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Adam Bloomfield
Kyung Min Lee
Jay Philbrick
Sita Slavov

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ABSTRACT

This paper investigates the effect of state retirement plan mandates on the supply of employer-sponsored retirement plans (ESRPs) by firms. These policies require employers to either (1) offer ESRPs to workers or (2) facilitate automatic payroll deductions that are deposited into individual retirement accounts (IRAs) established for workers by the state. In this paper, we utilize individual-level data from the Current Population Survey (CPS) and firm-level data from Form 5500 filings to examine the effect of these automatic-enrollment IRA ("auto-IRA") policies on employer decisions to offer, and worker inclusion in, ESRPs. We exploit variation in the timing of implementation across states and firm size categories and estimate that auto-IRA policies increase the probability that an individual works for a firm with an ESRP by roughly 3 percent, and the probability that the individual participates in that ESRP by 33 percent. These policies also increase the number of ESRP participants at the average firm in our sample by 3-5 percent.

Adam Bloomfield
U.S. Federal Deposit Insurance Corporation (FDIC)
550 17th St NW
Washington, DC 20429
abloomfieldclagett@fdic.gov

Kyung Min Lee
The World Bank
1818 H St., NW
Washington, DC 20433
klee12@worldbank.org

Jay Philbrick
Brown University
69 Brown St., Mail# 7657
Providence, RI 02912
jay_philbrick@brown.edu

Sita Slavov
Schar School of Policy and Government
George Mason University
3351 Fairfax Drive, MS 3B1
Arlington, VA 22201
and NBER
sslavov@gmu.edu

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 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 may not do so.

In this paper, we examine how auto-IRA legislation in the three earliest-adopting states affects employer behavior. In particular, we examine whether auto-IRA laws affect employers' decisions to create or terminate ESRPs, as well as the extent to which workers

[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. I 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 sends an email to each employee notifying 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.

report changes in their participation in such plans. As we explain in more detail below, the implementation of state auto-IRA programs can either increase, decrease, or leave unaffected the fraction of firms offering ESRPs. In an attempt 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 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. Our results suggest that 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 offering an ESRP by around 3 percent, and the probability of participating in that ESRP by around 33 percent. These policies also raise the number of ESRP participants at the average firm by around 3-5 percent. Moreover, employees at these firms are more likely to report being included in these plans.

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. (2019), who show that participation rates range from 48 percent to 67 percent of eligible employees; Chalmers et al. (2021), 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 (2022), who shows that OregonSaves resulted in a 27 percent increase in owning an auto-IRA among Oregon workers and a 5 percent increase in participating in a retirement plan at work.

A handful of policy studies have also focused on how auto-IRA legislation 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 legislation (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 firm-level data from Form 5500 filings and individual-level data from the Current Population Survey (CPS) to examine the impact of auto-IRA rollout on firm ESRP offerings and worker access to ESRPs. 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. In addition, our use of nationally representative 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.

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 (Pauly 1986; Woodbury and Huang 1991; Gentry and Peress 1994), 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., Madrian and Shea 2001; Beshears et al. 2022) 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 is a function of both employer and worker decisions.

Additionally, our work broadly intersects with the literature on the impact 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, CBO (2010) projects 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 state policies only altered the enrollment default for a non-employment-based retirement savings option that already existed.

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 the 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 legislation 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 legislation 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 legislation by starting to offer their own ESRPs. If firms and workers are cognitively constrained, the social debates and litigation around auto-IRA laws may increase the salience of retirement plans to firms and workers who previously were 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 summarizes the new state auto-IRA plans for small business owners, then promotes its own small business 401(k) services, arguing, "Providing workers access to simple retirement savings vehicles is becoming an increasingly popular legislative priority."[5] Moreover, it is possible that the legislation 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 initial 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.

[5] See https://www.morganstanley.com/atwork/articles/state-mandated-retirement-programs-small-businesses.

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. The CPS is a monthly, nationally representative survey of U.S. households that provides basic labor force data. The CPS ASEC – conducted in March of each year – provides more detailed information on work experience, income, education, employer characteristics, and receipt of noncash benefits in the previous calendar year. Since the CPS ASEC provides information for the previous year, we use data for years 2010 through 2021 to analyze outcomes for the years between 2009 and 2020. We restrict the sample to adults between ages 25 and 54 who are employed in the private sector. Auto-IRA legislation only affects 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.

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 may mistakenly report their state IRA as an ESRP, making it appear that employers are more likely to offer plans in states with auto-IRA legislation. As we discuss below, we can address this concern by complementing our individual-level analysis with an analysis based on firm-level Form 5500 filings, which would not misreport state IRAs as ESRPs. Additional variables derived from the ASEC include the size of the respondent's employer (number of employees, reported in categories), as well as the respondents, race, ethnicity, and education level. As auto-IRA legislation was 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.[6]

Table 1 shows summary statistics for the ASEC sample both overall and broken down by whether the individual resides in a state that adopted auto-IRA legislation (treatment state) or a state that did not adopt IRA legislation (control state). There are racial differences between control and treatment states: treatment states tend to have a smaller share of individuals who are white or black and have a larger share of individuals who are Asian or

[6] 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.

Hispanic. However, education levels, employment status, the firm size distribution, and the share of people enrolled in employer-sponsored retirement plans seem similar across the two groups. 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.

Our employer level data come from Form 5500 filings for 2009 to 2021. Form 5500 and Form 5550 Short Form (Form 5500 SF) are filed annually by organizations that offer their employees a retirement plan 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. 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 employer identification number (EIN) for the sponsor, the year of filing, counts of total 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.

Firms only file Form 5500 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 between 2009 and 2020 existed

during the whole period. Thus, we generate a balanced panel by filling in zeroes for total participants, as well as the number of plans, for firms in years during which no filings were made. 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 adoption of auto-IRA legislation.

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 of a firm offering at least one retirement plan is similar between treated (0.54) and control (0.56) states. However, the average estimated number of participants at the EIN level is lower in treated (91) than control (124) states. Given the highly skewed distribution of employees per firm, when we estimate our regressions, we transform the number of participants by taking the natural log. This transformation requires excluding firms with zero participants. To avoid dropping these firms, a common workaround is to add a small, positive constant to each of the participant counts before taking the log (MaCurdy and Pencavel 1986).[7] However, this approach has been criticized as ad hoc.[8] An alternative approach is to take the inverse hyperbolic sine of the dependent variable, which also allows for the retention of zeros. As discussed by Bellemare and Wichman (2019), this transformation may be a reasonable approximation to the natural log when the mean of the untransformed dependent variable is

[7] This procedure is sometimes referred to as a "log(x+1) transformation".
[8] See, for example, https://worthwhile.typepad.com/worthwhile_canadian_initi/2011/07/a-rant-on-inverse-hyperbolic-sine-transformations.html.

greater than 10 (as it is in this case). We use all three approaches and compare results. First, we take the natural log of the dependent variable, dropping firms with zero participants. Next, we add 1 to the participant count at each firm and take the natural log. Finally, we take the inverse hyperbolic sine of the number of participants. These transformed dependent variables are also included in Table 2.

b. Methods

Our identification strategy relies on the fact that auto-IRA mandates were rolled out at different times in different states (California, Oregon, and Illinois), and that implementation within each state 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 percent 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.

To estimate the impact of auto-IRA legislation on firm ESRP offerings, we begin with a difference-in-differences (DiD) specification comparing pre- and post-policy ESRP offerings in auto-IRA states versus other states. More precisely, we estimate

(1) $Y_{ist} = \alpha_1 AutoIRA_{st} + \alpha_2 \pi_s + \alpha_3 \tau_t + \boldsymbol{X_{ist}} \lambda + e_{ist}$,

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_1 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. Depending on the specification, it could include the size of the individual's employer. We also include state fixed effects π_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 2014 has resulted in the underreporting of retirement plan participation (e.g., Radpour et al. 2021; Investment Company Institute 2019). 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. For all regressions using CPS ASEC data, standard errors are clustered at the state level.

Equation (1) does not differentiate between firms of different sizes, though in some specifications we do include firm size 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 (DDD) specification:

(2) $Y_{igst} = \beta_1 AutoIRA_{gst} + \beta_2\gamma_g + \beta_3\pi_s + \beta_4\tau_t + \beta_5(\gamma_g \cdot \pi_s) + \beta_6(\gamma_g \cdot \tau_t) + \beta_7(\pi_s \cdot \tau_t) + \boldsymbol{X_{igst}}\kappa + \varepsilon_{igst}$,

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_1 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. In addition, we include firm size fixed effects γ_g; state fixed effects π_s; year fixed effects τ_t; and two-way interactions between size and state, size and year, and state and year. The inclusion of state fixed effects π_s and their interaction with the time dummies $(\pi_s \cdot \tau_t)$ control for both time-invariant and time-varying characteristics that affect all individuals, across firm size categories, in a state. Similarly, the inclusion of firm size fixed effects γ_g and their interaction with the time dummies $(\gamma_g \cdot \tau_t)$ control for time-invariant and time-varying characteristics that affect all individuals, across states, employed by firms in a size category. Finally, the interactions between the state and firm-size effects $(\gamma_g \cdot \pi_s)$ 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), we examine pre-trends by estimating an event study version of the equation:

(3) $Y_{igst} = \sum_{k=-11}^{4} \rho_k z_{gs,t+k} + \tau_t + \theta_2\gamma_g + \theta_3\pi_s + \theta_4\tau_t + \theta_5(\gamma_g \cdot \pi_s) + \theta_6(\gamma_g \cdot \tau_t) + \theta_7(\pi_s \cdot \tau_t) + \boldsymbol{X}_{igst}\eta + \omega_{igst}.$

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 lead of the auto-IRA indicator) where k can range from -11 to +4. (The policy was first implemented in 2017 for a subset of employers in Oregon; thus, there can be at most four years including and following implementation.) 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 data, the Form 5500 data have both advantages and disadvantages. As noted above, the CPS data may record an increase in the probability of 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.

On the other hand, the Form 5500 data do not include a measure of firm size, which prohibits us from taking advantage of variation across firm size. Using the Form 5500 data, we estimate the difference-in-differences (DiD) specification

(4) $Y_{jst} = \delta_1(AutoIRA_{st}) + \delta_2 q_j + \delta_3 \tau_t + \boldsymbol{X}_{jst}\lambda + \nu_{jst}$,

where Y_{jst} is the dependent variable for firm *j*, in state *s*, at time *t*. Dependent variables include an indicator for providing one or more employer sponsored retirement plans (the extensive margin) and the logged number of plan participants (the intensive margin). 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). For each outcome, we estimate both a pooled cross-sectional regression and a fixed-effects panel regression. The fixed effects regression includes firm dummies (q_j), while the pooled cross-sectional regression includes state fixed effects. (We do not include both state and firm fixed effects in a given regression. As relatively few firms move across state lines, state fixed effects are largely subsumed by the firm fixed effects.) In the pooled cross-sectional regressions, we use robust standard errors and in the fixed-effects panel regressions, standard errors are clustered at the firm level. The coefficient δ_1 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.

4. Results

a. Individual-Level Data (CPS)

Table 4 shows results from estimating equation (1). In the first two columns, the dependent variable is an indicator for whether the individual's employer offers an ESRP; the

first column does not include firm size dummies, while the second column does. The coefficient on the auto-IRA indicator is the same in both columns – a statistically significant 0.014. This coefficient implies that individuals in adopting states are 1.4 percentage points more likely to work for an employer who offers a retirement plan during the post-adoption period. Relative to the dependent variable mean of 47 percent (Table 1), this represents a 3 percent increase. In the last two columns, the dependent variable is an indicator for being included in a retirement plan at work. The point estimates suggest that there is a 1.1 percentage point increase (a 14 percent increase relative to the dependent variable mean of 8 percent shown in Table 1) in the probability of participating in a retirement plan at work in states with an auto-IRA mandate. This coefficient is significant at the 10 percent level without firm size dummies and the 5 percent level with firm size dummies.

Table 5 shows results from estimating equation (2). The first column shows that individuals working for affected firms (based on size and state) have a 1.4 percentage point (3 percent relative to the dependent variable mean) greater probability of working for an employer who offers a retirement plan during the post-implementation period. The second column shows that the probability of participating in an ESRP increases by 2.6 percentage points (33 percent relative to the dependent variable mean of 8 percent) after policy implementation for individuals in affected firms.

Our event study results from equation (3) are shown in Figure 1(A) (for whether the respondent's employer offers an ESRP) and 1(B) (for whether the respondent is covered by the ESRP). The results appear to validate the common trends assumption for our triple differences estimates. There is a post-adoption increase in the probability of an employer offering a retirement plan, with no obvious pre-trends. While the post-adoption coefficients

in the event study are not individually significant, that is likely due to the small number of post-adoption observations. The estimation of equation (2) suggests that these coefficients are jointly significant.

b. Firm-Level Data (Form 5500)

Table 6 shows estimates of equation (4). The first column is the pooled cross-sectional regression specification, which includes state and year dummies but no firm dummies. The coefficient of 0.008 suggests that following the adoption of auto-IRA mandates, the probability of a firm in a treated state offering any ESRP increases by 0.8 percentage points relative to control states. This represents a 1.4 percent increase relative to the dependent variable mean of 56 percent. The fixed-effects panel regression specification in the second column includes firm and year dummies as written in equation (4). This coefficient suggests the probability of offering at least one ESRP increases by 0.9 percentage points (1.6 percent) in states with an auto-IRA mandate relative to the states without it after the policy implementation.

The results in Table 6 are based on an extensive margin measure of ESRP offering at the firm level. In Table 7, the dependent variable is the natural log of the number of retirement plan participants. (Firm-year observations with zero participants are dropped.) The coefficient estimates suggest that there is a roughly 6 percent increase in the number of participants in existing plans in the pooled cross-sectional regression specification with state and year fixed effects, and a 2 percent increase in the number of participants in the fixed-effects panel

regression specification with firm and year fixed effects.[9] The next two columns show the same two specifications. However, the dependent variable is the natural log of the number of participants plus a small, arbitrary constant (in this case, 1). These estimates suggest that for all firms, including those that do not have plans in one or more years over the study period, the number of participants increases by around 3-4 percent depending on whether we use the pooled cross-sectional or fixed-effects panel regression specification. Finally, treating the inverse hyperbolic sine as an approximation to the natural log, the last two columns show that auto-IRA legislation is associated with a 4-5 percent increase, depending on whether we use the pooled cross-sectional or fixed-effects panel regression specification, in the number of participants in ESRPs.

5. Discussion and Conclusion

In summary, we have examined the impact of state auto-IRA legislation – 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 legislation 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-level and firm-level datasets, and quasi-experimental methods, we find that auto-IRA legislation has a positive and significant effect on the probability of employers

[9] As discussed by Halvorsen and Palmquist (1980), the estimated impact of a one-unit increase in the auto-IRA indicator on the dependent variable is $\exp(\delta_1) - 1$, where δ_1 is defined in equation (4). This value is approximated by δ_1 itself, as δ_1 is relatively small.

offering an ESRP, the probability that an employee is included in an ESRP, and the number of participants in existing ESRPs.

It is important to note that determining the impact of auto-IRA legislation 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). However, these results are relevant for policy makers who wish to understand the impact of auto-IRA legislation on firm behavior.

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Table 1: Descriptive Statistics, CPS ASEC 2009-2020

	All	Control States	Treated States	CA	IL	OR
Age	39.41	39.45	39.25	39.21	39.42	39.12
Female	0.47	0.48	0.46	0.45	0.47	0.47
Nonhispanic White	0.62	0.65	0.47	0.39	0.64	0.77
Nonhispanic Black	0.12	0.13	0.06	0.05	0.12	0.02
Nonhispanic Asian	0.06	0.05	0.13	0.16	0.06	0.05
Nonhispanic Other	0.02	0.02	0.02	0.03	0.01	0.05
Hispanic	0.18	0.15	0.31	0.38	0.16	0.12
<Highschool	0.08	0.07	0.11	0.13	0.06	0.07
Highschool	0.26	0.27	0.22	0.21	0.24	0.23
Some College	0.27	0.27	0.26	0.26	0.26	0.30
College Graduate	0.25	0.25	0.26	0.25	0.27	0.25
Graduate School	0.14	0.14	0.15	0.14	0.17	0.15
Firm size <10	0.19	0.18	0.21	0.22	0.16	0.22
Firm size 10 to 99	0.22	0.21	0.22	0.22	0.21	0.22
Firm size 100+	0.60	0.60	0.57	0.56	0.63	0.56
No pension at work	0.53	0.53	0.56	0.58	0.51	0.51
Pension at work	0.47	0.47	0.44	0.42	0.49	0.49
Included in Pension at Work	0.08	0.08	0.07	0.07	0.08	0.09
Non Included in Pension at Work	0.39	0.40	0.37	0.35	0.42	0.40

Note: Authors' calculations based on the CPS ASEC 2009-2020. 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.

Table 2: Descriptive Statistics, Form 5500 Data Aggregated at Firm Level 2009-2021

	All	Control States	Treated States
Firm offers at least one plan	0.56	0.56	0.54
Total participants at firm	117	124	91
Ln(total participants)	2.9	2.93	2.74
Ln(total participants + 1)	1.66	1.69	1.52
IHS(total participants)	1.98	2.02	1.82

Note: Authors' calculations based on the Form 5500 data 2009-2021. Sample includes all establishments filing Form 5500 or its short form.

Table 3: Rollout of Auto-IRA Legislation by 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	January 1, 2023
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: https://humaninterest.com/learn/articles/do-state-mandated-retirement-programs-really-work/, https://www.ilsecurechoice.com/home/faq.html, https://www.jdsupra.com/legalnews/illinois-expands-its-secure-choice-3771757/, https://www.zenefits.com/workest/what-is-oregonsaves-everything-you-need-to-know/

Table 4: Difference-in-Differences Effect of Auto-IRA Legislation on Worker-Level ESRP Availability and Participation, CPS ASEC 2009-2020

VARIABLES	(1) Employer Offers Pension	(2) Employer Offers Pension	(3) Included in Pension	(4) Included in Pension
$AutoIRA_{ist}$	0.014**	0.014**	0.011*	0.011**
	(0.006)	(0.005)	(0.006)	(0.005)
Observations	578,282	578,282	578,282	578,282
R-squared	0.055	0.193	0.062	0.164
Year FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Firm Size FE	YES	YES	YES	YES

Note: Authors' calculations based on CPS ASEC 2009-2020. 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.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Triple Differences Effect of Auto-IRA Legislation on Worker-Level ESRP Availability and Participation, CPS ASEC 2009-2020

VARIABLES	(1) Employer Offers Pension	(2) Included in Pension
$AutoIRA_{igst}$	0.014**	0.026**
	(0.007)	(0.011)
Observations	589,270	589,270
R-squared	0.20	0.17
Year FE	YES	YES
State FE	YES	YES
Firm Size FE	YES	YES
Year FE*State FE	YES	YES
Sate FE*Firm Size FE	YES	YES
Firm Size FE*Year FE	YES	YES
R-squared	0.20	0.17

Note: Authors' calculations based on data from the CPS ASEC 2009-2020. 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.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Difference-in-Differences Effect of Auto-IRA Legislation on Firm-Level ESRP Offerings, Form 5500 2009-2021

VARIABLES	(1) Employer Offers Pension	(2) Employer Offers Pension
$AutoIRA_{jst}$	0.008***	0.009***
	(0.001)	(0.001)
Observations	15,791,500	15,791,500
R-squared	0.011	0.008
Year FE	YES	YES
State FE	YES	NO
Firm FE	NO	YES

Note: Authors' calculations based on the Form 5500 data 2009-2021. Sample includes all establishments filing Form 5500 or its short form. All regressions also include year dummies.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Difference-in-Differences Effect of Auto-IRA Legislation on Firm-Level ESRP Participation, Form 5500 2009-2021

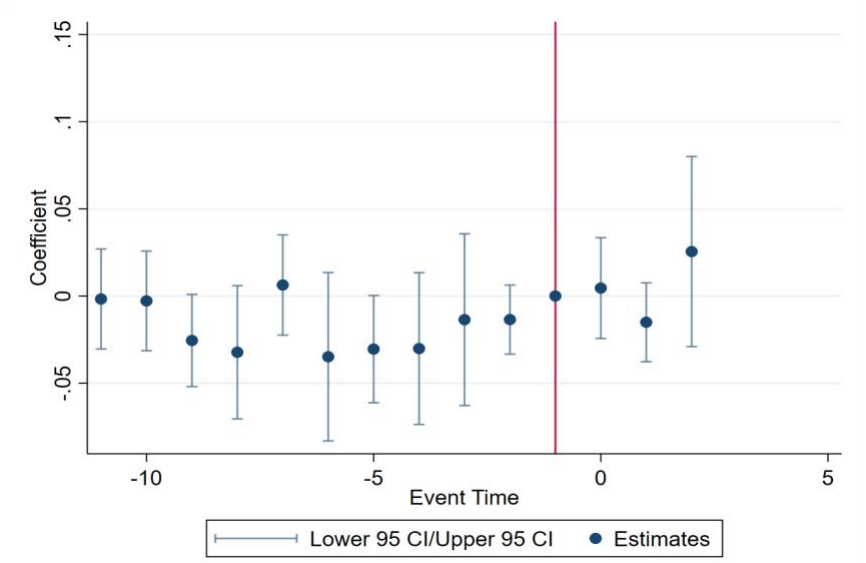
VARIABLES	(1) Ln(N of Participants)	(2) Ln(N of Participants)	(3) Ln(N of Participants+1)	(4) Ln(N of Participants+1)	(5) IHS(N of Participants)	(6) IHS(N of Participants)
$AutoIRA_{jst}$	0.061***	0.020***	0.029***	0.038***	0.034***	0.045***
	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	8,649,829	8,649,829	15,752,573	15,752,573	15,752,573	15,752,573
R-squared	0.011	0.044	0.013	0.008	0.013	0.008
Year FE	YES	YES	YES	YES	YES	YES
State FE	YES	NO	YES	NO	YES	NO
Firm FE	NO	YES	NO	YES	NO	YES

Note: Authors' calculations based on the Form 5500 data 2009-2021. Sample includes all establishments filing Form 5500 or its short form. All regressions include year dummies.

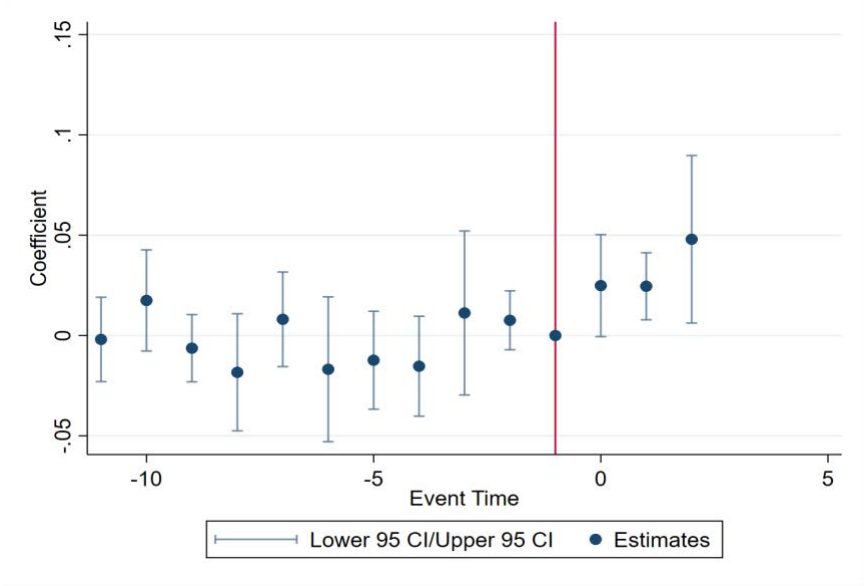
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Event Study Effect of Auto-IRA Legislation on Worker-Level Availability and Participation in ESRP

(A) Employer Offers Retirement Plan



(B) Included in Retirement Plan



Note: Authors' calculations based on CPS ASEC 2009-2020. 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.