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ABSTRACT

We investigate how state "Auto-IRA" mandates affect firm offerings of employer-sponsored retirement plans (ESRPs). These policies require firms without ESRPs to facilitate automatic employee contributions to state-created individual retirement accounts (IRAs). We find that these policies increase an individual's probability of working for a firm with an ESRP by 6-9 percent and of being included in the ESRP by 8-13 percent. At the firm level, these policies increase the probability of offering an ESRP by 7, the probability of establishing a new ESRP by 41-44 percent, and the number of ESRP participants by 6 percent.

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1. Introduction

Employer-sponsored retirement plans (ESRPs)[2] represent the largest source of private retirement saving, and over time an increasing share of employers have been automatically enrolling workers in these plans (requiring employees to make an active decision to opt out). While many workers are not covered by ESRPs, all workers can establish and contribute to Individual Retirement Accounts (IRAs) on their own; however, most do not (Chen and Munnell 2017). In recent years, many states have taken steps to increase participation in retirement savings plans. California, Oregon, and Illinois have adopted workplace retirement savings mandates and automatic enrollment IRA (Auto-IRA) programs, under which employers not offering an ESRP to any of their employees must facilitate payroll deductions from workers' paychecks to be transferred to state-facilitated IRAs. Other states are in the process of implementing similar policies.[3] As with ESRPs that feature automatic enrollment, employee payroll deductions in these Auto-IRAs occur by default and require no active choice on the part of workers.[4] While workers can opt out, a large body of evidence on automatic enrollment in ESRPs – starting with Madrian and Shea (2001) – suggests that many do not do so.

[2] Common types of ESRPs in the US include 401(k), 403(B), Thrift Savings, 457, and other plans.

[3] There are currently 19 states that have taken steps to adopt an Auto-IRA program, though most of them have not yet implemented their programs. Connecticut's mandate and program rolled out from mid-2022 to late 2023 while Colorado's policy went into effect in 2023, after the end of our sample period. While Maryland implemented its policy in 2022, it is significantly different from those of the three states we study because the employer mandate is incentivized by a plan filing fee exclusion instead of a financial penalty. The three states we focus on (i.e., Oregon, Illinois, and California) account for vast majority of IRAs opened and assets saved (97%) under implemented state Auto-IRA programs (Georgetown University, Center for Retirement Initiatives, 2023).

[4] To be more specific, Auto-IRA programs apply to employers that do not offer any of their employees a retirement plan. These employers are required to upload their payroll to a state portal. The state then notifies each employee to inform them that they will be added to the state plan unless they follow an opt-out mechanism detailed in the email. If the employee does not opt out within a specified period, they are automatically enrolled in the state plan. Employers must remit the state-mandated fraction of the employee's paycheck to the program administrator, who then deposits the money into the employee's IRA.

In this paper, we examine how workplace retirement savings mandates in the three earliest-adopting states affect employer behavior concerning non-wage benefits. In particular, we examine whether Auto-IRA policies affect employers' decisions to offer or terminate ESRPs, as well as worker participation in such plans. As we explain in more detail below, the implementation of state Auto-IRA policies can either increase, decrease, or leave unaffected the fraction of firms offering ESRPs. To reduce costs, some firms may drop their existing ESRPs if they believe that their employees would view automatic enrollment in the state program as a reasonable substitute. On the other hand, the state mandates may impose administrative costs on firms that do not offer ESRPs. These mandates may also change business norms and the salience of retirement benefits. In response, some firms may begin to offer their own ESRPs. We employ difference-in-differences and event studies to estimate these effects. Our results suggest that, overall, firms affected by the mandate are more likely to offer ESRPs. Auto-IRA policies increase an individual's probability of working for an employer that offers an ESRP by 6-9 percent and the probability that the individual reports being included in an ESRP by 8-13 percent. These policies also increase the likelihood that an employer offers an ESRP by 7 percent, the probability that an employer establishes a new ESRP by 41-44 percent, and the number of ESRP participants at the average firm by 6 percent. We find no scientifically meaningful effects on the probability that an employer terminates their ESRP.

This paper is most closely related to the small, recent, and growing literature on the impact of state Auto-IRA programs. This literature includes some early studies of Oregon's Auto-IRA program, such as Quinby et al. (2020), who show that participation rates range from 48 percent to 67 percent of eligible employees; Chalmers et al. (2022), who show that

participation rates in the program decline over time as workers increasingly opt out; Zhong (2021), who aims to estimate an optimal default savings rate by using survey data on time preference and empirical analysis of how Oregon employees react to changes in the default savings rate; and Dao (2023), who uses SIPP data to show that OregonSaves resulted in a 12 percent increase in IRA ownership, a 3 percent increase in IRA balances, a 3 percent increase in retirement savings, and no effect on 401(k) participation or balances among Oregon workers.

A handful of policy studies have also focused on how Auto-IRA policies may affect employer behavior across states. One such study was conducted in 2017 and presented data from a survey of small- and medium-sized employers. In that survey, 13 percent of employers currently offering retirement plans said they would drop them in favor of a hypothetical state IRA program; however, more than half of employers not offering plans stated that they would implement one in response to Auto-IRA mandates (Pew Charitable Trusts 2017). Other studies have examined trends based on filings of Form 5500 (a disclosure document required of ESRP administrators) to show that there appears to have been an increase in the rate of ESRP formation, and no change in ESRP termination, in Auto-IRA adopting states relative to non-adopting states (Scott 2021; Guzoto, Hines, and Shelton 2022; Olson 2022).

We advance the study of state Auto-IRA programs by using both individual-level data from the Current Population Survey (CPS) and firm-level data from Form 5500 filings to examine the impact of Auto-IRA policy rollout on firm ESRP offerings and worker access to ESRPs. Compared to prior work utilizing plan-level data, our use of nationally representative and individual-level CPS data provides external validity. Moreover, the CPS includes separate questions on whether a respondent's employer offers an ESRP to any employees, and whether

the respondent participates in that ESRP. Thus, we can examine not only changes in firm-level ESRP offerings, but also the extent of worker coverage by these ESRPs. In addition, our results based on Form 5500 filings extend the work done in the policy studies discussed above by utilizing quasi-experimental regression methods, with many controls, rather than a simple comparison of state trends. This approach helps us to rule out spurious or coincidental effects that may be driving the state-level trends. We also exploit additional sources of variation – including the policy's rollout by employer size – to strengthen causal identification.

More broadly, this paper contributes to the relatively small literature on the determinants of firm provision of fringe benefits to workers. Most of this literature considers firm provision of health insurance to employees (Gentry and Peress 1994; Pauly 1986; Woodbury and Huang 1991), while one recent paper considers why firms may design low-quality retirement plans for workers (Bhattacharya and Illanes 2022). Regarding retirement benefits more specifically, the literature (e.g., Beshears et al. 2022; Madrian and Shea 2001) has tended to focus on worker decisions to participate in ESRPs rather than firm decisions to provide them. In contrast, we focus primarily on the firm decision to provide retirement benefits. Our results highlight the fact that employee participation in a retirement plan is a function of both employer and worker decisions.

Additionally, our work broadly intersects with the literature on the impacts of employer mandates. For example, there have been studies of the Affordable Care Act's (ACA) requirement that employers provide health insurance to full-time employees. The ACA also created state health insurance exchanges (with pricing based on community rating), in conjunction with household premium subsidies and substantially expanded Medicaid coverage. These changes provided alternatives to employer-sponsored health insurance.

Theoretically, the ACA's employer mandate may have increased the availability of employer-sponsored health insurance. Alternatively, it may have caused employers to drop their existing health insurance plans – which are costly to administer – and send employees to the state exchanges or Medicaid, paying any applicable penalties. In its cost estimate of the ACA, the Congressional Budget Office (2010) projected that the legislation would reduce the number of jobs with employer-sponsored health insurance, leaving these employees to obtain coverage from the state insurance exchanges instead. However, empirical studies have found mixed evidence (Lennon 2021; Abraham et al. 2019).

This paper examines the impact of an employer mandate in a different context. An important difference is that the ACA coupled the employer mandate with the creation of an option that did not previously exist, namely non-employment-based group insurance coverage. In the current setting, recent Auto-IRA state policies only altered the enrollment default for a non-employment-based retirement savings option that already existed, namely the IRA.

2. Conceptual Framework

As discussed by Summers (1989), employers in a competitive labor market will provide a non-wage benefit to workers when the value of the benefit to workers exceeds its cost to employers. In this situation, monetary compensation will be reduced by an amount between the value to workers and the cost to employers, resulting in a mutually beneficial exchange. Applying this logic to the current setting, and assuming for now that workers and firms are fully rational, Auto-IRA mandates should not affect the behavior of firms that *already* provide ESRPs. Workers have always been able to contribute to IRAs with or without

state Auto-IRA programs, and, unlike ESRPs, state Auto-IRAs do not permit or require employer matching contributions. In other words, the state-run program merely alters the default enrollment for a savings vehicle that has always existed. Thus, an employer dropping an ESRP in favor of the state Auto-IRA program would amount to reducing workers' compensation by the value of the ESRP. If providing the ESRP was optimal according to the Summers (1989) analysis, that reduction in nonwage benefits must be offset by an increase in monetary compensation that exceeds the cost to employers of providing the ESRP. Stated a different way, the adoption of Auto-IRA policies should not suddenly change the calculus for firms and workers regarding ESRPs that are already being offered.

On the other hand, under the same set of assumptions, state Auto-IRA laws may alter the behavior of firms that do *not* offer ESRPs. For these firms, the value to workers of an ESRP does not exceed the employer cost of providing one. However, we would expect the amount by which the value falls short of the cost to vary across firms. Auto-IRA laws impose an administrative cost – effectively a tax – on employers that do not choose to offer their own retirement plans. Even if this cost is small, firms that are close to indifferent between offering and not offering an ESRP – i.e., firms for which the value to workers is slightly below the cost – may be induced to adopt an ESRP. Thus overall, if labor markets are competitive and agents are fully rational, then Auto-IRA mandates should expand the set of firms providing ESRPs, although the effect is likely to be small.

If we drop the assumption that firms and workers are fully rational, classical behavioral economics suggests an additional reason why firms may respond to Auto-IRA policies by starting to offer their own ESRPs. In the presence of cognitive constraints, the social debates and litigation around Auto-IRA laws may increase the salience of retirement plans to firms

and workers who were previously unaware of or unconcerned about the issue. This increased salience may in turn increase employees' *perceived* valuation of ESRPs relative to employers' costs of providing them, as well as the likelihood that firms and workers will notice this opportunity for mutually beneficial exchange. Moreover, even if workers on average have weak preferences between ESRPs and Auto-IRAs, decisions within a firm may be made by a single owner or a small group of senior managers. If these decision makers are more highly compensated, forward-looking, and wealth-maximizing than the average worker, then the increased salience of retirement benefits may weigh in favor of ESRPs over state IRAs.

Beyond these classic behavioral economic factors (i.e., cognitive limits and salience), there are other channels that could increase the propensity of firms to offer ESRPs to workers in the wake of state Auto-IRA policies. For example, financial services providers may use these policies, and the surrounding debate, to sell retirement products and services to employers who must now comply with new state rules. In many cases, these financial services companies also provide payroll services to employers and are therefore naturally positioned to cross-sell ESRPs to firms. To highlight one example, a recent Morgan Stanley brief alludes to the new state Auto-IRA laws, then promotes its own small business 401(k) services, arguing, "Many states are mandating that employers offer some type of retirement savings plan and workers are looking for job opportunities that offer this type of benefit" (Morgan Stanley 2023). Moreover, it is possible that the mandates may influence business culture and norms. For example, employers that do not provide plans may increasingly be viewed negatively in communities where these policies have been debated and implemented.

On the other hand, it is possible that some firms with existing ESRPs may drop these plans – which are costly to administer and are subject to potentially burdensome regulation

such as nondiscrimination tests – and instead choose to facilitate automatic enrollment of workers into IRAs via the state program, which require no ongoing costs from employers other than the administrative effort of initially enrolling employees. Given that state Auto-IRA programs simply change the default for an existing savings vehicle, this substitution is only plausible if employees are less than fully rational. If employees have present biased preferences and suffer from inertia, for example, the auto-enrollment component of the state programs – which allows them to overcome their tendency to under-save – may have value. Moreover, if employees are not fully informed about the longstanding availability of IRAs, they may perceive the state plans as a new employer-provided benefit or equivalent substitute for an ESRP. This hypothesis is reminiscent of the classic scenario where publicly provided services "crowd-out" privately provided alternatives.

3. Data and Methods

a. Data

We draw on two sources of data, one at the individual level and another at the employer level. Individual level data comes from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS). We access a harmonized version of this data available through the University of Minnesota's Population Center (Flood et al. 2023). The CPS is a monthly, nationally representative survey of U.S. households that provides basic labor force data, implemented by the U.S. Census Bureau for the Bureau of Labor Statistics. The CPS-ASEC – conducted in March of each year – provides more detailed information on work experience, income, education, employer characteristics, and receipt of nonwage benefits in the previous calendar year. Since the CPS-ASEC provides information for the previous year, we use

data for the years 2010 through 2023 to analyze outcomes for 2009 through 2022. We restrict the sample to adults between the ages of 25 and 54 who are employed in the private sector. Auto-IRA mandates only affect respondents employed in the private sector, and individuals in their prime working years are the main population targeted by retirement savings policy.

The CPS-ASEC includes a question about whether a respondent's union or employer offered a pension or other retirement plan, and whether the respondent was included in that plan. This question refers to the longest job the respondent held during the previous year. There are three possible responses to this question: (1) a respondent's employer or union did not offer a plan to any of its employees, (2) a respondent's employer or union offered a plan to some of its employees, but the respondent was not included, or (3) a respondent's employer or union offered a plan to some of its employees, and the respondent was included. As our main dependent variables, we construct two indicators: one for the respondent's employer offering a plan and the other for the respondent being included in a plan. Consistent with some prior academic work (e.g., Radpour et al. 2021; Sabelhaus 2022) and a series of Employee Benefit Research Institute briefs (including Copeland 2014, 2015, 2019, 2020), we interpret an affirmative response to question (3) (being "included" in a retirement plan) to mean that the employee *participates* in the plan. Additional variables derived from the CPS-ASEC include the size of the respondent's employer (number of employees, reported in categories), as well as the respondent's gender, race, ethnicity, and education level. As Auto-IRA policies were rolled out at different times for firms of different sizes, we can use firm size to identify more precisely which respondents would be affected by the mandate.[5]

[5] Firm size is reported in categories that are not consistent across years; these categories also do not line up exactly with those in the Auto-IRA laws. We match the available categories as closely as possible. More details are available upon request.

There are three potential measurement issues in the CPS data. First, as noted above, we interpret inclusion in an ESRP to mean participation in the ESRP. While that interpretation is in line with other work, it is not obvious whether respondents would necessarily interpret the term "included" to imply participation. Second, as discussed by Sabelhaus (2022), Radpour et al. (2021), and Copeland (2015, 2019, 2020), a 2013 redesign of the CPS questions on ESRPs appears to have resulted in substantial understatement of ESRP coverage compared to other data sources. Finally, while the questions about pensions and retirement plans instruct respondents to exclude Social Security, there is no mention of state Auto-IRAs. Therefore, one might be concerned that respondents who notice payroll deductions directed to their Auto-IRA may mistakenly report this program as an ESRP. There is no reason to expect the first two sources of measurement error to be correlated with the adoption of Auto-IRA legislation (in the latter case once year is controlled for). Thus, they should not bias our findings. However, the third source of error – employees potentially mistaking their state Auto-IRA for an ESRP – is obviously correlated with Auto-IRA mandates and may bias upward our estimates of employer responses. As we discuss below, however, we can address all three concerns by complementing our individual-level analysis with an analysis based on firm-level analysis of Form 5500 filings. These filings are not subject to the same kinds of ambiguity and misreporting as an individual survey.

Our baseline estimates include all states other than California, Illinois, Oregon, Connecticut, and Maryland as control states.[6] We also present results utilizing a smaller control group of untreated "blue" states – states classified as Democratic-leaning by

[6] Connecticut's policy and program rolled out from mid-2022 to late 2023, but all employers received an extension of time to register until August 31, 2023. Maryland also implemented a retirement savings mandate and Auto-IRA program in September 2022. However, Maryland's program was not enforced by imposing penalties on noncompliant employers. Rather, the state incentivizes participation by exempting participating employers from a state plan filings fee. Therefore, we do not expect Maryland's policy to have the same impact as the three state Auto-IRA policies enforced by financial penalty. Maryland and Connecticut are therefore excluded from the control group.

FiveThirtyEight's partisan lean scores calculated in 2021 (Rachkin 2021). All three treated states are "blue" by this metric.[7] Table 1 shows the means and standard deviations of all variables in the CPS-ASEC sample overall. It also shows the means of each variable broken down by whether the individual resides in a state that implemented Auto-IRA legislation (treatment state), a state that did not implement Auto-IRA legislation (never-treated states), or a blue state that did not implement Auto-IRA legislation (never-treated blue states). The table suggests that there are racial composition differences across the three treated states and both groups of never-treated states. California has a lower share of individuals covered by an ESRP compared to never-treated states. However, the shares of ESRP-covered individuals in Illinois and Oregon are similar to those in the control groups. Education levels, employment status, and the employer size distribution appear similar across all treated and never-treated states. Since retirement plan coverage may be correlated with demographic and human capital variables for reasons other than Auto-IRA legislation, we control for these variables in our specifications.

Figures 1-6 present raw plots of the two dependent variables – working for an employer that offers a pension or retirement plan and being included in such a plan – for each adopting state relative to each control group. Figures 1-3 present results for California, Illinois, and Oregon, respectively, compared to all non-adopting states. Figures 4-6 present results for California, Illinois, and Oregon, respectively, compared to non-adopting blue states. All figures include vertical lines at the date of first adoption in the treated state. While the data are noisy, Figures 2-3 and 5-6 are suggestive of a post-policy increase in ESRP

[7] Blue states include California, Colorado, Delaware, District of Columbia, Hawaii, Illinois, Maine, Massachusetts, Minnesota, New Hampshire, New Jersey, New Mexico, New York, Oregon, Rhode Island, Vermont, Virginia, and Washington.

coverage in Illinois and Oregon relative to both groups of control states. Figures 1 and 4, for California, are less clear. However, these raw plots do not control for any other factors that may influence trends in ESRP coverage, such as changes in demographics, the firm size distribution, or macroeconomic factors.

Our employer-level data come from Form 5500 filings for 2009 to 2022. Form 5500 and Form 5550 Short Form (Form 5500 SF) are filed annually by organizations that offer their employees a retirement plan or other fringe benefits to satisfy the reporting requirements of the Department of Labor, the Internal Revenue Service, and the Pension Benefit Guaranty Corporation. Form 5500 and Form 5500 SF data are organized at the plan level but can be readily aggregated to the firm-level using the employer identification number (EIN). The filing for each plan contains information on the number of participants in the plan, as well as information about the plan sponsor, income, expenses, assets, and type (defined benefit or defined contribution). For each plan in this dataset, we extract the sponsor's EIN, industry, year of filing, total number of participants, and indicators for the type of plan. Defining a firm as an EIN, we then collapse the dataset to the firm-year (EIN-year) level. Because employees at a firm may be covered by multiple plans, we proxy the number of participants at a firm with the maximum of the number of total participants across plans within a firm. We drop a small group of EINs that switch states or have invalid state codes during the sample period. We also drop the firms with missing EINs or those in American Samoa, Guam, the Northern Mariana Islands, Puerto Rico, and the U.S. Virgin Islands.

Firms file Form 5500 only if they offer retirement plans. That is, we do not observe firms that do not offer plans. Moreover, if a firm that appears in the dataset in one year does not appear in another, we cannot tell if the firm did not offer plans during the year in which

it does not appear, or if it simply did not exist. To fill in this gap, we make a strong assumption: we assume that all firms appearing even once in the plan filings between 2009 and 2022 existed during the whole period. Thus, we generate a balanced panel by filling in zeroes for plan offering status, and total participants for firms in years during which no filings are observed. This assumption is obviously unrealistic, as it ignores the processes of firm creation and destruction. In our dataset, the creation of a firm with a retirement plan gets recorded as the introduction of a plan in an existing firm, and the destruction of a firm gets recorded as the dropping of a plan in a firm that continues to exist. However, there is no reason to expect these errors to vary across states in a way that is correlated with the implementation of Auto-IRA policies.[8]

We derive several dependent variables from the form 5500 data. First, we construct an indicator equal to 1 if a firm offers an ESRP in each year, and 0 otherwise. This variable simply measures whether at least one Form 5500 was filed for an EIN for the year. We also construct a measure for starting an ESRP. This variable takes on a value of 1 for a firm in year t if the firm did not offer an ESRP in year $t-1$ but offers an ESRP in year t. It takes on a value of 0 for firms that either continue to offer, or continue not to offer, an ESRP in $t-1$ and t. Similarly, we construct an indicator for ending an ESRP that takes on a value of 1 for a firm in year t if the firm offered an ESRP in year $t-1$ but does not offer an ESRP in year t. It takes on a value of 0 for firms that either continue to offer, or continue not to offer, an

[8] We test this assumption using the Census Bureau's business dynamics statistics (BDS), which provides the annual entry and exit rates of establishments at the state level. Applying the same difference-in-differences methodology used to produce the main results in the paper, we find no statistically significant post policy change in firm entry and exit rates in Auto-IRA states compared to never-treated states. Another potential issue with our creation of a balanced panel is that a small number of firms also switch EINs during the sample. We effectively treat each EIN of the firm as a separate firm. For robustness, we re-estimate our main difference-in-differences results using only firms that do not switch EINs and show that they are similar. Results for both exercises are available upon request.

ESRP in $t-1$ and t. All three variables are subject to the caveat discussed above: they are measured with error because they conflate firm creation and destruction with ESRP creation and destruction. Our final firm-level dependent variable is the total number of participants in an ESRP. To reduce the influence of outliers (the maximum number of ESRP participants reported by an EIN is more than 12 million), we winsorize this variable, setting the bottom and top 0.5 percent of values to the 0.5^{th} and 99.5^{th} percentiles, respectively. To check robustness, we also consider winsorizing the top and bottom of the distribution at both 0.1 and 1 percent. (Since many firms have zero participants – in years when no form 5500 is filed – winsorizing effectively censors only the top of the distribution.) The three indicators for offering, starting, or stopping an ESRP are intended to capture the extensive margins of responses (whether to offer a plan or not), while the total number of participants includes both intensive and extensive margins. The policy may increase the number of participants at treated firms that already offer ESRPs if employers change the plan default from non-participation to automatic enrollment (intensive margin), or if retirement benefits become more salient to covered workers in treated states, causing more of them to sign up. It also increases the number of participants at firms that begin offering plans (extensive margin).

Table 2 shows descriptive statistics for the Form 5500 data, both overall and for firms in the control and treatment states separately. It shows that the propensity for a firm to offer at least one retirement plan is slightly higher in Illinois, and slightly lower in California, compared to control states. The total participant count at the EIN level is lower in California and Oregon compared to control states. However, the rates of starting and stopping plans are similar across all three treatment states and both groups of control states. Figures 7-12 show raw plots of the four dependent variables for each treated state compared to each set of

controls. Figures 7-9 show results for California, Illinois, and Oregon, respectively, compared to all non-treated states. Vertical lines indicate the year in which the policy was implemented for any set of firms. Figures 7 and 9 show marked increases in the probability of offering or starting a plan in California and Illinois following the adoption of Auto-IRA policies. There is also a slight increase in total participants in the two treated states relative to the control group. However, we see no post-policy divergence in the probability of ending an ESRP. Figure 8 shows that the trends for Illinois are less clear. Figures 10-12 show similar findings for California, Illinois, and Oregon, respectively, compared to never-treated blue states. Of course, these figures do not control for any other factors that may influence ESRP provision in treated versus control states.

b. Methods

Auto-IRA mandates were rolled out at different times in different states (California, Oregon, and Illinois), and implementation within each state has also varied with firm size. Larger firms, measured in terms of their number of employees, have generally faced earlier enrollment deadlines relative to smaller firms, although firms of any size could voluntarily enroll employees in the state plan (or their own retirement plans) at any time. All three states' plans begin with a default contribution rate of 5 percent; however, contribution rates in Oregon and California can automatically increase by 1 percentage point each year until they reach 10 percent in Oregon and 8 percent in California. In Table 3, we summarize the rollout of each state's plan by firm size. We treat this staggered rollout as a series of policy experiments. We begin by combining all experiments and estimate aggregate treatment effects using an imputation approach (Gardner 2021; Borusyak et al. 2024). However, we also estimate models for each of the adopting states separately.

To estimate the impact of Auto-IRA legislation on firm ESRP offerings, we begin with a difference-in-differences specification comparing pre- and post-policy ESRP offerings in Auto-IRA states versus other states, using the CPS-ASEC data. More precisely, we estimate

(1) $Y_{ist} = \alpha AutoIRA_{st} + \mu_s + \tau_t + \boldsymbol{X_{ist}}\lambda + e_{ist}$,

where Y_{ist} is an indicator that takes on a value of 1 if individual i working in state *s* at time *t* works for an employer that offers an ESRP (or is included in an ESRP, depending on the specification), and zero otherwise. The key independent variable is $AutoIRA_{st}$, which takes on a value of 1 if state s has adopted Auto-IRA legislation in year t, and zero otherwise. Thus, α measures the increase in the probability of an individual's employer offering an ESRP, or the individual being included in an ESRP, associated with Auto-IRA legislation being adopted for any firms in the state. $\boldsymbol{X_{ist}}$ is a vector of individual-level demographic (age group, gender, race, and ethnicity) and education-level controls, as well as dummies for the size of the individual's employer. We also include state fixed effects, μ_s, to control for time-invariant factors affecting all individuals in a state and time dummies, τ_t, to control for any economy-wide factors that affect all individuals during the year. The time dummies additionally control for any year-to-year differences in the CPS that affect all respondents. For example, some studies suggest that a change to the CPS in 2013 has resulted in the underreporting of retirement plan participation – see, e.g., Copeland (2015, 2019, 2020), Radpour et al. (2021), and Sabelhaus (2022). Provided that this mismeasurement affects the control and treatment groups equally, it should be absorbed by the year dummies and not affect estimates of the policy-related changes in firm offerings and worker participation. More generally, the identifying assumption behind this specification is that we have not omitted any time-varying, state-specific factors that influence the probability of having access to an employer-sponsored retirement plan.

Equation (1) does not differentiate between firms of different sizes, though we do include firm size category dummies in $\boldsymbol{X_{ist}}$, as mentioned above. This approach may be appropriate depending on the mechanism by which Auto-IRA legislation affects firm behavior. For example, if an Auto-IRA policy changes business norms, then even firms in the state not directly covered by the legislation may be incentivized to offer ESRPs. Moreover, smaller firms may adopt ESRPs as soon as the legislation goes into effect in anticipation of the fact that they will be affected later. However, if Auto-IRA legislation effectively imposes a tax (the administrative cost of facilitating payroll deductions) on firms not offering ESRPs, then firms may not respond until the law directly applies to them. In this scenario, we can exploit additional variation coming from the fact that the mandate went into effect at different times for different firm sizes. That is, we can estimate a triple differences specification:

(2) $Y_{igst} = \beta AutoIRA_{gst} + \pi_{st} + \lambda_{gt} + \gamma_{gs} + \boldsymbol{X_{igst}}\kappa + \varepsilon_{igst}$,

where Y_{igst} is an indicator for access to an employer sponsored retirement plan for individual i working at a firm in size category g in state s in time t. The key independent variable is $AutoIRA_{gst}$, which takes on a value of 1 if firm size category g in state s in year t is affected by Auto-IRA legislation, and zero otherwise. The coefficient β represents the change in the probability of the individual's employer offering a retirement plan, or the individual being included in a retirement plan, associated with the implementation of the employer mandate within the individual's state for the relevant firm size.

Equation (2) also includes controls. Specifically, $\boldsymbol{X_{igst}}$ is the vector of demographic and education controls discussed above. State-by-year fixed effects, π_{st}, control for both time-invariant and time-varying characteristics that affect all individuals, across firm size categories, in a state. Similarly, firm-size-by-year fixed effects, λ_{gt}, control for time-invariant and time-varying

characteristics that affect all individuals, across states, employed by firms in a size category. Finally, state-by-firm-size fixed effects, γ_{gs}, control for time-invariant characteristics affecting individuals working at firms in a combination of size category and state. The identifying assumption is that there is no time-varying, state- and firm-size specific omitted variable that affects the probability of having access to an employer-sponsored retirement plan.

To justify the identifying assumption behind equation (2), as well as to examine the dynamics of ESRP adoption, we estimate event study models replacing the $AutoIRA_{gst}$ indicator above with a set of relative time indicators:

(3) $Y_{igst} = \sum_{k=-13}^{5} \rho_k z_{gs,t-k} + \pi_{st} + \lambda_{gt} + \gamma_{gs} + \varepsilon_{igst}$.

In this model, $z_{gs,t-k}$ takes on a value of 1 if the individual's state s and firm size category g are affected by Auto-IRA legislation in period $t-k$ (i.e., it is the k-period lag of the Auto-IRA indicator) where k can range from -13 to +5.[9] This variable takes on a value of zero otherwise, including for all individuals in states that are not affected by Auto-IRA legislation. For example, consider $k = -5$. The associated indicator variable is $z_{gs,t+5}$, which takes on a value of 1 if firm size category g in state s will be affected by Auto-IRA legislation in five years. When $k = 0$, the associated indicator z_{gst} takes on a value of 1 in the year of implementation for firm size g in state s. If Auto-IRA legislation has a causal effect on having access to a retirement plan, and under the assumption that there are no anticipatory effects, then we would expect $\rho_k = 0$ for $k < 0$ and $\rho_k \neq 0$ for $k > 0$.

Compared to the CPS-ASEC data, the Form 5500 data have both advantages and disadvantages. As noted above, the CPS-ASEC data may record an increase in the probability of

[9] The policy was first implemented in 2017 for a subset of employers in Oregon; thus, there can be at most five years following implementation at event time zero. The most recent expansions occurred in 2022 for subsets of employers in California and Illinois; thus, there can be up to 13 years (2009-2021) before implementation.

having access to a retirement plan at work if individuals misreport their state Auto-IRA plan as an employer-sponsored retirement plan. Such misreporting is not a concern in the firm-level Form 5500 data. Using the Form 5500 data, we estimate the difference-in-differences specification:

(4) $Y_{jst} = \delta\, AutoIRA_{st} + q_j + \tau_t + \nu_{jst}$,

where Y_{jst} is the dependent variable for firm *j*, in state *s*, at time *t*. Dependent variables include an indicator for providing, stopping, or starting an ESRP (the extensive margin), as well as the winsorized number of plan participants. The key independent variable is $AutoIRA_{st}$, which takes on a value of 1 if state s has adopted Auto-IRA legislation in year t, and zero otherwise. We also include year fixed effects (τ_t) and firm fixed effects (q_j).[10] The coefficient δ provides the estimated impact of Auto-IRA legislation. The identifying assumption is that in the absence of Auto-IRA legislation, firms in adopting states would have a similar propensity to provide retirement plans compared to firms in non-adopting states.

Unfortunately, we do not have a measure of firm size in the Form 5500 data. Therefore, we are unable to estimate the equivalent of equation (2) using this dataset. However, we can estimate event studies using only state-level variation in treatment adoption:

(5) $Y_{jst} = \sum_{k=-11}^{5} \gamma_k z_{js,t-k} + q_j + \tau_t + \nu_{jst}$.

In this model, $z_{js,t-k}$ takes on a value of 1 if firm j's state, s, is affected by Auto-IRA legislation in period $t-k$ (i.e., it is the k-period lag of the Auto-IRA indicator) where k can range from -11 to +5.[11] This variable takes on a value of zero otherwise, including for all firms in states that are not affected by Auto-IRA legislation.

[10] Because we drop the small number of firms that switch states, the firm fixed effects perfectly identify each firm's state. Thus, we do not include additional state fixed effects in our specification.

[11] The earliest implementation date was 2017 (Oregon), allowing for a possible six post-policy years (indexed 0 through +5). The firm-level regressions include only state-level (not firm-size level) variation in adoption. The most recent state to adopt a policy for any firm size category was California in 2020. Thus, we can have up to 11 pre-policy years (2009-2019).

We estimate all regressions using two alternative control groups. First, we use all states other than the three treated states as a control group. Second, we use all blue states, as defined in the previous subsection. We also consider two alternative time periods. First, we consider the full period from 2009-2022. Second, we consider only data from 2015-2022, which provides a more symmetric window around the treatment. In the CPS-ASEC data, this restriction has the additional advantage of excluding years before the 2013 redesign of the survey. Standard errors are clustered at the state level in the CPS-ASEC data, and at the EIN level in the Form 5500 data.

Recent research suggests that traditional two-way fixed effects estimation of difference-in-differences and event study models, like those above, can result in biased estimates when treatment timing varies across cross-sectional units and treatment effects may be heterogenous over time (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Gardner, 2021; Borusyak et al. 2024). To summarize the concern, the causal effect in a traditional two-way fixed effects model (i.e., using OLS to estimate one of the equations above) derives from comparing treated units to both not-yet-treated units and earlier treated units, and the latter comparison can result in biased estimates in the presence of heterogeneous treatment effects.

We address this concern about staggered treatment in two ways. First, we estimate our baseline regressions using a recently developed imputation estimator that is valid in the presence of staggered adoption (Gardner 2021; Borusyak et al. 2024). Specifically, we use Gardner's (2021) two-stage difference-in-differences procedure, which involves estimating a first-stage regression using all never-treated and not-yet-treated observations.[12] The dependent variable in the first-stage regression is the dependent variable of interest. The independent variables include all controls

[12] The Gardner (2021) estimator is implemented using the Stata did2s package developed by Butts and Gardner (2022).

except the $AutoIRA_{st}$ or $AutoIRA_{gst}$ treatment indicators (or the $z_{gs,t-k}$ or $z_{js,t-k}$ event-time indicators in the case of the event studies). In the second stage, the residuals from the first regression are regressed on the treatment indicator. The measured treatment effect, therefore, is based on the portion of the post-policy change in the dependent variable that is not accounted for by a pre-treatment model that includes all controls. Gardner (2021) shows that this procedure delivers valid estimates of treatment effects in the presence of staggered adoption. For comparison, we also present results from traditional two-way-fixed effects models. Second, we estimate separate regressions for each treated state separately; that is, we drop observations for the other two treated states and estimate the equation using OLS. In these models, each treated state is compared separately with each control group. When the estimation is performed state-by-state, equations (1) and (4) no longer involve staggered treatment, as the treatment occurs in a single year for each state. Equations (2) and (3) still involve staggered treatment (by firm size), but there is less variation in treatment timing than when each adopting state is considered separately.

4. Results

a. Individual-Level Data (CPS)

Table 4 shows the results from estimating equation (1) using Gardner's (2021) two-stage difference-in-differences approach. For comparison, Table 5 shows corresponding results from a traditional two-way-fixed effects model. In the first column of these tables, the dependent variable is an indicator for whether the individual's employer offers an ESRP. In the second column, the dependent variable is an indicator for the individual being included in an ESRP. The top panel of each table uses all non-adopting states as controls; the second

panel uses only non-adopting blue states. These panels use the full sample from 2009-2022. The third and fourth panels present corresponding results for the shorter period, 2015-2022.

Table 4 suggests that, when all non-adopting states are used as controls over the full period from 2009-2022, the policy is associated with a 1.8 percentage point increase in the probability that an individual works for an employer offering an ESRP, and a 1.1 percentage point increase in the probability of being included in an ESRP. Relative to the treated states' pre-treatment means of these dependent variables, these coefficients represent increases of 4.5 percent (= 0.018/0.40) in the probability of working for an ESRP-offering employer and 3.4 percent (= 0.011/0.32) in the probability of being included in an ESRP. When blue states are used as controls, Table 4 suggests that the policy is associated with a 2.1 percentage point (5.3 percent) increase in the probability that an individual works for an employer offering an ESRP, and a 1.6 percentage point (4.9 percent) increase in the probability that an individual is included in an ESRP. When only observations from 2015-2022 are used, the impact of the policy on ESRP provision is quite similar. The impact of the policy on inclusion, however, is no longer statistically significant. The loss of significance may be due to the smaller sample size, as the point estimates are positive and of similar magnitude to those in the top two panels. Table 5 shows that the traditional two-way-fixed effects coefficients are similar in magnitude and significance.

The last six columns of Tables 4 and 5 also provide estimates for each treated state separately, using both groups of control states. These state-by-state results suggest a relatively strong policy impact in California and Illinois, but no statistically significant impact in Oregon. Results from the Gardner (2021) approach (Table 4) and traditional two-way fixed effects (Table 5) are (not surprisingly) identical as the treatment is not staggered when

estimating equation (1) for individual states. As discussed above, one important caveat to this analysis is that CPS respondents in treated states may mistake state Auto-IRAs for ESRPs and report that they are covered by an ESRP when they are in fact simply enrolled in the state Auto-IRA program. If employees mistake Auto-IRAs for ESRPs, then Table 5 overestimates the impact of the policy.

Table 6 shows our main results for the CPS, which come from estimating equation (2) using the Gardner (2021) approach, while Table 7 shows our results from estimating equation (2) using traditional two-way fixed effects. Like the previous two tables, Tables 6 and 7 include results using both control groups and time periods, for the three states combined, and for each state separately. Table 6 suggests that for the three treatment states combined, the policy is associated with a 2.7 percentage point (6.8 percent) increase in the probability of working for an employer that offers an ESRP and a 2.5 percentage point (7.7 percent) increase in the probability of being included in an ESRP. The coefficients remain positive and significant, though they vary in magnitude, when blue states are used as controls and when the sample is restricted to 2015-2022. When the three Auto-IRA states are considered separately, we estimate treatment effects that are consistently positive for California and Illinois. The treatment effect is negative (though small in magnitude) for Oregon in some specifications. However, most of the estimated coefficients for Oregon are positive and significant when the sample is restricted to 2015-2022. As noted above, however, these coefficients may overestimate the policy impact if CPS respondents mistake Auto-IRAs for ESRPs. (The form 5500 results in the following section, which are based on firms' legal obligation to file a form for each ESRP, are not subject to this bias.)

Figures 13 and 14 show event study plots from estimating equation (2) using Gardner's (2021) approach, with all never-treated states serving as controls. The figures suggest an absence of pre-trends and a treatment effect, beginning at the time of adoption, which is in line with the coefficient estimates in Tables 6. Figures 15 and 16 suggest similar results when never-treated blue states are used as controls. A concern with these event studies is that the validity of the Gardner (2021) approach relies on the assumption of the absence of anticipation effects. Anticipation effects are possible because Auto-IRA policies were announced, and in some cases litigated, years before implementation. If present, anticipation effects will effectively cause treated observations to be improperly included in the first stage regression. To test for anticipation effects, we follow the procedure suggested by Butts and Gardner (2022) and re-estimate the model after shifting the date of treatment forward by two years. Thus, only observations more than two years before treatment are included in the first stage. When the treatment date is shifted forward, we find that treatment effects do not begin until two years after the new treatment date, suggesting an absence of any anticipation effects.[13]

b. Firm-Level Data (Form 5500)

Next, we present estimates of equation (4) using, respectively, the Gardner (2021) imputation estimator (Table 8) and traditional two-way fixed effects (Table 9). Due to computational constraints, we select a 10 percent random sample of EINs from the 5500 data for the Gardner (2021) model, while we use the full sample for the traditional two-way fixed effects model. The top two panels of each table show results for the full 2009-2022 period, using all never-treated states and never-treated blue states, respectively, as controls. The bottom two

[13] These results are not shown but are available upon request.

panels show corresponding results for the 2015-2022 period. The results in Table 8 suggest that, using all never-treated states as controls, Auto-IRA policies are associated with an increase of 3.1 percentage points in the probability that a firm offers an ESRP. Relative to the treated states' pre-treatment dependent variable mean, this coefficient represents a 7.1 percent (= 0.0310 / 0.44) increase. Results are similar when blue states are used as a control, as well as when the sample period is restricted to 2015-2022. We also find a statistically significant 1.58 percentage point increase in the probability of starting an ESRP, which is about 40.7 percent (= 0.0158 / 0.039) increase from the pre-treatment mean. There is, however, no statistically significant increase in the probability of stopping an ESRP. Table 9 suggests that the traditional two-way fixed effects estimator produces similar results, although the coefficients for stopping a plan are sometimes statistically significant (but small), possibly due to the larger sample size. We also find increases in winsorized (0.5 percent) total participants ranging from 1.69 to 1.94 (5.5-6.3 percent relative to the pre-treatment dependent variable means) depending on the specification.[14]

Tables 10-12 show results from estimating equation (4) for each treated state separately. These state-by-state results are estimated using the traditional two-way fixed effects approach, as treatment is not staggered in these models. Table 10 suggests somewhat larger treatment effects in California, which has a lower base probability of ESRP offering compared to other states (Table 2). Table 12 suggests relatively smaller effects in Oregon. The results in Table 11 are surprising, showing a decrease in ESRP provision and participation in Illinois. However, we hesitate to conclude that Illinois' Auto-IRA policy crowded out

[14] Chalmers et al. (2022) report that the top five industries covered by the OregonSaves Auto-IRA program are food services, business support, health care, retail, and agriculture. We obtain similar results when we restrict our sample to these industries, suggesting that our treatment effect occurs among firms most directly affected by Auto-IRA mandates. These results are available upon request.

private ESRP offerings for two reasons. First, the policy is also associated with a statistically significant decrease in the probability of ending an ESRP. Second, the results are at odds with the results from the CPS data (Tables 4-7). As we discuss later in the context of the event study results, we suspect this anomalous result may be driven by state-specific trends in the form 5500 data.

Figures 17-20 show event study plots from estimating equation (5) using all never-treated states as controls, and including firm fixed effects. The equations are estimated using the Gardner (2021) approach on a 10 percent random sample of EINs. These plots suggest a clear increase in ESRP offering, starting, and participation in the initial two years after an Auto-IRA policy goes into effect. However, we observe decreases in post-policy years 3 and 4. These decreases are a compositional effect driven by Illinois. California's policy first went into effect in 2020. As California is the largest of the treated states, treatment effects at time 0, +1, and +2 (corresponding to 2020-22 in California), are dominated by this state. Illinois' policy went into effect in 2018, and Oregon's in 2017. Thus, the treatment effect at time +3 and +4 include only these two states. As Illinois is the larger state, the negative coefficients in these periods are driven by the negative association between the policy and ESRP provision in Illinois. The treatment effect at time +5 includes only Oregon's positive coefficient. Figures 21-24 show similar results when blue states are used as controls.

To explore the state-specific results more carefully, we present separate event studies for each state for offering and starting a plan, using all never-treated states as controls.[15] Since treatment is not staggered when states are considered individually, we use the traditional two-way fixed effects approach. Figures 25 and 26 show a post-policy increase in the probability

[15] Results for the other dependent variables and for blue state controls are available upon request.

of offering or starting a plan in California. While there is an upward trend in the probability of offering a plan prior to policy adoption, the increases in the post-policy periods – especially in period 2 – are large relative to that trend. There is no evidence of a pre-trend, and clear evidence of a post-policy increase, in the probability of starting an ESRP. Figures 27 and 28 suggest that the anomalous results for Illinois may be driven by a strong underlying trend in Illinois compared to controls. Thus, we do not think the estimates in Table 11 should be interpreted causally. Finally, Figures 29 and 30 suggest a post-policy increase in the probability of offering and starting an ESRP in Oregon. There is no evidence of pre-trends. The increase in the probability of starting an ESRP is temporary, which is consistent with the fact that the start variable represents a flow (the increase in the number of ESRPs), and the last expansion of Oregon's policy during the sample period occurred in 2019.

5. Discussion and Conclusion

In summary, we have examined the impact of state Auto-IRA mandates – requiring firms to either facilitate payroll deductions to fund a state-facilitated IRA for each employee or to provide an ESRP – on firm provision of ESRPs. Theoretically, Auto-IRA policies could either increase, decrease, or leave unchanged firms' propensity to provide ESRPs. Firms could terminate existing ESRPs, treating state-facilitated Auto-IRAs as a substitute for these offerings. Alternatively, the new legislation could prompt firms to adopt ESRPs by imposing administrative costs on those that do not offer ESRPs and changing business norms. Using rich individual- and firm-level datasets, and quasi-experimental methods, we find that Auto-IRA mandates have a positive and significant effect on the probability of employers offering

an ESRP, the probability that an employee is included in an ESRP, and the number of individuals who participate in ESRPs.

It is important to note that determining the impact of Auto-IRA policies on overall saving or welfare is beyond the scope of this paper. Existing studies find mixed results regarding the impact of auto-enrollment in ESRPs on overall saving (e.g., Beshears et al. 2022; Choukmane 2021). Moreover, determining welfare effects requires comparison to a benchmark for rational saving behavior over the life cycle (see Scott et al. 2023). Additionally, none of the states studied had finished implementing their Auto-IRA legislation by the last year included in this study. Thus, further research needs to be done to determine the ultimate impact of Auto-IRAs in these three states, as well as in other states where such legislation is being developed. With these caveats, however, these early results are relevant for policy makers who wish to understand the impact of Auto-IRA or similar legislation on firm behavior.

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Table 1: Summary Statistics, CPS-ASEC 2009-2022

	Full Sample		CA	IL	OR	Never-Treated States	Never-Treated Blue States
Variable	Mean	Std. Dev.	Mean	Mean	Mean	Mean	Mean
Dependent Variables							
Employer Offers Pension	0.41	0.49	0.37	0.45	0.43	0.41	0.43
Included in Pension	0.33	0.47	0.30	0.37	0.34	0.33	0.35
Controls							
Age 25-34	0.35	0.48	0.36	0.35	0.36	0.35	0.35
Age 35-44	0.33	0.47	0.33	0.33	0.34	0.33	0.32
Age 45-54	0.32	0.47	0.31	0.32	0.30	0.32	0.32
Female	0.45	0.50	0.43	0.46	0.45	0.46	0.46
Non-Hispanic White	0.61	0.49	0.37	0.63	0.76	0.65	0.63
Hispanic	0.19	0.39	0.39	0.18	0.13	0.16	0.15
Non-Hispanic Black	0.11	0.31	0.05	0.12	0.02	0.12	0.10
Non-Hispanic Asian	0.07	0.25	0.16	0.07	0.05	0.05	0.09
Non-Hispanic Other	0.02	0.15	0.03	0.01	0.04	0.02	0.03
Less than High School	0.09	0.28	0.14	0.07	0.08	0.08	0.07
High School	0.27	0.44	0.23	0.25	0.25	0.28	0.25
Some College	0.27	0.45	0.25	0.25	0.31	0.28	0.24
College	0.25	0.43	0.25	0.28	0.26	0.25	0.28
More than College	0.12	0.33	0.13	0.15	0.11	0.12	0.16
Employer Size < 10	0.22	0.41	0.25	0.18	0.24	0.21	0.21
Employer Size 10-99	0.24	0.43	0.25	0.23	0.24	0.24	0.24
Employer Size 100+	0.55	0.50	0.50	0.59	0.51	0.55	0.55
Observations	647,399		67,891	20,612	10,242	548,654	175,189

Notes: Authors' calculations using CPS-ASEC data. Sample includes all private-sector workers from ages 25-54. Treated states include CA, IL, and OR. Never-treated states include all other states except CT and MD. Never-treated blue states are Democratic-leaning states, other than CT and MD, as defined by Rachkin (2021).

Table 2: Summary Statistics, Data Aggregated at Firm Level 2009-2021

	Observations	Mean	Std. Dev.	Min.	Max.
		Full Sample			
Offers Plan	17,860,276	0.48	0.50	0	1
Starts Plan	16,584,542	0.04	0.20	0	1
Stops Plan	16,584,542	0.04	0.19	0	1
Total Participants	17,811,687	96.45	5281.94	0	12,300,000
Winsorized (0.5%) Total Participants	17,811,687	40.09	191.58	0	2,108
		California			
Offers Plan	3,002,384	0.44	0.50	0	1
Starts Plan	2,787,928	0.05	0.21	0	1
Stops Plan	2,787,928	0.04	0.19	0	1
Total Participants	2,991,738	58.65	2,411.21	0	3,358,323
Winsorized (0.5%) Total Participants	2,991,738	28.08	154.73	0	2,108
		Illinois			
Offers Plan	755,832	0.52	0.50	0	1
Starts Plan	701,844	0.04	0.19	0	1
Stops Plan	701,844	0.04	0.19	0	1
Total Participants	753,019	125.94	2,953.78	0	481843
Winsorized (0.5%) Total Participants	753,019	50.09	224.91	0	2,108
		Oregon			
Offers Plan	269,752	0.49	0.50	0	1
Starts Plan	250,484	0.04	0.20	0	1
Stops Plan	250,484	0.04	0.19	0	1
Total Participants	269,032	58.78	941.14	0	294087
Winsorized (0.5%) Total Participants	269,032	36.08	174.18	0	2,108
		Never-Treated States			
Offers Plan	13,832,308	0.48	0.50	0	1
Starts Plan	12,844,286	0.04	0.20	0	1
Stops Plan	12,844,286	0.04	0.19	0	1
Total Participants	13,797,898	103.77	5,853.22	0	12,300,000
Winsorized (0.5%) Total Participants	13,797,898	42.23	196.96	0	2108
		Never-Treated Blue States			
Offers Plan	4,977,728	0.50	0.50	0	1
Starts Plan	4,622,176	0.04	0.20	0	1
Stops Plan	4,622,176	0.04	0.19	0	1
Total Participants	4,959,588	119.98	6,642.01	0	3,128,161
Winsorized (0.5%) Total Participants	4,959,588	41.78	199.95	0	2,108

Note: Authors' calculations based on Form 5500 data aggregated to EIN level. Never-treated states include all states except CA, IL, OR, CT, and MD. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Table 3: Rollout of Auto-IRA Legislation by State and Firm Size

State	Firm Size	Effective Date
California (CalSavers)	100+ employees	September 30, 2020
	50-99 employees	June 30, 2021
	5-49 employees	June 30, 2022
	1-4 employees	December 31, 2025
Illinois (Illinois Secure Choice)	500+ employees	November 1, 2018
	100-499 employees	July 1, 2019
	25-99 employees	November 1, 2019
	16-24 employees	November 1, 2022
	5-15 employees	November 1, 2023
Oregon (OregonSaves)	100+ employees	November 15, 2017
	50-99 employees	May 15, 2018
	20-49 employees	December 15, 2018
	10-19 employees	May 15, 2019
	5-9 employees	November 15, 2019
	3-4 employees	March 1, 2023
	1-2 employees	July 31, 2023

Sources: Center for Retirement Initiatives, Georgetown University, https://cri.georgetown.edu/wp-content/uploads/2022/04/State-Programs-Employer-Implementation-Timeline-Gantt-Chart.pdf; Oregon Saves Frequently Asked Questions, https://www.oregonsaves.com/faqs/employer?tag=timing

Table 4: Difference-in-Differences Estimates of Auto-IRA Legislation on Worker-Level ESRP Availability and Participation, CPS ASEC 2009-2022 (Imputation Estimator)

	CA, IL, OR		CA Only		IL Only		OR Only	
VARIABLES	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP
				2009-2022				
				Controls: All Never-Treated States				
$AutoIRA_{ist}$	0.018***	0.011**	0.020***	0.011***	0.025***	0.020***	-0.003	-0.005
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	647,399	647,399	616,545	616,545	569,266	569,266	558,896	558,896
				Controls: Never-Treated Blue States				
$AutoIRA_{ist}$	0.021***	0.016***	0.024***	0.016***	0.032***	0.029***	0.001	0.001
	(0.006)	(0.006)	(0.005)	(0.005)	(0.007)	(0.006)	(0.007)	(0.006)
Observations	273,934	273,934	243,080	243,080	195,801	195,801	185,431	185,431
				2015-2022				
				Controls: All Never-Treated States				
$AutoIRA_{ist}$	0.017**	0.008	0.011***	0.001	0.036***	0.026***	0.000	0.001
	(0.008)	(0.007)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
Observations	359,165	359,165	342,132	342,132	315,208	315,208	310,163	310,163
				Controls: Never-Treated Blue States				
$AutoIRA_{ist}$	0.021**	0.013	0.015***	0.005	0.046***	0.037***	0.008	0.010
	(0.009)	(0.009)	(0.004)	(0.003)	(0.008)	(0.006)	(0.008)	(0.008)
Observations	144,781	144,781	127,748	127,748	100,824	100,824	95,779	95,779

Note: Authors' calculations based on CPS-ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, and controls for education and demographics. Standard errors clustered by state. The treatment states' pre-treatment means of the probability of working for an ESRP-offering employer is 0.4 and that of probability of being included in an ESRP is 0.32. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Difference-in-Differences Estimates of Auto-IRA Legislation on Worker-Level ESRP Availability and Participation, CPS ASEC 2009-2022 (Two-Way Fixed Effects Estimator)

	CA, IL, OR		CA Only		IL Only		OR Only	
VARIABLES	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP
			2009-2022					
			Controls: All Never-Treated States					
$AutoIRA_{ist}$	0.018***	0.011**	0.020***	0.011***	0.025***	0.020***	-0.003	-0.005
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	647,399	647,399	616,545	616,545	569,266	569,266	558,896	558,896
			Controls: Never-Treated Blue States					
$AutoIRA_{ist}$	0.020***	0.015**	0.024***	0.016***	0.032***	0.029***	0.001	0.001
	(0.007)	(0.006)	(0.006)	(0.005)	(0.007)	(0.006)	(0.007)	(0.006)
Observations	273,934	273,934	243,080	243,080	195,801	195,801	185,431	185,431
			2015-2022					
			Controls: All Never-Treated States					
$AutoIRA_{ist}$	0.015**	0.006	0.011***	0.001	0.036***	0.026***	0.000	0.001
	(0.007)	(0.006)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Observations	359,165	359,165	342,132	342,132	315,208	315,208	310,163	310,163
			Controls: Never-Treated Blue States					
$AutoIRA_{ist}$	0.018**	0.010	0.015***	0.005	0.047***	0.037***	0.008	0.010
	(0.008)	(0.008)	(0.004)	(0.004)	(0.008)	(0.007)	(0.008)	(0.008)
Observations	144,781	144,781	127,748	127,748	100,824	100,824	95,779	95,779

Note: Authors' calculations based on CPS-ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, and controls for education and demographics. Standard errors clustered by state. The treatment states' pre-treatment means of the probability of working for an ESRP-offering employer is 0.4 and that of probability of being included in an ESRP is 0.32. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Triple Difference Estimates of Auto-IRA Legislation on Worker-Level ESRP Availability and Participation, CPS ASEC 2009-2022 (Imputation Estimator)

	CA, IL, OR		CA Only		IL Only		OR Only	
VARIABLES	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP
				2009-2022				
				Controls: All Never-Treated States				
$AutoIRA_{igst}$	0.027***	0.025**	0.040***	0.033***	0.024***	0.038***	-0.008***	-0.027***
	(0.009)	(0.010)	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.001)
Observations	647,399	647,399	616,545	616,545	569,266	569,266	558,896	558,896
				Controls: Never-Treated Blue States				
$AutoIRA_{igst}$	0.035***	0.042***	0.031***	0.026***	0.029***	0.048***	-0.015***	-0.020***
	(0.011)	(0.011)	(0.003)	(0.004)	(0.008)	(0.009)	(0.003)	(0.002)
Observations	273,934	273,934	243,080	243,080	195,801	195,801	185,431	185,431
				2015-2022				
				Controls: All Never-Treated States				
$AutoIRA_{igst}$	0.036***	0.037***	0.048***	0.042***	0.015***	0.026***	0.036***	0.043***
	(0.009)	(0.005)	(0.003)	(0.003)	(0.005)	(0.005)	(0.002)	(0.002)
Observations	359,165	359,165	342,132	342,132	315,208	315,208	310,163	310,163
				Controls: Never-Treated Blue States				
$AutoIRA_{igst}$	0.023***	0.036***	0.019***	0.024***	0.018***	0.036***	-0.007***	0.031***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)	(0.008)	(0.002)	(0.002)
Observations	144,781	144,781	127,748	127,748	100,824	100,824	95,779	95,779

Note: Authors' calculations based on data from the CPS-ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, firm size dummies, their two-way interactions, and controls for education and demographics. Standard errors clustered by state. The treatment states' pre-treatment means of the probability of working for an ESRP-offering employer is 0.4 and that of probability of being included in an ESRP is 0.32. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Triple Difference Estimates of Auto-IRA Legislation on Worker-Level ESRP Availability and Participation, CPS ASEC 2009-2022 (Two-Way Fixed Effects Estimator)

	CA, IL, OR		CA Only		IL Only		OR Only	
VARIABLES	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP	Employer Offers ESRP	Included in ESRP
				2009-2022				
				Controls: All Never-Treated States				
$AutoIRA_{igst}$	0.017***	0.020***	0.025***	0.024***	0.013***	0.030***	-0.010	-0.046***
	(0.006)	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(0.006)	(0.005)
Observations	647,399	647,399	616,545	616,545	569,266	569,266	558,896	558,896
				Controls: Never-Treated Blue States				
$AutoIRA_{igst}$	0.018**	0.010	0.015***	0.005	0.047***	0.037***	0.008	0.010
	(0.008)	(0.008)	(0.004)	(0.004)	(0.008)	(0.007)	(0.008)	(0.008)
Observations	273,934	273,934	243,080	243,080	195,801	195,801	185,431	185,431
				2015-2022				
				Controls: All Never-Treated States				
$AutoIRA_{igst}$	0.011*	0.013**	0.020***	0.018***	0.002	0.018***	-0.005	-0.027***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)	(0.007)	(0.006)
Observations	359,165	359,165	342,132	342,132	315,208	315,208	310,163	310,163
				Controls: Never-Treated Blue States				
$AutoIRA_{igst}$	0.014*	0.017**	0.030***	0.028***	0.009	0.027***	-0.005	-0.033***
	(0.008)	(0.008)	(0.006)	(0.007)	(0.005)	(0.009)	(0.010)	(0.010)
Observations	144,781	144,781	127,748	127,748	100,824	100,824	95,779	95,779

Note: Authors' calculations based on data from the CPS ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, firm size dummies, their two-way interactions, and controls for education and demographics. Standard errors clustered by state. The treatment states' pre-treatment means of the probability of working for an ESRP-offering employer is 0.4 and that of probability of being included in an ESRP is 0.32. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Difference-in-Differences Effect of Auto-IRA Legislation on Firm ESRP Offering and Participant Counts, Form 5500 2009-2022 (Imputation Estimator)

VARIABLES	Offers ESRP	Starts ESRP	Stops ESRP	Total Participants (winsorized, 0.1%)	Total Participants (winsorized, 0.5%)	Total Participants (winsorized, 1%)
			2009-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0310***	0.0158***	0.000983	1.557	1.688**	1.408***
	(0.00321)	(0.000954)	(0.000834)	(1.573)	(0.667)	(0.433)
Observations	1,786,022	1,658,449	1,658,449	1,781,008	1,781,008	1,781,008
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0320***	0.0167***	6.36e-05	1.378	1.930**	1.601***
	(0.00367)	(0.00108)	(0.000966)	(1.805)	(0.764)	(0.495)
Observations	900,578	836,251	836,251	897,216	897,216	897,216
			2015-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0308***	0.0163***	0.000720	1.810	1.935***	1.610***
	(0.00313)	(0.000962)	(0.000839)	(1.528)	(0.648)	(0.422)
Observations	1,020,584	1,020,584	1,020,584	1,018,014	1,018,014	1,018,014
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0315***	0.0171***	6.39e-05	1.406	1.935***	1.650***
	(0.00350)	(0.00110)	(0.000976)	(1.713)	(0.725)	(0.470)
Observations	514,616	514,616	514,616	512,826	512,826	512,826

Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm. Treatment states' pre-treatment of the probability that a firm offers an ESRP is 0.44, the probability of starting an ESRP is 0.039, the probability of stopping an ESRP is 0.035, the number of the total participation is 67.5, and the winsorized number of total participation (0.5%) is 30.7. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Difference-in-Differences Effect of Auto-IRA Legislation on Firm ESRP Offering and Participant Counts, Form 5500 2009-2022 (Two-Way Fixed Effects Estimator)

VARIABLES	Offers ESRP	Starts ESRP	Stops ESRP	Total Participants (winsorized, 0.1%)	Total Participants (winsorized, 0.5%)	Total Participants (winsorized, 1%)
			2009-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0274***	0.0146***	0.00128***	1.813***	1.300***	1.073***
	(0.000991)	(0.000304)	(0.000263)	(0.483)	(0.208)	(0.134)
Observations	17,860,276	16,584,542	16,584,542	17,811,493	17,811,493	17,811,493
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0247***	0.0147***	0.000269	2.579***	1.655***	1.416***
	(0.00111)	(0.000340)	(0.000301)	(0.555)	(0.234)	(0.150)
Observations	9,005,696	8,362,432	8,362,432	8,973,255	8,973,255	8,973,255
			2015-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0265***	0.0155***	0.00166***	1.585***	1.076***	0.892***
	(0.000843)	(0.000352)	(0.000298)	(0.371)	(0.163)	(0.107)
Observations	10,205,872	10,205,872	10,205,872	10,180,917	10,180,917	10,180,917
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0268***	0.0160***	0.00125***	2.120***	1.252***	1.091***
	(0.000931)	(0.000392)	(0.000340)	(0.421)	(0.181)	(0.117)
Observations	5,146,112	5,146,112	5,146,112	5,129,118	5,129,118	5,129,118

Note: Authors' calculations based on the full sample of Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm. Treatment states' pre-treatment of the probability that a firm offers an ESRP is 0.44, the probability of starting an ESRP is 0.039, the probability of stopping an ESRP is 0.035, the number of the total participation is 67.5, and the winsorized number of total participation (0.5%) is 30.7. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Difference-in-Differences Effect of Auto-IRA Legislation in California on Firm ESRP Offering and Participant Counts, Form 5500 2009-2022 (Two-Way Fixed Effects Estimator)

VARIABLES	Offers ESRP	Starts ESRP	Stops ESRP	Total Participants (winsorized, 0.1%)	Total Participants (winsorized, 0.5%)	Total Participants (winsorized, 1%)
			2009-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0508***	0.0225***	0.00288***	2.031***	1.681***	1.492***
	(0.00115)	(0.000387)	(0.000321)	(0.503)	(0.221)	(0.145)
Observations	16,834,692	15,632,214	15,632,214	16,789,458	16,789,458	16,789,458
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0516***	0.0236***	0.00205***	2.966***	2.153***	1.939***
	(0.00132)	(0.000432)	(0.000369)	(0.604)	(0.259)	(0.168)
Observations	7,980,112	7,410,104	7,410,104	7,951,220	7,951,220	7,951,220
			2015-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0397***	0.0209***	0.00260***	1.827***	1.351***	1.204***
	(0.000999)	(0.000422)	(0.000347)	(0.406)	(0.181)	(0.120)
Observations	9,619,824	9,619,824	9,619,824	9,596,525	9,596,525	9,596,525
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0421***	0.0220***	0.00236***	2.487***	1.602***	1.463***
	(0.00114)	(0.000473)	(0.000400)	(0.485)	(0.211)	(0.138)
Observations	4,560,064	4,560,064	4,560,064	4,544,726	4,544,726	4,544,726

Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Difference-in-Differences Effect of Auto-IRA Legislation in Illinois on Firm ESRP Offering and Participant Counts, Form 5500 2009-2022 (Two-Way Fixed Effects Estimator)

VARIABLES	Offers ESRP	Starts ESRP	Stops ESRP	Total Participants (winsorized, 0.1%)	Total Participants (winsorized, 0.5%)	Total Participants (winsorized, 1%)
			2009-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	-0.0325***	-0.00341***	-0.00250***	1.616	0.322	-0.123
	(0.00205)	(0.000496)	(0.000496)	(1.258)	(0.525)	(0.328)
Observations	14,588,140	13,546,130	13,546,130	14,550,731	14,550,731	14,550,731
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	-0.0331***	-0.00284***	-0.00346***	2.459*	0.787	0.328
	(0.00215)	(0.000521)	(0.000519)	(1.303)	(0.542)	(0.339)
Observations	5,733,560	5,324,020	5,324,020	5,712,493	5,712,493	5,712,493
			2015-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	-0.0207***	-0.00152**	-0.00155**	0.988	0.275	-0.163
	(0.00172)	(0.000621)	(0.000613)	(1.008)	(0.431)	(0.271)
Observations	8,336,080	8,336,080	8,336,080	8,316,524	8,316,524	8,316,524
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	-0.0194***	-0.00105	-0.00184***	1.567	0.498	0.105
	(0.00181)	(0.000654)	(0.000643)	(1.044)	(0.443)	(0.279)
Observations	3,276,320	3,276,320	3,276,320	3,264,725	3,264,725	3,264,725

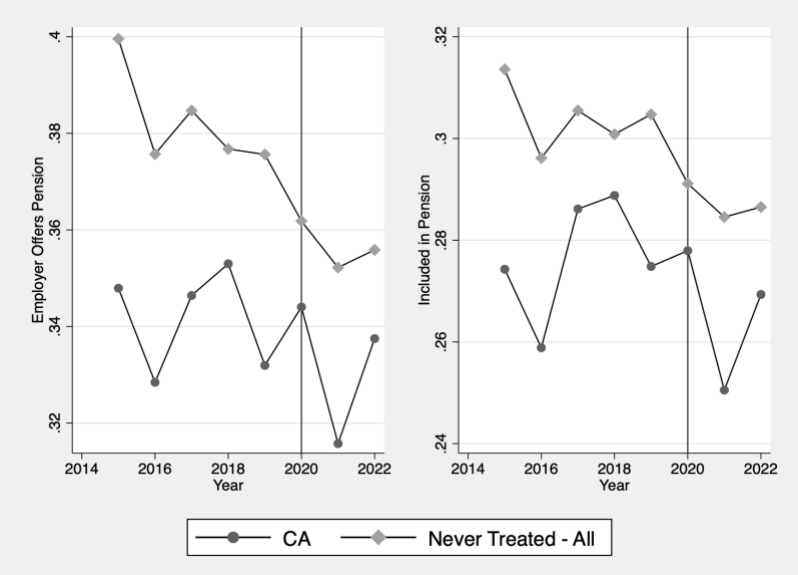
Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Difference-in-Differences Effect of Auto-IRA Legislation in Oregon on Firm ESRP Offering and Participant Counts, Form 5500 2009-2022 (Two-Way Fixed Effects Estimator)

VARIABLES	Offers ESRP	Starts ESRP	Stops ESRP	Total Participants (winsorized, 0.1%)	Total Participants (winsorized, 0.5%)	Total Participants (winsorized, 1%)
			2009-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0264***	0.00527***	0.000223	0.992	1.421**	1.415***
	(0.00343)	(0.000842)	(0.000783)	(1.140)	(0.568)	(0.402)
Observations	14,102,060	5,712,493	5,712,493	5,247,480	4,872,660	5,228,516
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0253***	0.00558***	-0.00102	1.835	1.901***	1.872***
	(0.00349)	(0.000856)	(0.000797)	(1.189)	(0.584)	(0.410)
Observations	5,247,480	4,872,660	4,872,660	5,228,516	5,228,516	5,228,516
			2015-2022			
			Controls: All Never-Treated States			
$AutoIRA_{ist}$	0.0241***	0.00431***	0.00163	0.671	0.519	0.634*
	(0.00296)	(0.00121)	(0.00108)	(0.756)	(0.454)	(0.337)
Observations	8,058,320	8,058,320	8,058,320	8,039,810	8,039,810	8,039,810
			Controls: Never-Treated Blue States			
$AutoIRA_{ist}$	0.0252***	0.00428***	0.000878	1.300	0.758	0.921***
	(0.00301)	(0.00123)	(0.00110)	(0.804)	(0.466)	(0.344)
Observations	2,998,560	2,998,560	2,998,560	2,988,011	2,988,011	2,988,011

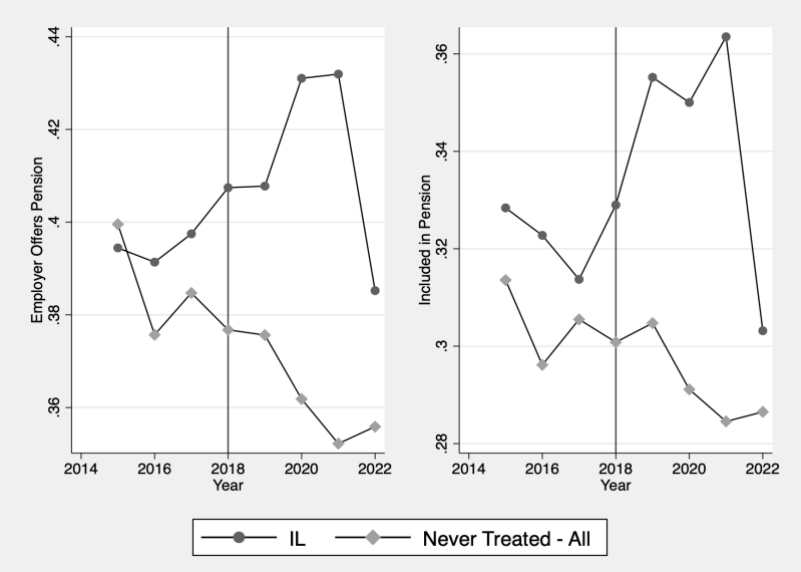
Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Raw Plots of ESRP Offering and Participation, CPS ASEC 2015-2022, California vs. Never-Treated States



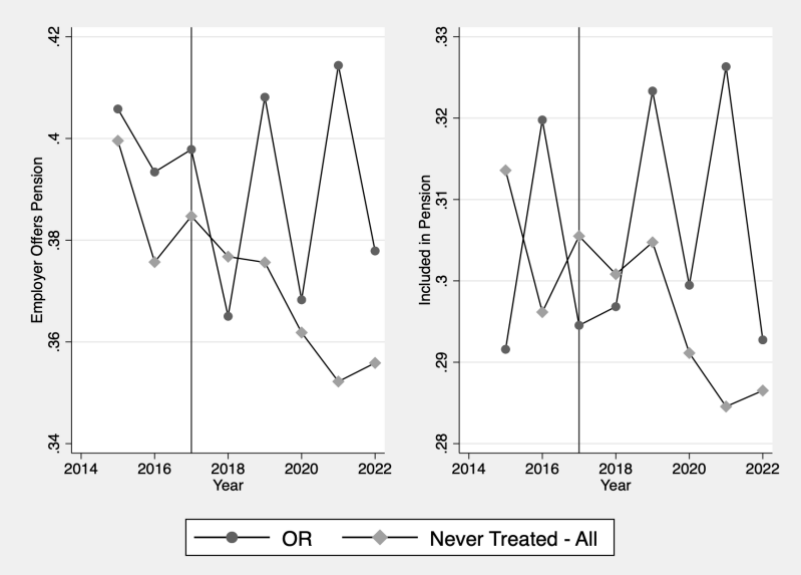
Notes: Authors' calculations using CPS-ASEC data. Sample includes all private sector workers from ages 25-54. Never-treated states include all states except CA, IL, OR, CT, and MD.

Figure 2: Raw Plots of ESRP Offering and Participation, CPS ASEC 2015-2022, Illinois vs. Never-Treated States



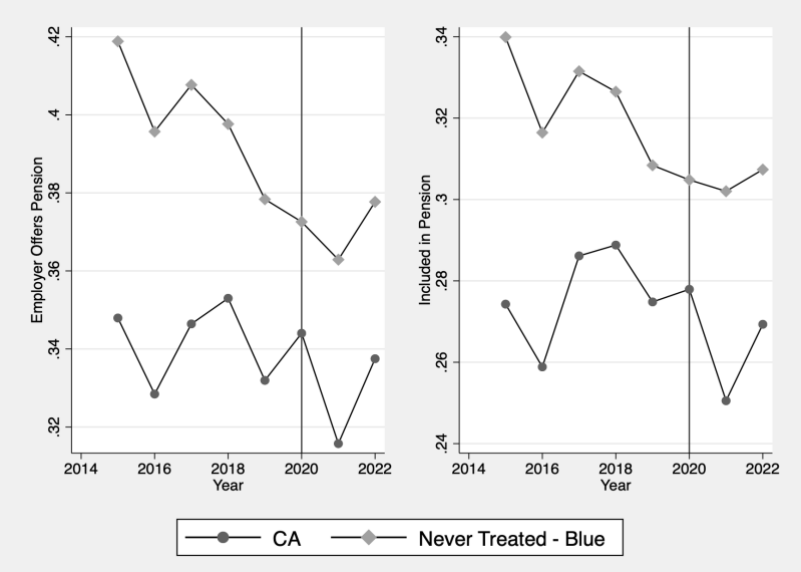
Notes: Authors' calculations using CPS-ASEC data. Sample includes all private sector workers from ages 25-54. Never-treated states include all states except CA, IL, OR, CT, and MD.

Figure 3: Raw Plots of ESRP Offering and Participation, CPS ASEC 2015-2022, Oregon vs. Never-Treated States



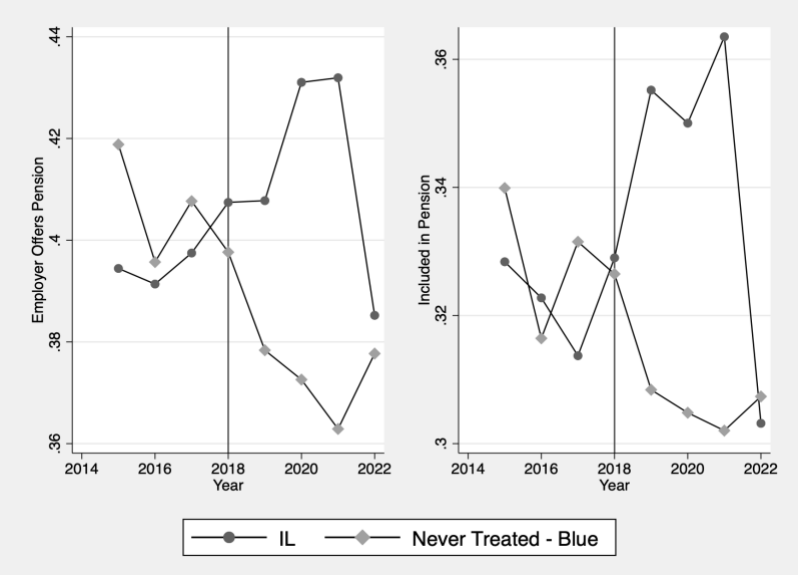
Notes: Authors' calculations using CPS-ASEC data. Sample includes all private sector workers from ages 25-54. Never-treated states include all states except CA, IL, OR, CT, and MD.

Figure 4: Raw Plots of ESRP Offering and Participation, CPS ASEC 2015-2022, California vs. Never-Treated Blue States



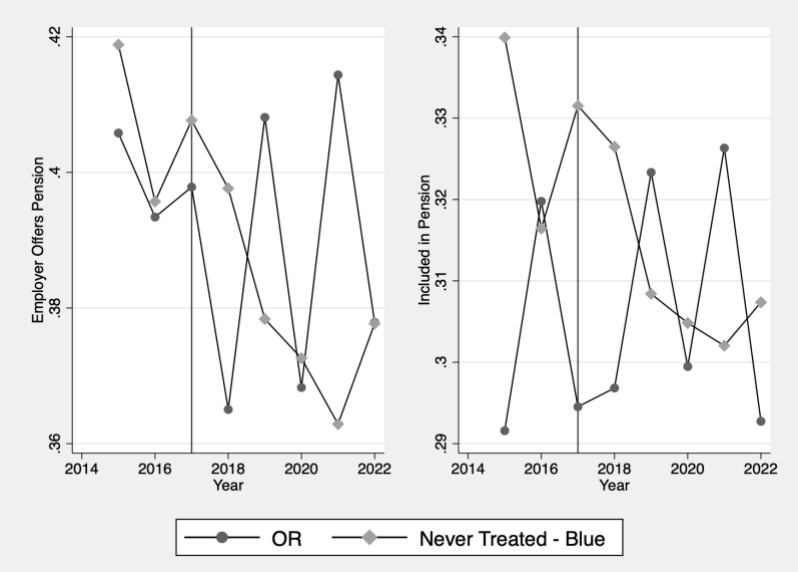
Notes: Authors' calculations using CPS-ASEC data. Sample includes all private sector workers from ages 25-54. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Figure 5: Raw Plots of ESRP Offering and Participation, CPS ASEC 2015-2022, Illinois vs. Never-Treated Blue States



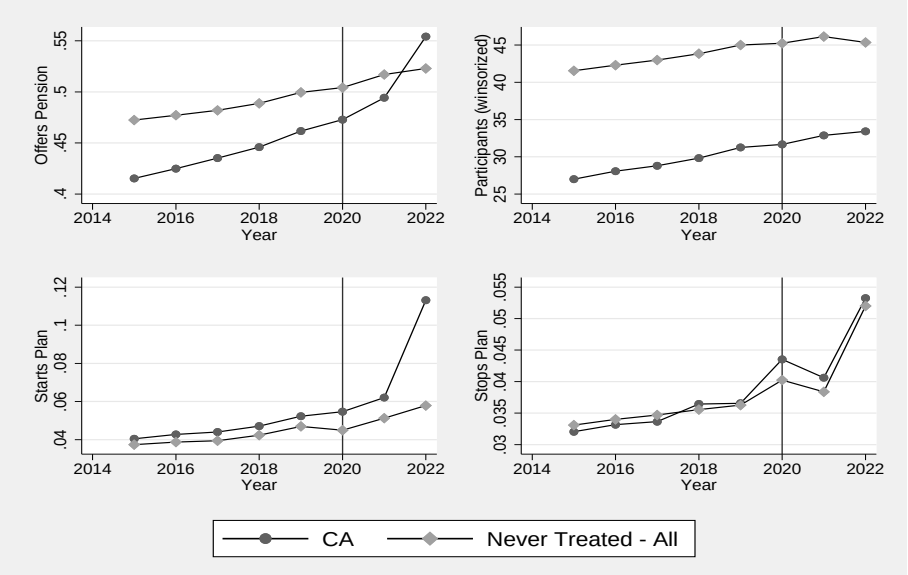
Notes: Authors' calculations using CPS-ASEC data. Sample includes all private sector workers from ages 25-54. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Figure 6: Raw Plots of ESRP Offering and Participation, CPS ASEC 2015-2022, Oregon vs. Never-Treated Blue States



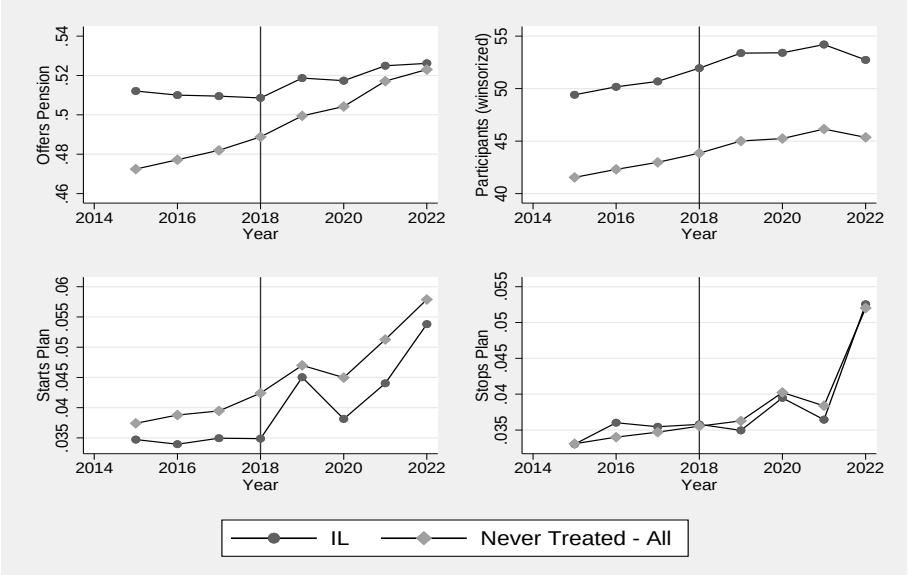
Notes: Authors' calculations using CPS-ASEC data. Sample includes all private sector workers from ages 25-54. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Figure 7: Raw Plots of Firm ESRP Offering and Participants, Form 5500 2015-2022, California vs. Never-Treated States



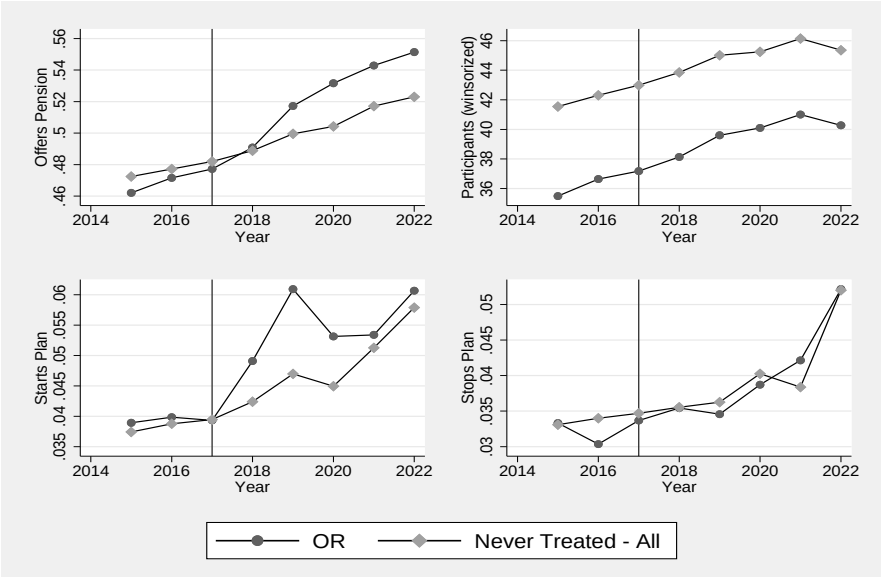
Notes: Authors' calculations using Form 5500 data aggregated to EIN level. Never-treated states include all states except CA, IL, OR, CT, and MD.

Figure 8: Raw Plots of Firm ESRP Offering and Participants, Form 5500 2015-2022, Illinois vs. Never-Treated States



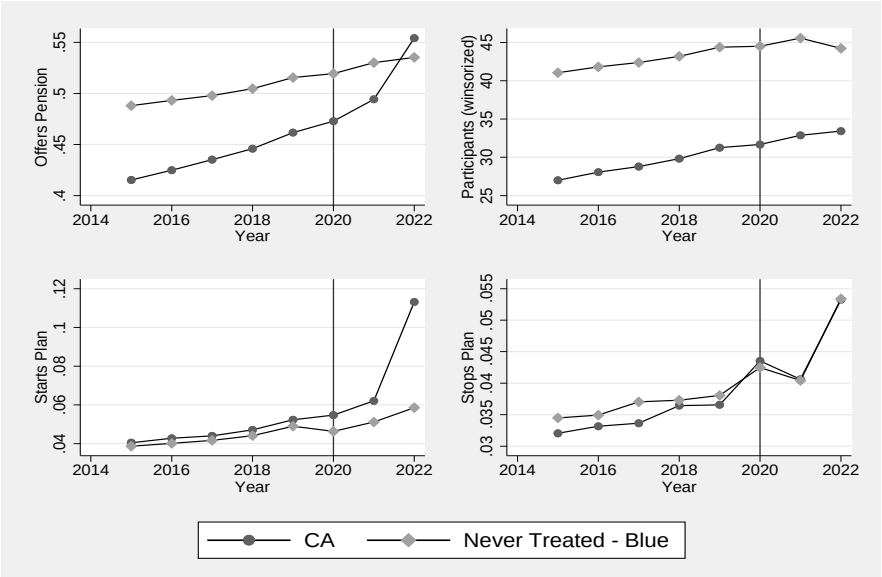
Notes: Authors' calculations using Form 5500 data aggregated to EIN level. Never-treated states include all states except CA, IL, OR, CT, and MD.

Figure 9: Raw Plots of Firm ESRP Offering and Participants, Form 5500 2015-2022, Oregon vs. Never-Treated Blue States



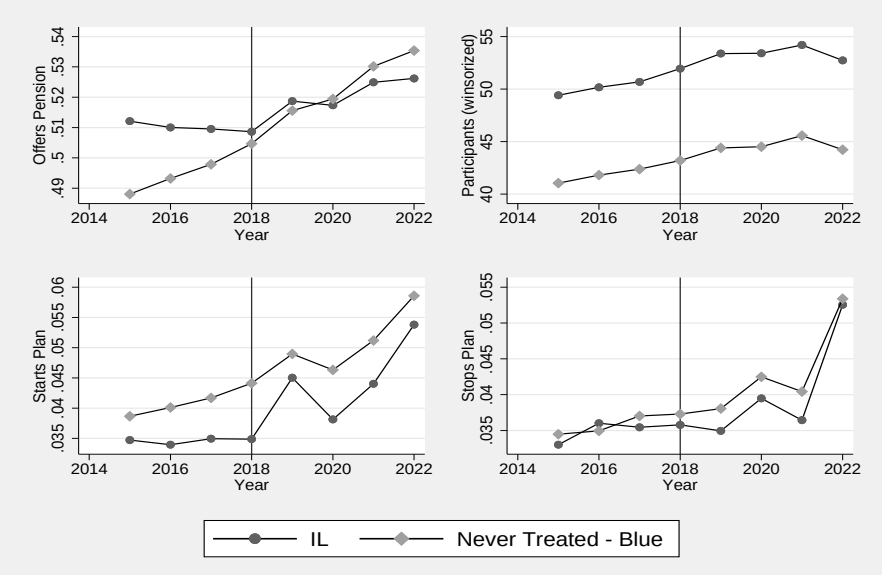
Notes: Authors' calculations using Form 5500 data aggregated to EIN level. Never-treated states include all states except CA, IL, OR, CT, and MD.

Figure 10: Raw Plots of Firm ESRP Offering and Participants, Form 5500 2015-2022, California vs. Never-Treated Blue States



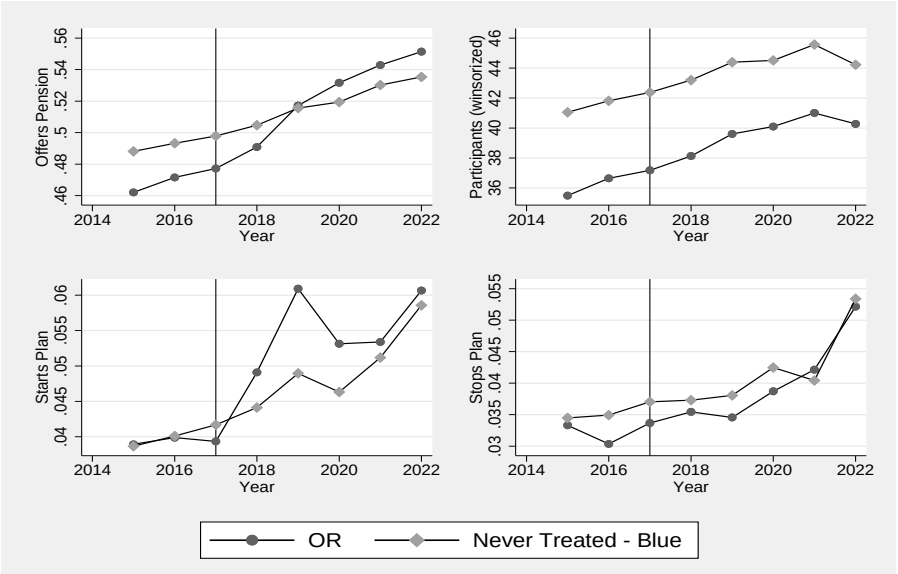
Notes: Authors' calculations using Form 5500 data aggregated to EIN level. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Figure 11: Raw Plots of Firm ESRP Offering and Participants, Form 5500 2015-2022, Illinois vs. Never-Treated Blue States



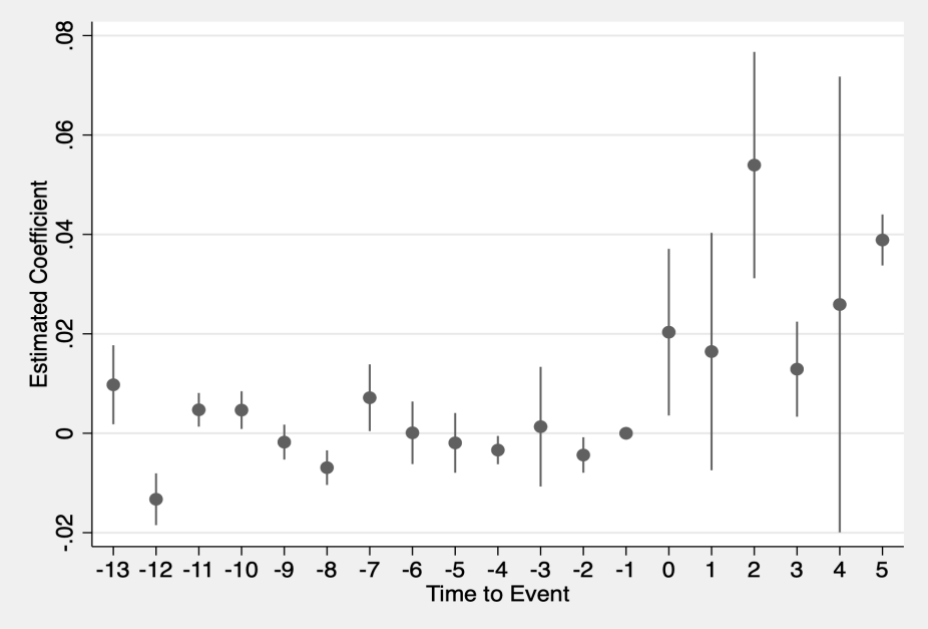
Notes: Authors' calculations using Form 5500 data aggregated to EIN level. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Figure 12: Raw Plots of Firm ESRP Offering and Participants, Form 5500 2015-2022, Oregon vs. Never-Treated Blue States



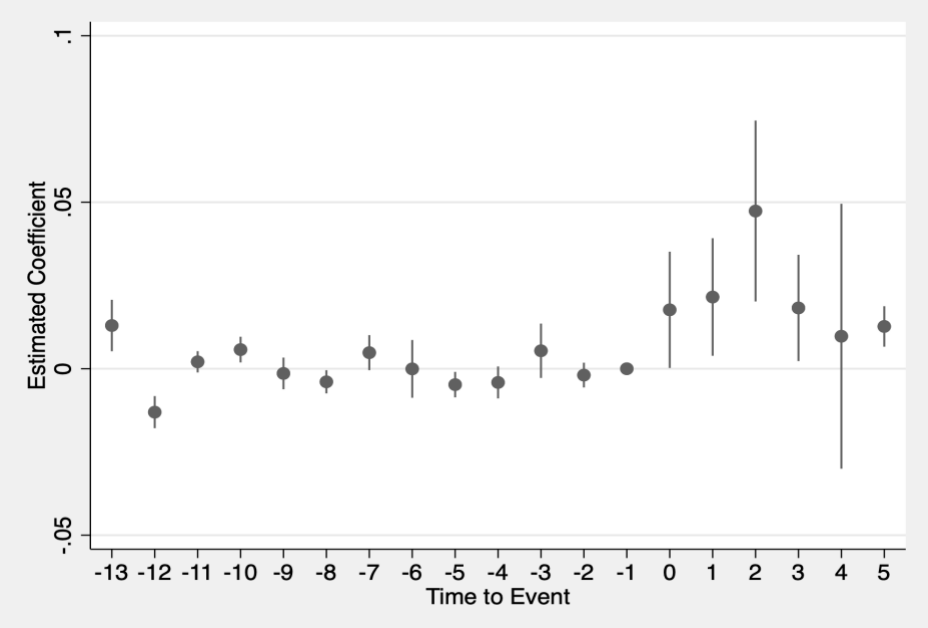
Notes: Authors' calculations using Form 5500 data aggregated to EIN level. Never-treated blue states are Democratic-leaning states, other than CA, IL, OR, CT, and MD, as defined by Rachkin (2021).

Figure 13: Event Study of the Effect of Auto-IRA Legislation on Worker-Reported ESRP Availability, CPS ASEC 2009-2022 (Treated vs. All Never-Treated States)



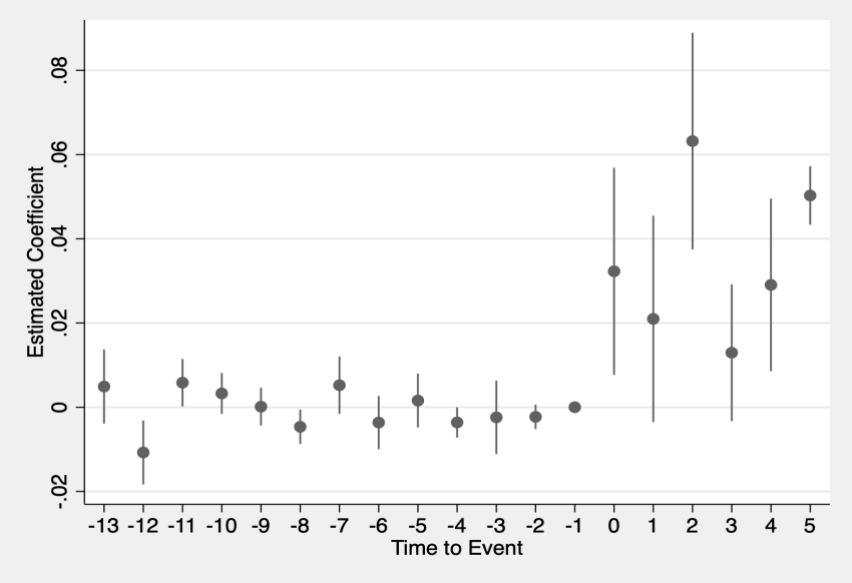
Notes: Authors' calculations based on data from the CPS ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, firm size dummies, their two-way interactions, and controls for education and demographics. Standard errors clustered by state.

Figure 14: Event Study of the Effect of Auto-IRA Legislation on Worker-Reported ESRP Participation, CPS ASEC 2009-2022 (Treated vs. All Never-Treated States)



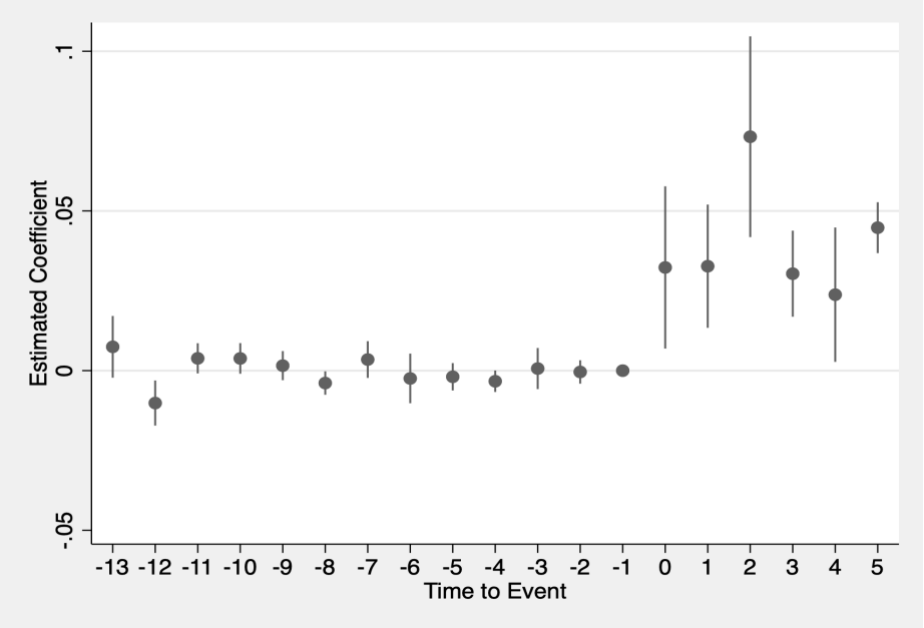
Notes: Authors' calculations based on data from the CPS ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, firm size dummies, their two-way interactions, and controls for education and demographics. Standard errors clustered by state.

Figure 15: Event Study of the Effect of Auto-IRA Legislation on Worker-Reported ESRP Availability, CPS ASEC 2009-2022 (Treated vs. Never-Treated Blue States)



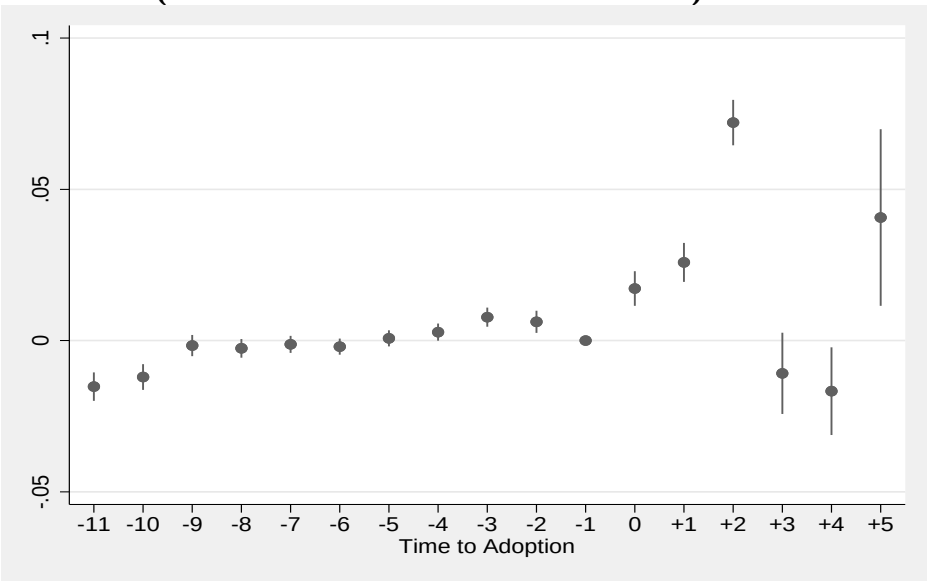
Notes: Authors' calculations based on data from the CPS ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, firm size dummies, their two-way interactions, and controls for education and demographics. Standard errors clustered by state.

Figure 16: Event Study of the Effect of Auto-IRA Legislation on Worker-Reported ESRP Participation, CPS ASEC 2009-2022 (Treated vs. Never-Treated Blue States)



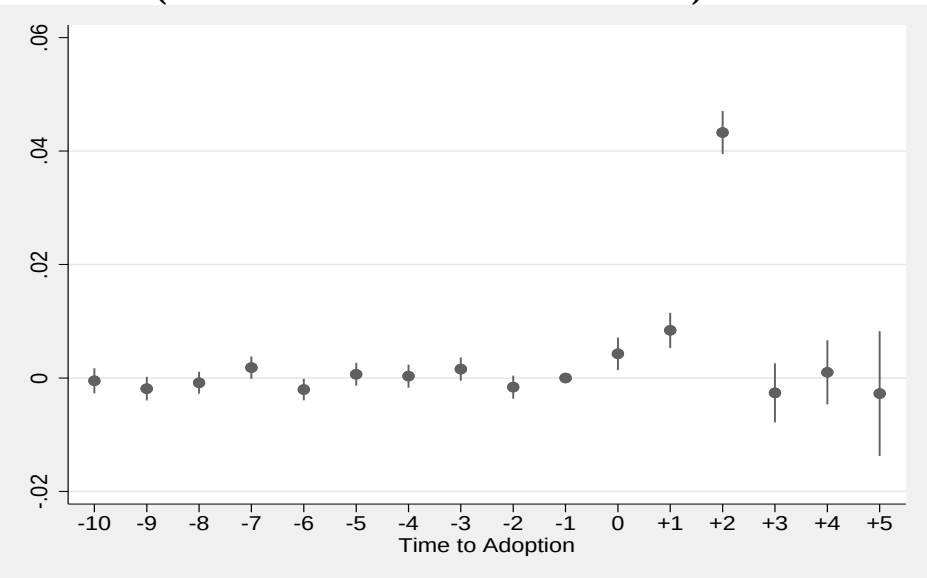
Notes: Authors' calculations based on data from the CPS ASEC. Sample is restricted to private sector employees with age between 25 and 54. Estimates are weighted by the ASEC weight provided by the U.S. Census Bureau. All regressions include state dummies, year dummies, firm size dummies, their two-way interactions, and controls for education and demographics. Standard errors clustered by state.

Figure 17: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Offering, Form 5500 2009-2022 (Treated vs. All Never-Treated States)



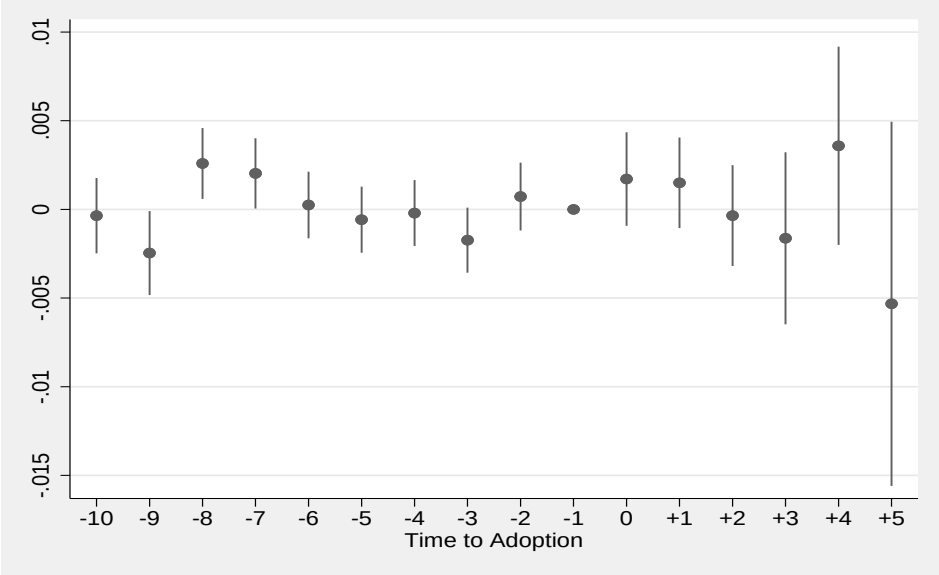
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 18: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Starting, Form 5500 2009-2022 (Treated vs. All Never-Treated States)



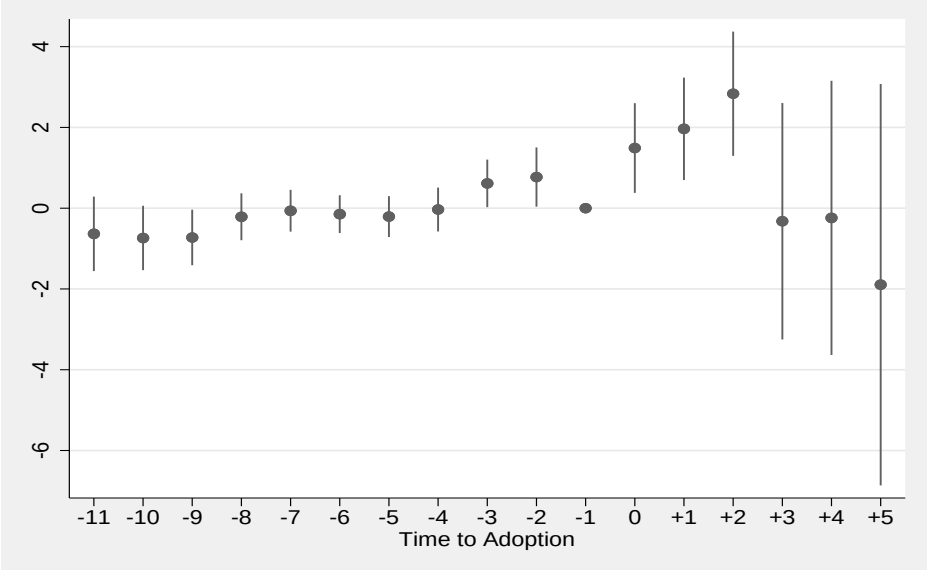
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 19: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Stopping, Form 5500 2009-2022 (Treated vs. All Never-Treated States)



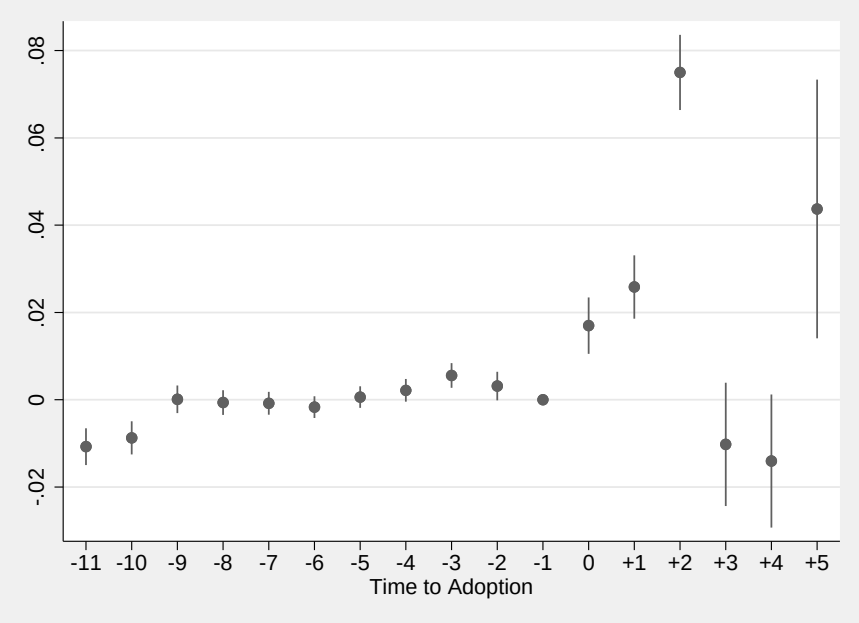
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 20: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Participants, Form 5500 2009-2022 (Treated vs. All Never-Treated States)



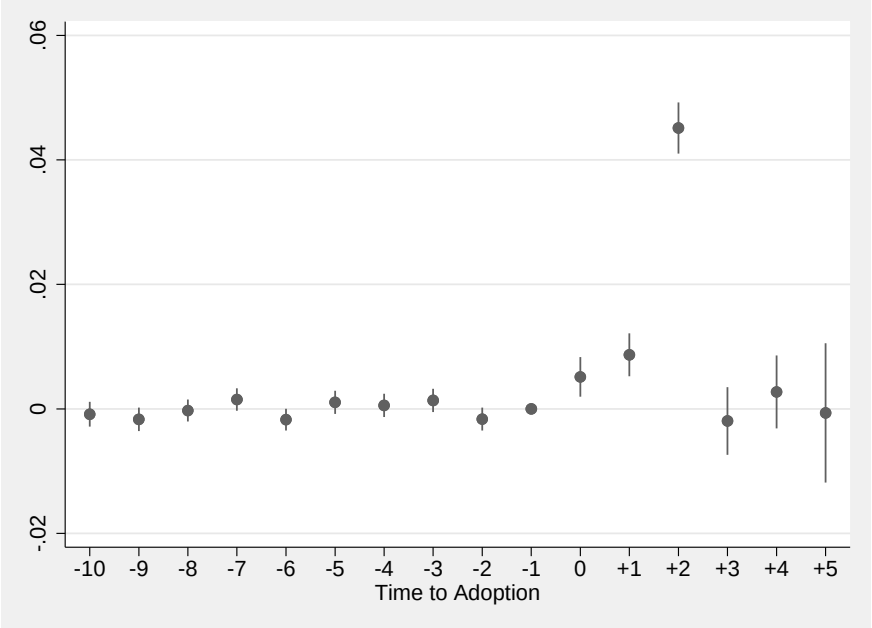
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 21: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Offering, Form 5500 2009-2022 (Treated vs. Never-Treated Blue States)



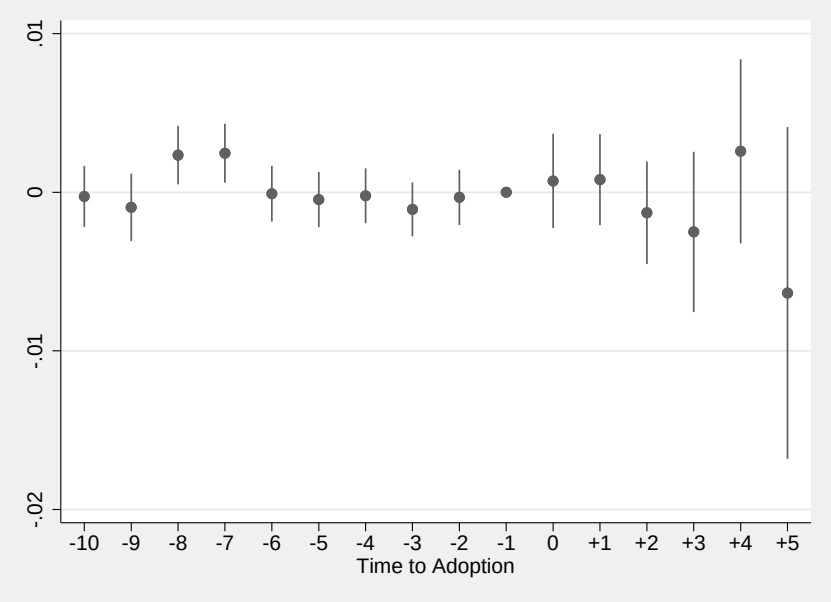
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 22: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Starting, Form 5500 2009-2022 (Treated vs. Never-Treated Blue States)



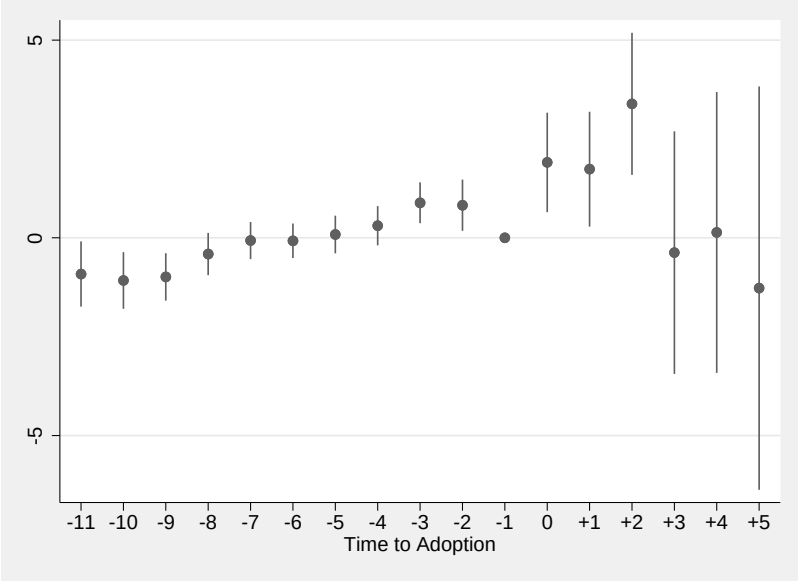
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 23: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Stopping, Form 5500 2009-2022 (Treated vs. Never-Treated Blue States)



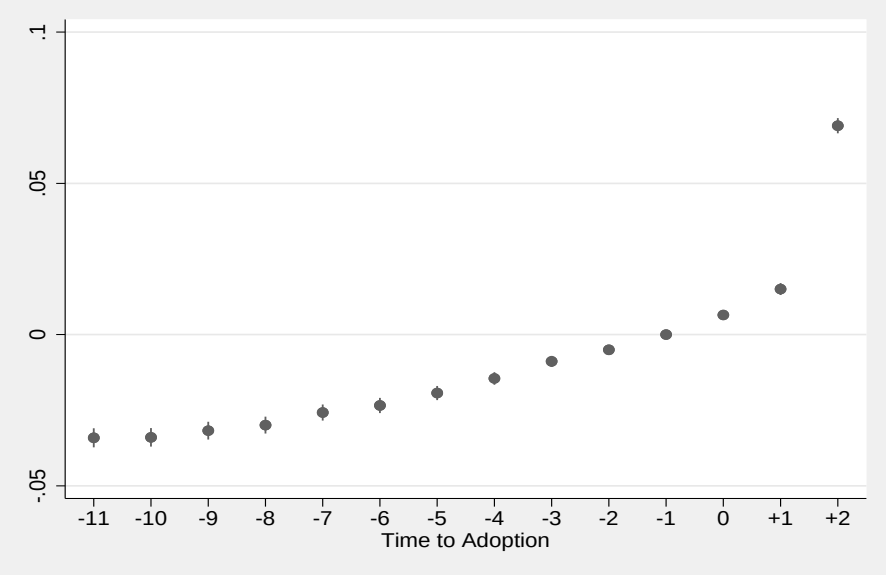
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 24: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Participants, Form 5500 2009-2022 (Treated vs. Never-Treated Blue States)



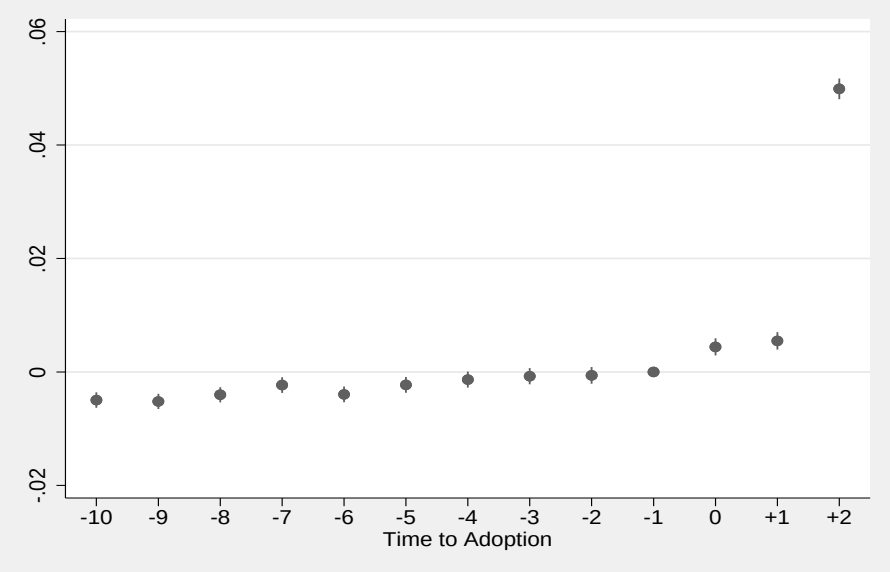
Note: Authors' calculations based on 10 percent random sample of EINs in Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 25: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Offering, Form 5500 2009-2022 (California vs. All Never-Treated States)



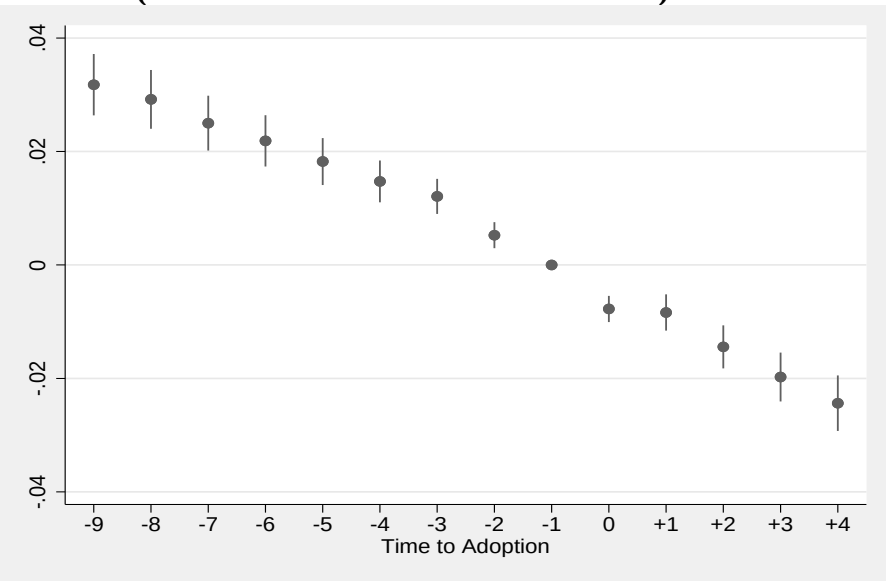
Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 26: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Starting, Form 5500 2009-2022 (California vs. All Never-Treated States)



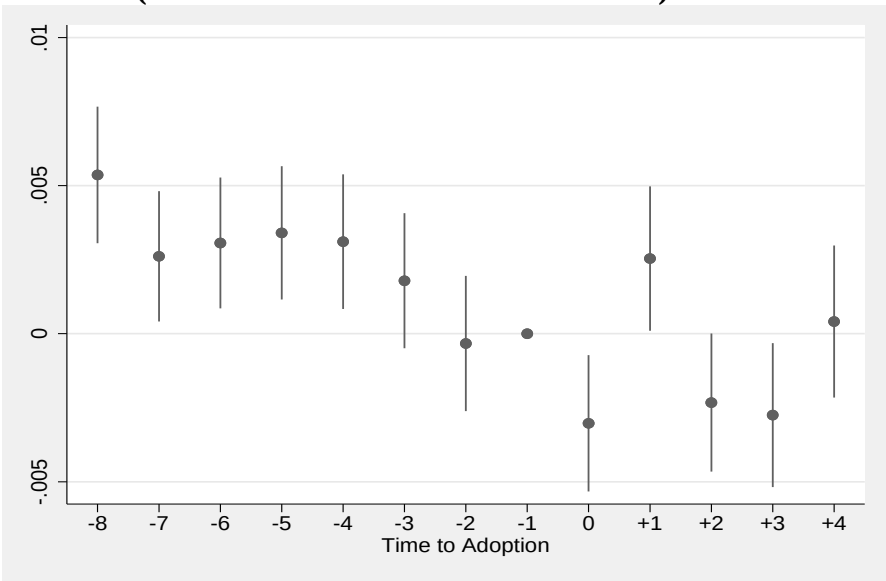
Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 27: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Offering, Form 5500 2009-2022 (Illinois vs. All Never-Treated States)



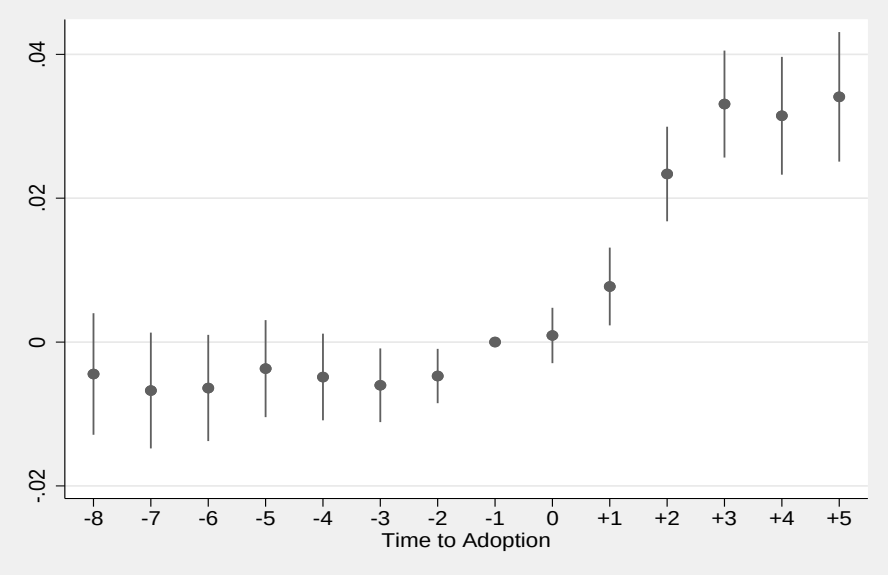
Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 28: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Starting, Form 5500 2009-2022 (Illinois vs. All Never-Treated States)



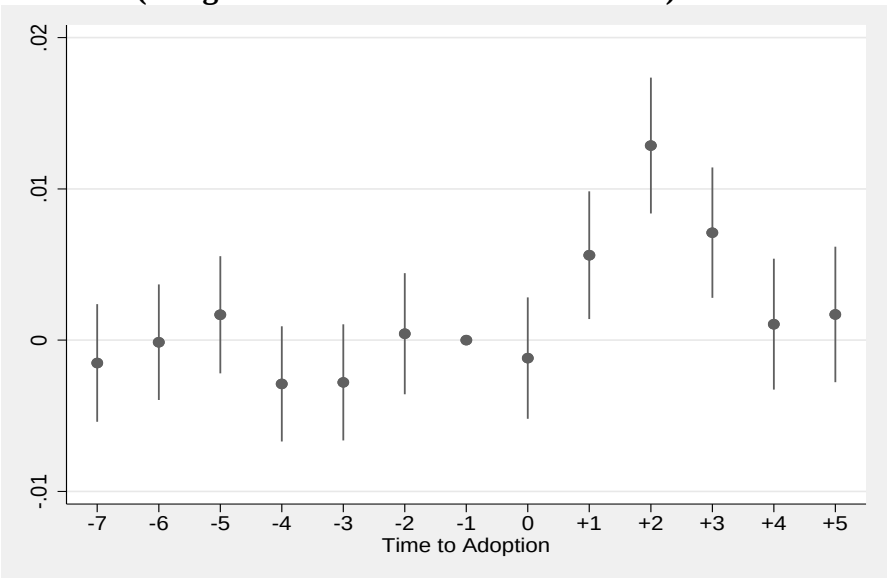
Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 29: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Offering, Form 5500 2009-2022 (Oregon vs. All Never-Treated States)



Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.

Figure 30: Event Study of the Effect of Auto-IRA Legislation on Firm ESRP Starting, Form 5500 2009-2022 (Oregon vs. All Never-Treated States)



Note: Authors' calculations based on Form 5500 data. All regressions also include firm and year dummies. Standard errors clustered by firm.