 2 percentage point increase in the probability of having credit card debt (6.2 percent increase relative to the pre-policy mean in Table 1), and a \$90 increase in credit card balances (5.6 percent increase relative to the pre-policy mean in Table 1). These results are consistent with some households partially offsetting increased retirement saving through higher credit card borrowing. These findings add

support to the literature on automatic enrollment in ESRPs, which finds that increases in retirement contributions are sometimes accompanied by increases in consumer debt.²

IV. Conclusion

Using data from the Survey of Income and Program Participation and the rollout of Oregon’s Auto-IRA policy, we show that state Auto-IRA policies affect household balance sheets along multiple margins. Consistent with prior research, we find that Oregon’s policy increased IRA and ESRP ownership and balances.

Beyond retirement saving, we document changes in households’ liquid financial holdings and credit card debt. Exposure to the policy is associated with higher checking and savings account ownership and balances, alongside increases in credit card debt. These patterns suggest that Auto-IRA policies may influence how households manage liquidity, borrowing, and their engagement with financial institutions. While our analysis does not address the impact of Auto-IRAs on long-run wealth accumulation, it highlights important financial responses that go beyond retirement saving and are relevant for policymakers designing and evaluating these policies.

Taken together, our findings suggest that evaluation of Auto-IRA programs should look beyond retirement-account participation and balances. Those measures remain central, but they may miss effects on household balance sheets, including changes in checking-or-savings account ownership, cash-outs or withdrawals, and short-term borrowing. For program administrators,

² We also examine the effect of the policy on non-financial assets and debts, such as housing values, mortgage balances, and vehicle loans (results available upon request). The policy is associated with increases in non-financial debt; though these findings should be interpreted cautiously given the well-documented supply constraints and in-migration dynamics specific to Oregon during our study period (e.g., Lewis and Parker 2021; Oregon Department of Land Conservation and Development).

researchers, and policy analysts, the findings point towards the importance of linkages between program records, survey data, and credit-bureau or banking data.

Future work with longer post-treatment horizons and administrative data could assess whether these short-run portfolio adjustments persist and whether Auto-IRA policies have a long-term impact on wealth accumulation and composition. Furthermore, future research could examine whether these findings generalize to other state Auto-IRA programs – where program design, rollout structure, and eligible worker populations vary significantly.

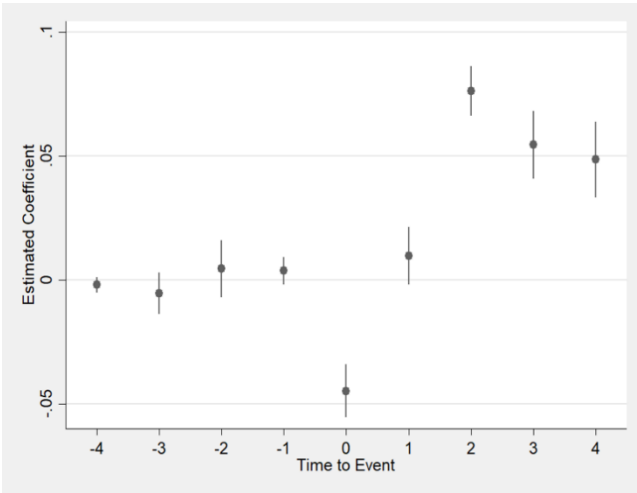
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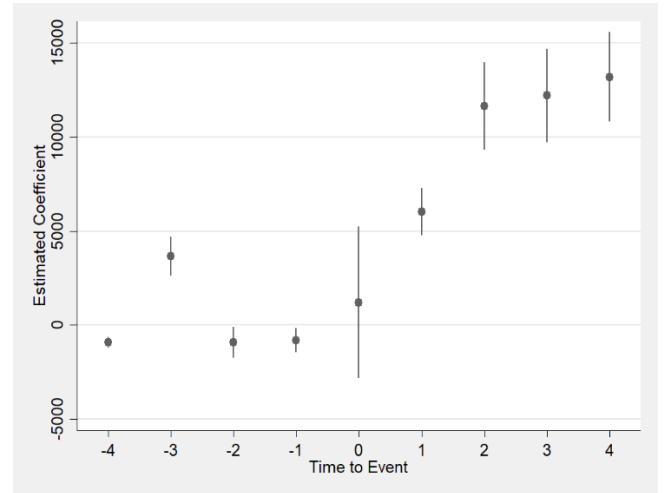
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Figures

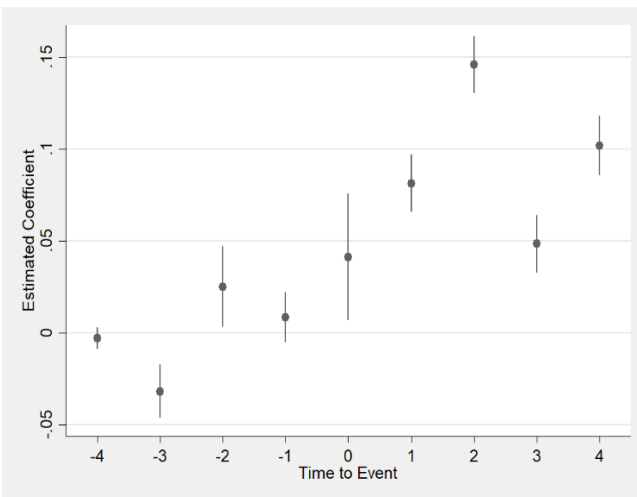
Figure 1. Event-Study Estimates for Retirement Accounts



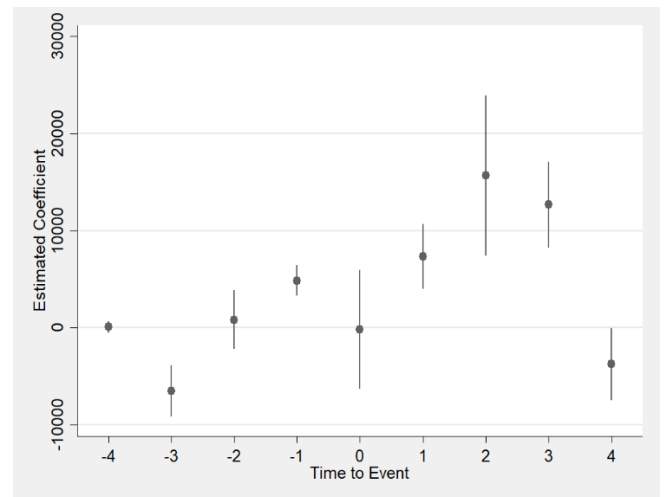
(A) IRA Ownership



(B) IRA Balance



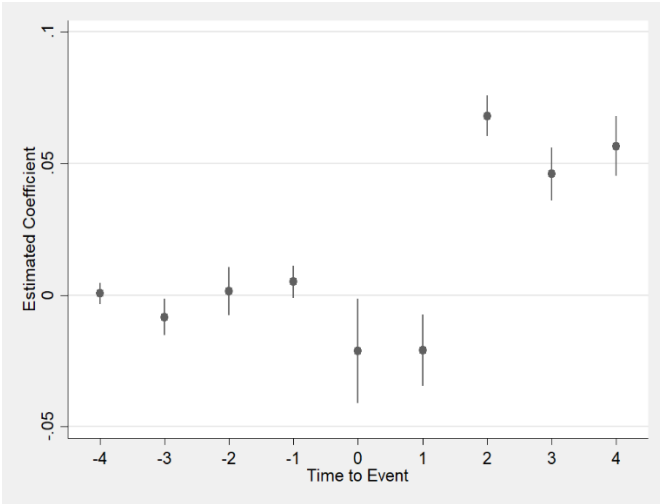
(C) 401(k) Ownership



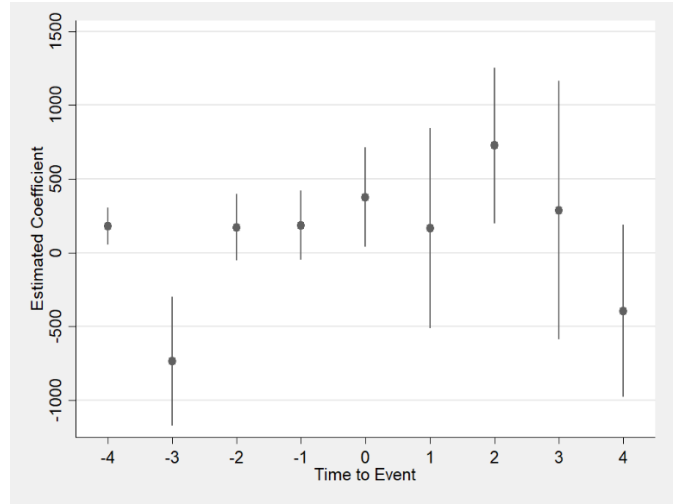
(D) 401(k) Balance

Notes: Authors' calculations from SIPP 2013–2022. Sample includes individuals in the last month of each SIPP wave. Sample restricted to private-sector workers aged 18–60 who worked least 2 months during the reporting period. Each point represents the impact of OregonSaves on the outcome at the indicated time relative to implementation in the relevant firm size category. Vertical lines represent 95 percent confidence intervals.

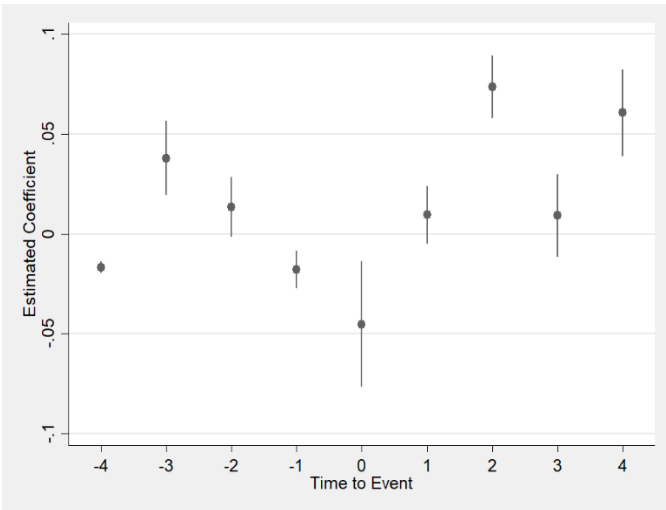
Figure 2. Event-Study Estimates for Non-retirement Accounts



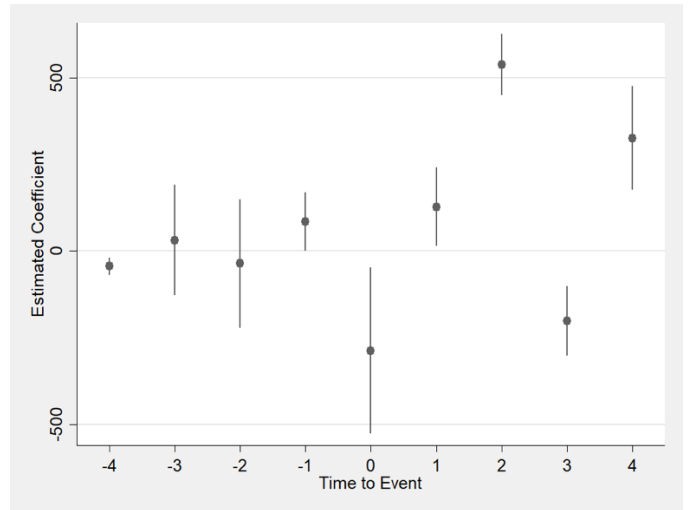
(A) Checking and Savings Account Ownership



(B) Checking and Savings Account Balance



(C) Credit Card Debt Ownership



(D) Credit Card Debt Balance

Notes: Authors' calculations from SIPP 2013–2022. Sample includes individuals in the last month of each SIPP wave. Sample restricted to private-sector workers aged 18–60 who worked least 2 months during the reporting period. Each point represents the impact of OregonSaves on the outcome at the indicated time relative to implementation in the relevant firm size category. Vertical lines represent 95 percent confidence intervals.

Table 1. Summary Statistics

Variable	Oregon			Control States			Simple Net Difference
	Pre	Post	Post-Pre	Pre	Post	Post-Pre	
<i>Outcome Variables</i>							
IRA Ownership	0.212	0.204	-0.008	0.223	0.225	0.002	-0.010
401K Ownership	0.424	0.475	0.051	0.440	0.487	0.047	0.004
Checking Account	0.640	0.916	0.276	0.647	0.916	0.269	0.008
Savings Account	0.646	0.767	0.121	0.676	0.728	0.052	0.069
Checking or Savings Account	0.849	0.929	0.080	0.856	0.933	0.077	0.004
Credit Card Debt	0.323	0.342	0.019	0.368	0.350	-0.018	0.037
IRA Amount (\$)	11025	15307	4282	14048	17644	3597	686
401K Amount (\$)	32141	41728	9587	40903	49028	8125	1463
Checking Account Amount (\$)	895	3128	2233	1564	3958	2394	-161
Savings Account Amount (\$)	3071	5292	2221	4096	5666	1570	651
Checking or Savings Account Amount (\$)	3310	6124	2814	4443	7365	2922	-108
Credit Card Debt Amount (\$)	1603	1884	282	1927	2027	100	181
<i>Demographics Variables</i>							
Female	0.487	0.465	-0.022	0.480	0.468	-0.012	-0.011
Age < 30	0.341	0.280	-0.061	0.287	0.273	-0.014	-0.047
Age 30-39	0.264	0.266	0.001	0.253	0.278	0.025	-0.023
Age 40-49	0.184	0.252	0.068	0.223	0.221	-0.002	0.070
Age 50-59	0.192	0.186	-0.005	0.220	0.208	-0.011	0.006
Age 60	0.019	0.016	-0.003	0.018	0.021	0.003	-0.006
Less than High School	0.085	0.116	0.031	0.074	0.059	-0.015	0.046
High School	0.254	0.245	-0.009	0.235	0.238	0.003	-0.012
Some College	0.347	0.309	-0.038	0.313	0.259	-0.054	0.015
University	0.221	0.206	-0.014	0.249	0.287	0.038	-0.053
More than University	0.093	0.123	0.031	0.130	0.158	0.028	0.003
Hispanic	0.151	0.201	0.050	0.164	0.168	0.004	0.046
Non-Hispanic White	0.753	0.708	-0.045	0.617	0.600	-0.016	-0.029
Non-Hispanic Black	0.009	0.008	0.000	0.108	0.104	-0.004	0.004
Non-Hispanic Asian	0.031	0.036	0.005	0.083	0.099	0.016	-0.011
Non-Hispanic Other	0.056	0.047	-0.009	0.029	0.029	0.000	-0.010
Never Married	0.384	0.363	-0.021	0.357	0.389	0.032	-0.053
Married	0.455	0.478	0.024	0.490	0.481	-0.009	0.033
Divorced/Separated	0.113	0.126	0.013	0.108	0.096	-0.012	0.025
Widowed	0.006	0.007	0.001	0.010	0.010	0.000	0.001

Notes: Data comes from SIPP 2013–2022. The sample includes individuals in the last month of each SIPP wave, who were private workers aged 18–60 and worked at least 2 months in each wave. All statistics are adjusted by sample weights. Monetary values are deflated to 2019 dollars and winsorized at the 1st and 99th percentiles.

Table 2. Estimated Effects of Auto-IRA on Retirement and Non-retirement Accounts

Panel A: Account Ownership				
	(1)	(2)	(3)	(4)
	IRA	401K	Checking or savings account	CC debt
OR x Post x Firm Size	0.027*** (0.002)	0.082*** (0.006)	0.023*** (0.004)	0.020*** (0.006)
Observations	27,895	27,895	27,895	27,895
Panel B: Account Balance				
	(1)	(2)	(3)	(4)
	IRA balance	401K balance	Checking or savings account balance	CC debt balance
OR x Post x Firm Size	8,642.117*** (398.714)	6,207.262*** (1,317.35)	225.513* (122.95)	90.088*** (28.544)
Observations	27,895	27,895	28,554	27,895

Notes: Each coefficient represents the estimate of β from equation (1) using Gardner et al.'s (2024) two-stage difference-in-differences method. All models include state-by-year, firm size-by-year, firm size-by-state fixed effects, and individual controls. Standard errors in parentheses are clustered at the state level. Sample weights used in estimation. Monetary outcomes expressed in 2019 dollars and winsorized at the 1st and 99th percentiles. *** p<0.01, ** p<0.05, * p<0.1