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Does a Requirement to Offer Retirement Plans Help Low-Income Workers Save for Retirement? An Early Evidence from the OregonSaves Program

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Abstract

This study examines the first implemented state-run retirement program (Auto IRA) in Oregon (OregonSaves) in 2017 and provides early evidence of the significant impact on saving for retirement among uncovered private workers. Early results from the Difference-in-difference using SIPP data show that the program was associated with a 27 percent increase in owning an auto- IRA among Oregon workers compared to other states after the program implemented in 2017 while estimates using CPS-ASEC data indicate 5 percent increase in participating into pension at work, on average. Important findings demonstrate that mandated retirement saving program is more likely to benefit lower-income, less-educated, and older workers who previously were not covered in retirement plans at work-place in private sectors.

Keywords: state-run retirement plan, auto enrollment, IRA participation, peer effects

1.Introduction

In the United States, employment-based retirement plans such as 401(k) plans play an essential role in the retirement system. However, over the past 40 years, share of private workers offered any retirement plans at work place is persistently low overtime (roughly 50%) (Munnell & Chen, 2020). Median account balance experienced downward trend from $31,396 in 2013 to $25,775 in 2019 for all ages, with roughly a third of plan participant having a balance of less than $10,000 (Vanguard, 2021). The retirement landscape among private workers in small-size firms are even worse because the smaller firms are less likely to offer retirement plans to their employees. A recent study by the Center for Retirement Research at Boston College[1] shows that firms with less than 100 workers account for 97.5% of total businesses and 35% of private workers as in 2019. Yet less than 50% of firms with less than 50 employees offer retirement plans at work while larger firms (100-499 and more than 500 employees) are more likely to provide pension (87% and 95% respectively).

While studies on understanding why smaller firms do not offer retirement plans to their workers, an equally important question is that how policy acts to boost saving for retirement among uncovered workers. In the absence of national policy that aims at helping uncovered workers access to retirement plans at workplace, many states took the initiative. Since 2012, 40 states have either enacted legislation that will implement a state-facilitated retirement program, studied program options, or considered such legislation. The goal of these programs is to provide private-sector workers with greater access to retirement savings plans, especially among low-income employees. California, Illinois, Massachusetts, Oregon, and Washington recently launched their programs (see Appendix A for details)[2]. While California, Illinois, and Oregon chose to develop a mandatory auto-enrollment IRA style[3], Washington has decided on a

[1] Center for Retirement Research at Boston College (2022). “Why Do Some Small Businesses Offer Retirement Plans?”. Special Report, May 2022.

[2] City of Seattle, Washington has launched similar auto IRA program similar to California, Illinois, and Oregon, and expected to launch its program in January 2019 (Klančnik et.al (2018)).

voluntary marketplace option. Massachusetts had a different approach; it offers voluntary auto enrollment in Multiple Employer Plans (MEPs) for workers in non-profit organizations with 25 employees and fewer. A key rationale for state-based retirement plans is the fact that a significant share of workers lacking access to employer-sponsored retirement plans have no dedicated retirement saving vehicles (Chen and Munnell 2017, Chalmers et al.2020).

Launched in 2017, the Oregon retirement savings program (OregonSaves), like ones implemented in California and Illinois, mandated businesses to facilitate a state-run retirement program by setting up payroll and automatically adding employees to a Roth IRA. The program is required for all businesses that do not offer a retirement plan for their workers, with a phased enrollment based on firm sizes (see Appendix A-Table 2). Workers can opt-out at any time.

The OregonSaves model is of interest since it is the first state-mandated retirement program that offers an auto-enrollment IRA with a default rate. The auto-enrollment with a default rate feature is expected to increase participation, especially among younger, lower-paid, and less-educated workers (e.g., Madrian and Shea, 2001; Mitchell and Utkus, 2012; Chalmers and Reuter, 2019). Further, experiences from other countries like Denmark and UK, and a recent experiment in the US Army[4] show that automictic contribution is likely to be more effective at boosting savings than subsidies or tax incentives (Chetty *et al*., 2014; Cribbs & Emmerson, 2016; Beshears *et al*., 2019, Henrik Yde *et al*., 2020). Evidence from the Oregon model, therefore, has important policy implications for policymakers at both federal and state levels in determining an optimal retirement plan design that promotes savings among private uncovered workers.

As discussed in Cribb & Emmerson (2019), three essential reasons that one would expect the positive effects of auto-enrolment on workplace retirement savings. First, the default feature is likely to solve the "procrastination" problem described as the situation in which individuals wait until the last day to make election decision because either they are time-inconsistent, present-biased, unorganized, financially unsophisticated, or liquidity constrained (Brown & Previtero, 2020). Hence, by defaulting employees into a retirement plan, it is expected that

[4] A recent study by Beshears *et al*. (2019) examines a natural experiment that introduced an automatic enrolment at a 3% of income default contribution rate for newly hired *civilian* employees of the US Army. They confirm that auto enrolment increases contributions to the retirement savings plan.

participation will be higher. Second, the automated retirement plan (with default contributions and asset allocations) might help the decision to take part in less financially complex, as a result, participation (especially among individuals with less financial literacy) would be higher. Third, as pointed out in Beshears *et al.* (2009), individuals may consider the default features under automatic enrolment scheme as implicit advice on the best option, which in turn would boost the participation. And finally, many uncovered workers wished to enroll but were not offered any plans. Therefore, when the retirement plan becomes available, they would be willing to enroll.

Early studies by Quinby *et al*. (2020) and Chalmers *et al*. (2020) using Oregon administrative data show significant increases in participation rates (though the exact participation rates are sensitive to how they define participation rate due to the data limitation), and positive balances in these accounts among participated workers. However, these studies have some limitations. First, they are unable to define a sample of eligible Oregonian workers, therefore it is yet unclear about the size of the effects within Oregon and between Oregon and other states. Second, these studies are descriptive (due to the nature of the administrative program data) work, they are too early to claim a causal effect of the program. Third, further study is needed to understand whether OregonSaves actually increased additional savings or created a shift across assets. And lastly, an important question is whether the state-run retirement program created positive effects on promoting savings among low-income and disadvantageous groups has not been answered yet. This question is vital since previous studies show that both coverage rates and plan participation have remained persistently low among low-income, less-educated private workers under the current system (Copeland, 2014; GAO,2015; Gale and John,2018).

This study contributes to the existing literature in this area in several ways. First, this first study provides early evidence on the impact of OregonSaves on savings, employment, and the interaction with other social programs using public survey data with rich information on household's assets. These data include information for workers in other states, which serves as a comparison group in estimating the causal effect of the Program. Second, the study uses a difference – in – difference approach to identify the causal effects within Oregon and in comparison, with other states so as to measure the comparable size of the effects on outcomes. Third, it further explores the potential spill-over (informational) effects across firms within

Oregon due to the fact that peer information could affect the financial decision like retirement saving (Duflo and Saez,2002,2003; Beshears *et al*., 2015).

Results from the DID models show that the OregonSaves led to higher probability of IRA participation (3 percentage points or 27 percent) than that of pre-reform period and in comparison with the similar workers in other states, which is relatively sizeable. The impact seems to be driven by employees who worked for larger firms (firms with over 100 employees and more), with an estimated rise of 2.6 percentage points (or 24 percent) following the implementation of the Program. These findings are plausible given the fact that firms with at least 100 employees were the first to be required to set up the default plan during the study period. For other smaller firm size groups, there are not statistically significant evidence of the positive effects on IRA enrollment given they were required to register in the program in latter days. More interesting and important, I observe large peer effects among employees at very small firms (less than 10 employees)[5]. The findings show the OregonSaves was associated significant jump in enrolment, with 15.4 percentage points (or 109 percent) higher after 2017. The results additionally confirm that the overall household retirement assets statistically significantly increased (by 6 percent) following the roll-out of the Program. Despite the unclear mechanism of the peer effects at this moment, these observed effects are important to demonstrate the strong impact of the OregonSaves, at least during the study period.

The subgroup analysis by demographic characteristics once again confirms the findings from previous studies that retirement savings are correlated with ages, education, and income. In addition, the study provides interesting findings that such state-mandated retirement plans as the OregonSaves also created positive impact among less advantageous groups. In particular, I find that the impact of the OregonSaves was centered among female (increase in IRA participation by 7 percentage points or 40 percent) and nonwhite (a rise in participation by 7 percentage points or 52 percent). Further, I also find that less educated (those with high school degrees) and lower-earning workers (those with family incomes were less than 25k) benefited from the state-run plan. Among these groups of workers, their IRA participation were 8 percentage points (or 73

[5] This group of firms were required to sign up into the state-run plan by the end of year 2019. And so, one would expect very small to null effects among workers at these firms.

percent) and 4 percentage points (or 44 percent) higher compared to the similar workers in other states following the OregonSaves' roll-out.

The remainder of our paper is organized as follows. In Section 2 I describe the program background and provide a brief summary of the related literature. In Section 3 I describe the data and empirical models. In the next section – Section 4 - I present the main results. And finally, I conclude and provide a discussion in Section 5.

2. Background and Literature Review

2.1. Program Background

The state of Oregon passed its legislature in 2015 to create state-run auto IRA retirement savings program (OregonSaves). The program requires employers without a retirement plan to automatically enroll their employees[6] in state-run program, with a phased roll-out approach in which firms are required to register their workers based on firm sizes (see Appendix A – Table 2 for details). Under the OregonSaves, a 5 percent default rate is set with auto-escalation of one percentage points (up to 10 percent of the gross paycheck) in the following years, and there is no employer matching. The contributions are made into a Roth IRA under workers' names. The retirement fund is administered by the Oregon Treasury Savings Network. By default, the first $1,000 contributions will be invested in government money market fund, then all the subsequent contributions will be invested in an OregonSaves Target Retirement Fund[7]. However, employees have alternative investment options such as S&P 500 Index or money market. In terms of program fee, participants (plan holders) are charged an annual rate of 1% of their contributions and no charges applied to making withdrawals, changing contribution rates, or moving monies between investment funds. The OregonSaves started rolling out in July 2017 when firms with 100 employees and over were the first to meet the obligation.

2.2. Literature Review

Two branches of literature relate to this study: auto enrolment (or default) and mandated retirement savings policies.

[6] Although OregonSaves is mandatory to employers, workers have options to opt out if they do not wish to participate.
[7] More details can be obtained at https://www.oregonsaves.com/savers/investments

Since Madrian & Shea (2001), substantial research on the effects of defaults on saving behaviors show strong relationship between defaults and retirement savings in the context of the United States. Particularly, Madrian & Shea analyzed change in 401k plan enrolment at one big publicly traded firm, which automatically enrolled (with 3 percent default rate) new hires starting in 1998. The author compared the outcomes (401k participation, contribution rates, and fund allocations) between employees hired before and after March 31 1998. The overall findings show that participation rate among workers hired under auto enrollment was approximately 86 percent[8], which was mostly double that of old-cohort employees who were hired prior to the default feature. However, of course, the new cohorts under auto enrolment experienced lower contribution rates, and most of them invested in money market fund.

In latter studies, similar findings (relatively high participation rates among eligible workers) support the significant effects of default enrollment on 401k participation (Choi et al, 2004; Beshears et al.,2009; Goda &Manchester, 2013). For example, Choi et al., (2004) used data from 3 large firms and found that 401(k) participation rates were roughly 85 percent in all firms, regardless of the tenure of the employees, compared to the rates of 26 – 69 percent (depending on the tenure) prior to automatic enrollment. Two mechanisms that likely explains the "default effect" are present bias (refer to the preference to put more weight on present's benefits and costs relative to future's) and financial literacy (Goda et al., 2019)[9]. However, auto-enrollment with default rate comes at costs. Prior studies Choi et al., (2004) also find that participation rates under the auto enrollment regime falls over time, and that with lower contribution rates due to low default savings rate and a conservative default investment choice might undermine long-term wealth accumulation (Choi et al., 2004; VanDerhei, 2010; Butrica & Karamcheva, 2012; Vanguard Group, 2016; Goda et al., 2019)

The second strand of literature related to this paper is studies on how mandated savings programs and polices affect retirement savings. First study by Chetty et al., 2012 (though their

[8] The results also mask considerable variation by different demographic characteristics such as age, gender, races, and earnings. For example, older or highly compensated workers participated in 401k plan with higher rates.

[9] Employees who are financially illiterate are more likely to have no clear understanding of what a good saving rate is, and may therefore be more strongly influenced by the default.

work examined the case of the Denmark) analyzed the government mandated retirement savings plan that required Danish citizens above a specific income threshold to contribute 1% of labor income (auto contribution), starting in 1998. They show that since majority of individuals (85 percent) were passive savers, so hence, they would have saved more when induced to do so by an automatic contribution. Similarly, UK introduced a mandated auto enrollment policy took effect in 2012 that required employers (without offering any retirement plans) to auto enroll their workers in retirement savings plan in which workers contribute 4 percent of after-tax earnings, the employer provides a 3-percent match, and government adds 1 percent (Sass, 2014). Empirical studies on the UK experience again support the powerful effects of auto enrollment (with default rate) on participation (Turner et al., 2018; Cribb & Emmerson, 2016,2019) (roughly 70% increase in participation rates among small firms as shown in Cribb & Emmerson, 2019).

Regarding studies on the OregonSaves Program, two working papers (Quinby et al., 2019 and Chalmers et al., 2021) both use administrative data from Oregon and document relatively high participation rates after the Program rolled out. For example, Quinby et al., 2019 show that the participation rate (among eligible workers) ranged between 48 and 67 percent. However, balances in these registered accounts are low ($750 on average), and about one fifth of the workers with balance made at lease on pre-retirement withdrawal during the subsequent year. These findings suggest that while participation in the mandated program is high, contribution rates (and therefore the accumulated balances) are relatively modest. However, this is still an important policy to promote retirement savings among those who previously were not covered under employer-sponsored retirement plans. And it is also an important empirical question on the impact of the OregonSaves on participation and contributions, compared to other states as Oregon was the first state launched state-mandated retirement savings plan.

3. Data and Empirical Methods

3.1. Data

Two main data sources are used in this study: the Surveys of Income and Program Participation (SIPP) covers years from 2013 to 2020 (SIPP panel 2014 and panel 2018), and the Annual Social and Economic Supplement to the Current Population Survey (CPS-ASEC) running from 2013 to 2019.

The Surveys of Income and Program Participation (SIPP) data is a nationally representative, longitudinal sample of civilian, non-institutionalized U.S households (approximately 14,000 to 52,000 households in each panel) for a short duration of 2.5 to 4 years. Besides the monthly core modules that cover topics on labor force, welfare programs participation, demographics, and income, SIPP contains detailed information on financial assets (personal savings, retirement accounts), and nonfinancial, tangible assets (personal residence and vehicles), as well as information on households' liabilities such as home mortgages and credit card debt. Since SIPP 2014, details on assets and liabilities are surveyed annually and recorded for the last month of the reference year while information on employment, labor incomes, welfare programs enrollment, health insurance, and other demographics is fielded on a monthly basis.

The Annual Social and Economic Supplement to the Current Population Survey (CPS-ASEC) is an annual cross-sectional survey that collects supplemental information to the Monthly CPS on work experience, income, cash and non-cash benefits including those from public assistance programs, health insurance coverage, migration, and pension plans and benefits. Since transfer income tends to be underreported in CPS data (Wheaton,2008; Meyer,Mok, and Sullivna, 2009; Moffitt and Scholz, 2009), this study uses the SIPP as the main data and uses CPS-ASEC as a robustness check for the main results.

Outcome Variables.

The first set of outcome variables measure IRA ownership including IRA enrollment and IRA Balance. First, the IRA enrollment is a binary variable indicates whether a working individual ever owned any IRA accounts[10]. This variable is derived from survey question whether household members owned any IRA or KEOGH accounts during the reference period (as of the last day of the reference period). Second, regarding IRA balance, I construct three variables: IRA balance, conditional IRA balance (the balance among those who ever owned any IRA or KEOGH accounts), and log transformation of IRA balance. IRA balance and conditional IRA balance are measured in real 2017 dollars when the program was phrased out.

Second set of outcome variables constructed to examine the spill-over effects of the OregonSaves program. These variables include a measure of 401k plan ownership (k401 plan enrollment and balance), and the household-level value of all retirement accounts which allows

[10] I utilize short panel design to observe if an individual ever owned any IRAs over a duration of 3 -4 years.

assessing the overall effect of the new policy. Similar to the IRA balance measure, all the monetary variables are inflated in real 2017 dollars.

Sample of Analysis using SIPP data.

I use a sample of currently working individuals ages 18-60, working in the private sector, and containing information on size of firms that they worked for. This information is important given the phase-in by firm size feature of the program. In the analysis sample, two states (Illinois and Washington state) are excluded since these states began implementing similar policies in 2018 and 2019 respectively. For the DID analysis of the impact of the OregonSaves on IRA ownership and subsequent analysis, the sample further restricts to private workers who did not own any 401k plans. This exclusion is to reflect the fact that employers who already offered 401k or similar plans (401k plans, for short) are not subjected to the state mandate, and therefore, those covered by 401k plans are not able to enroll in state-facilitated IRA plans via their employers. Hence, they are not affected by the OregonSaves[11]. For the spillover effect analysis, I use the full sample which contains all private workers ages 18-60 as described above.

Table ….. summarize the sample characteristics between Oregon and the rest of the states (except Illinois and Washington) before and after the Program rolled out.

3.2. Empirical Methods

I provide two quasi-experimental research designs, beginning with a Difference – in – Difference (DID) analysis, then a "event time" study to examine the casual effects of the OregonSaves program. I begin with the DID design by estimating the following model:

$$Y_{ist} = \alpha + \beta OregonSaves_s * Nov2017_t + \gamma X_{ist} + \delta_s + \tau_{st} + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is a set of measures of IRA ownership (e.g., IRA participation and IRA balance); $OregonSaves$ is a binary variable equal to 1 if an individual resided in Oregon[12] and 0 otherwise; $Nov2017$ is a binary variable equals to 1 if the time variable indicates after November 2017 (when the program actually rolled out) and 0 otherwise; X_{ist} is a set of individual socioeconomic and demographic characteristics including: age, gender, education,

[11] As a robustness check, I also explore the effects of OregonSaves using the full sample. Findings show no positive effects on IRA ownership. Results are presented upon request

[12] I assume that an individual resided in Oregon also means that she/he worked in Oregon. To further address the issue of cross-border employment that may contaminate the treatment effect due to the fact that Oregon residents might work in other neighboring states (CA, WA, ID, and NV) or vice versa, I will investigate the employment effect using county-pair estimates borrowing from Peng et.al (2018), Dube et.al (2010). Early analysis show that the employment effect is insignificantly small among shared border states with Oregon, meaning that the cross-border workers are not likely to affect the treatment effect estimate specified as in Equation (1).

race, marital status, metropolitan status, family size, family income, and firm sizes. δ_s and τ_{st} are state fixed effects and state-specific linear time trend that capture unobserved time trends and state-level conditions that may be correlated with both outcomes and the OregonSaves program; and ε_{ist} are error terms that will be clustered at state level (Cameron and Miller,2015). The Equation (1) is equivalent to the simplest form of the conventional DID regression, which incorporates a "treated unit" (OregonSaves) indicator, a "treated period" indicator (for 2017 to 2019), and the interaction of the two. With the inclusion of state and year fixed effects, the dummies for the "treated unit" and "treated period" are absorted. Hence, the $OregonSaves_s * Nov2017_t$ dummy implicitly interacts the Oregon dummy with the sum of the 2017-to-2019-year dummies, providing the estimate of the treatment effect. In the model (1), the comparison states are all other states (except for Illinois and Washington states that implemented their programs during the study period).

The validity of the causal inference of the program effect under the DID approach relies on the assumption that the expected values of the outcome variables in the comparison group during the treated period presents the counterfactual for the treated units during the same period in the absence of the treatment (known as the parallel trend assumption). While this assumption cannot be strictly tested (Arora & Wolf, 2018; Rossin-Slater et al.,2013), I conduct the analysis using alternative comparison states. In the robustness check section, I provide the estimates using these alternative comparison selection[13]. The results show consistent and statistically significant effects of the Oregon program on IRA participation and IRA balance (log). The estimates are robustly close to the baseline model as specified in the Equation (1) (see Table 7).

The OregonSaves Program used a phased roll-out enrolment approach in which firms with 100 employees or more were required to facilitate to automatically enroll their employees into the auto IRAs by November 2017, then smaller-size businesses were obligated to enroll their employees to state-run IRA plans by 2018 and 2019, I provide the DID estimates for five groups of working for firms classified by five firm-size groups, including less than 10, 10 – 19, 20 – 49, 50 – 99, and over 100. Since firms with 100 employees and more were the first to sign up for the

[13]Many studies use Synthetic Control Method to select comparison states when there is one single treated state, and large enough pre- and post-treatment period (Abadie, Diamond &Hainmueller,2010, 2015; Sterns, 2015). Having sizable pre-treatment data points is vital to Synthetic Control Method to assure the validity of inference (Aurora & Wolf, 2015; Ferman &Pinto, 2015). However, in this study, there are only 4 years prior to the Program, which makes the Synthetic Control Method be irrelevant.

program, it is expected to observe the strongest impact of the OregonSaves among workers in this group. I also explore the heterogenous effects across subgroups by ages, marital status, races, educational attainment, and family income to understand how the state-run retirement program affected less advantageous groups.

To testify the likely validity of the DID's pre-trend assumption, I estimate event study regression models that allows for a complete set of interactions between the indicator of treatment status and years in the following form:

$$Y_{ist} = \alpha + \delta_s + \tau_{st} + \sum_{k=2013}^{2019} \beta_k Oregon_s Year_t + \gamma X_{ist} + \varepsilon_{ist} \quad (2)$$

where all the covariates are similar to Equation (1). Year 2016 is the reference year, and hence, is dropped out of the model. The pre-trend assumption under the DID model implies that β_k would be statistically indifferent from zero in years prior to 2017.

4. Results

4.1. IRA Enrolment

Table 2 shows the results for the OregonSaves difference-in-difference models using two samples: private workers (full sample), and private workers who worked for firms with more than 100 employees (large firm sample). Column 1 and 2 of the Table 2 display the coefficients estimates using the full sample while Column 3 exhibits the estimate using the large firm sample. As expected, result from the baseline model where demographic characteristics are added (as presented in the Column 2) shows that the OregonSaves led to increase in IRA enrolment. It is estimated that after the Program rolled out IRA enrollment among private workers who previously did not have any 401k plan rose by 2.8 percentage points (ppts) (or 27 percent), which is relatively sizeable. The effect became larger among workers in large firms (100 employees and over), with the participation being likely to raise by 2.6 ppts (or 24 percent) compared to the similar workers in other states. The coefficient estimates of the IRA participation are relatively large, which might raise concern about the validity of the estimation, especially when there is only treated unit (Conley & Tabler, 2011; Mackinnon & Web, 2018; Roodman et al., 2019; and Cengiz et al., 2019). I then follow the procedure proposed in Roodman et al., 2019 accessing the treatment effects by using the robust cluster residual bootstrap. The results are robustly similar

and the "boottest" test shows that the coefficient estimates are statistically significant from zero, suggesting the true effect of the OregonSaves[14].

Explore if the gradual roll-out feature under the mandate might affect the IRA participation differently, I estimate the Equation (1) with sample is split by firm sizes: less than 10, 10 – 19, 20 – 49, 50 – 99, and over 100 employees (as described in the Section 3). Figure 7 plots the treatment effects of the OrgeonSaves by firm sizes. Surprisingly (and importantly) it reveals that the effects were most centered among very small firms (less than 10 employees), with the observed increase in IRA enrollment by 15.4 ppts (or 109 percent) following the introduction of the state-run retirement plan. This large effects of the auto-enrolment is not surprised as similar findings are found in Madrian & Shea (2001), Choi et al. (2004), and Cribb & Emmerson (2019)15 (though this group was not targeted to be enrolled during the study period). As previously discussed, although small-size firms were not required to facilitate enrollment by end of 2019, they can do so in any time since the program started. Hence, peer effect is one plausible mechanism for positive effect among workers in small-size firms (Duflo & Saez 2002, Cribb & Emmerson (2019). In contrast, among those worked in larger firms (ranging from 10 to 99 employees), IRA enrolment did not go up after the OregonSaves rolled out, compared to other states. Indeed, IRA participation among workers in these groups fell 5 – 9 ppts, compared with the pre-implementation years (though the estimate for the 50 -99 group is not statistically significant). Three possible reasons that might explain the decline in enrollment among them. First, although employers are required to register their workers for the auto enrolment, however, the enrolment is not mandatory to workers. This means workers would not have saved anyway as they did in the prior years to the OregonSaves introduction. Second, by law, firms must set up the auto enrolment for their workers by the assigned date, there is not enforcement applied to noncompliance yet[16]. Employers might delay the registration because of the associated costs to employers to set up the auto enrolment[17]. And finally, there is likelihood

[14] Results are presented upon request.
[15] Within Oregon, consistence with the DID estimates, auto IRA enrolment among workers in private firms with less than 10 and over 100 raised from 13% to 24% and from 16% to 24% respectively. Results are presented upon request
[16] Program document states an annual penalty of $100 per eligible employees, up to $5,000 (Oregon Retirement Savings Board, 2020). However, there is not compliance penalty applied yet.
[17] Pew's survey in 2021 reveals that one-third of surveyed firms reported to have incur costs associated with OregonSaves' registration. Among them, the out-of-pocket costs were more likely to be larger among smaller-size firms (10 – 49 employees) that were obligated to register during the study period.

that instead of facilitating OregonSaves, firms might offer their employees with traditional 401k plan to either get benefit from tax-motivated compensation or attain / attract talented workers (while they also must provide minimum incentives to lower-paid workers under federal nondiscrimination test rule (Mitchell et.al., 2005). I investigate this potential spill-over effect and present the estimated results in a later section.

I turn the analysis to examine heterogenous effects by different subgroups. Figure 8 – 11 demonstrate the estimated results by ages, marital status, races, education, and family income groups. The overall findings are consistent with previous studies that retirement savings increase with ages, higher among whites, and higher income workers. For example, the effects of the OregonSaves were largest among older workers (51 years old and older) with enrolment increasing by 10 ppts (or 78 percent), and among workers whose family income were over $100,000 (an 8 ppts up (or 42 percent) following the roll-out of the mandate). More importantly, the OregonSaves led to higher probability of participating in the state-run retirement plan among young, less educated, lower-income employees who were likely to be not covered in any retirement plan prior to the OregonSaves. It is estimated that high school graduates and some college workers were substantially affected than the other groups, with 7.7 ppts (or 148 percent) and 1.3 ppts (or 21 percent). The observed effects are vital given the lower IRA ownership among these workers, compared with higher educated employees. In addition, the results by subgroups also show that lower income workers (those with annually family incomes less than $45k benefited from the auto IRA enrollment (though the effects were smaller than the highest income group). Finally, the findings suggest that the effects of the program were centered among single and female employees.

4.2. IRA Balance

I estimate the effect of the OregonSaves on IRA balance, measuring by three outcomes: IRA balance, IRA balance conditioning on IRA ownership, and log form of IRA balance. Table 4 presents the coefficient estimates from the DID models as specified in the Equation (1). Consistently with the IRA enrolment estimates, the findings appear that IRA balance (both unconditional and conditional on IRA ownership) rose following the OregonSaves' roll-out, by $7,489 (or 165 percent) and $36,839 (or 85 percent) respectively. For the preferred outcome – log of IRA balance, I observe a 3.2 ppts (or 35 percent) increase in the IRA balance following

the OregonSaves' launch[18]. The estimates seem to be noisy (in terms of relatively large effects) since it is unable to detect whether the IRA accounts are set up via the OregonSaves program or via the traditional enrolment. However, the overall positive effects are important to document the spillover effect of the program.

4.3. The Spill-over Effects of the OregonSaves

Now I turn the analysis to explore if the OregonSaves would have led to increase in other retirement plans including the 401k plans as mentioned in Section 4.1. In doing so, I estimate both 401k ownership (probability of participation and balance) and total household retirement assets, using the pool sample. Table 5 displays the results from the DID models (as in the Equation (1) estimating the spillover effects of the Oregon on other retirement accounts: 401k ownership (401k participation and balance (log)) and household-level retirement assets (log). For 401k participation and balance, there is no evidence of the positive effects. However, I observe that household-level retirement assets increased by 4.3 ppts (or 6 percent) after 2017. When the sample is split by firm sizes (see Table 6), similarly to the estimates for IRA enrolment, I observe the positive impact among employees who worked for private firms of less than 10, 26-49, and over 100 employees. Among workers in 26 – 49 firm-size group, the 401k participation was more likely to be 2.5 ppts (or 8 percent) higher in the post-OregonSaves period. These findings, altogether, show that firm sizes matter in the context of retirement savings as workers in bigger firms tend to save for retirement, and that as long as savings at workplace is optional a portion of workers might not save anyway as they did in pre-reform years. Nevertheless, the positive spill-over effects observed in other retirement plans is important given that the OregonSaves might create the overall boost for retirement savings although the effects are heterogenous, and the observed effects by firm-size groups for both IRA and 401k plan participation does not clearly explain whether 401k plans is an substitute for auto IRA.

4.4. Robustness Check

Event-study Analysis

[18] The estimates for IRA balance are higher than the reported average funded balance in the Program report that was approximately $ 136 monthly as of September 2020. This difference might be because it is not likely to distinguish between the traditional IRA accounts and those set up via the OregonSaves Program.

I conduct event-study models as described in the Equation (2) to examine the dynamics treatment effects and validate the DID parallel assumption, and summarize the coefficient estimates in Figure 12 for two samples: full samples and sample contains individuals worked for firms with 100 and over employees, which is the first group of firms required to register for the program. As shown in the Figure 12, the effects of the OregonSaves appear to be consistently positive over the post-treatment periods, especially among "100+ Firms" sample. The auto-IRA participation among workers in this sample is estimated to increase over time from 5 to 7 percentage points after 3 years since the introduction of the Program, compared to the similar workers in other states. The statistically significant coefficient estimates in the pre-Program (before 2017) suggest that there is possibly systematically different between Oregon and other states regarding IRA participation, which essentially challenge the validity of the DID estimates. After carefully taking care of observed characteristics and using various alternative of comparison groups, I am not aware of any other state policies that might affect the IRA participation during the study period.

Evidence from the CPS data.

As mentioned above, it is indistinguishable types of IRAs (whether they are traditional, Roth, or state-initiated accounts) in SIPP data, hence, it is hardly to detangle the treatment effect of the OregonSaves from the general trend in IRAs. Additionally, it is not clear if worker's perception of the state-run IRA plan facilitated via employer's payroll as work-place pension. To this end, I supplement the main analysis with estimates using CPS data to evaluate the overall effects of the OregonSaves on the work-place pension plans using the similar sample (i.e. private employed workers aged 18 – 60 years old). I report the estimated results in Table 7 for the pooled sample and for each firm-size group: less than 10, 10 – 99, 100 – 499, and over 500. Note that since question on pension plans offered at work place in the CPS data is asked for the preceding year, I drop year 2017 to make the estimates less complicated. As shown in the Table 7, overall, the OregonSaves created the positive effects on retirement plan coverage at work-place. It is estimated that the program led to 2 percentage points (or 5 percent) increase in having a pension plan at work, which is relatively close to the estimates of 401k plan ownership using the SIPP data. When the sample is split by firm-size groups, the larger effects of the OregonSaves were centered among employees worked at smaller firms. More particularly, the results show that Oregonian workers at firms with number of employees less than 10 and between 10 and 99 were

more likely to own retirement plans by 4.5 percentage points (or 39 percent) and 5.4 percentage points (or 21 percent) respectively compared to the similar workers in other states after the Program launched. These findings may partly explain the decline in IRA ownership among employees (worked for firms with sizes of ranging from 10 to 99) found in the main results.

Alternative selection of comparison states.

I run several alternative specifications in which different groups of comparison states are selected including: (1) States have enacted state-run retirement programs, but have not implemented yet, or implemented but have not yet observed in the data (CA,, CT, MD, NJ, and VT)[19]; (3) states in group 1 and states considering similar programs (AZ, CO, IN, KY, LA, ME, OH, ND, NE, NH, UT, NC, WI, VA, and WV); (4) states considering similar programs; (5) states with similar characteristics using k-means cluster method (AZ, CT,CO, DE, MT, PA, RI, VA) ; and (5) states with similar methods using Principal Component Analysis clustering (AK, AZ, CT, DE, KS, MI, MT, NV, NJ, PA, RI, VA, and WI) (see Appendix B for details). The estimated results presented in Table 7 explicitly demonstrate robustly and statistically significant effect of the OregonSaves on IRA enrolment, for both pooled sample and the sample contains workers at firms with more than 100 employees. Although the alternative control groups take into account the similarity between Oregon and comparison states, the baseline model is preferred since the findings can be generalized to the general population, which improve the external validity of the study.

5. Discussion and Conclusion

With concerns about undersaving for retirement across the developed world, there is intense interest amongst economists and policymakers regarding policies that can boost saving for retirement. This paper has studied the first state-mandated retirement plan in which employers are obliged to automatic enroll employees into a Roth IRA, which employees can then choose to leave if they wish. I provide the first assessment of the impact of automatic enrolment that allows for changes in employer behaviors in response to the policy in a context where those employers did not choose to introduce automatic enrolment, but instead were obliged to do it. This kind of

[19] Although MA has implemented its program since 2012, the program offers only for non-profit organizations. Therefore, I exclude MA due to difference in eligibility criteria comparing to other states' programs.

impact cannot be identified when automatic enrolment is introduced voluntarily by some large employers, such as was studied in Madrian and Shea (2001) and Choi et al. (2004).

I exploit the gradual roll-out by employer sizes of the state-mandated retirement plan in Oregon that requires employers to enroll their eligible employees to the auto Roth IRA between 2017 and 2019 (OregonSaves) to estimate the effects of the OregonSaves on IRA participation among private sector workers using a difference-in-difference model. Results from the DID models show that the OregonSaves led to slightly higher probability of IRA participation (2.8 ppts or 27 percent) than that of pre-reform period. The impact seems to be driven by employees who worked in larger firms (firms with over 100 employees and more), with an estimated rise of 2.6 ppts (or 24 percent) following the implementation of the Program, which is relatively sizeable. These findings are plausible given firms with at least 100 employees were the first to be required to set up the default plan during the study period. And hence, it is expected that the impact would have been larger among this group. However, the estimates are smaller than those in previous studies (roughly about a half of the estimate points as found in Madrian & Shea, 2001 and Choi et al., 2004) for two reasons. First, the IRA plan under the OregonSaves do not have employer matching feature, which might make the plan less appealing to workers. And second, it is still early to conclude the true effects of the program given the short time in the post-reform periods. In addition, the OregonSaves seems to have heterogenous effects upon different demographic groups, with larger effects were centered among female, non-white workers. More importantly, the Program led to significantly increase in savings among less educated and low-income employees who were considered as less active in savings for retirement in previous studies.

The peer effects observed in this study provides an important policy implication as the results show that firms that were obligated to facilitate retirement plans in later stages might want to move ahead of time and sign up before their required timeline is due. As a result, workers in this firm-size group were more likely to participate (an estimated increase by 15.4 ppts (or 109 precent) after the OregonSaves started to roll-out in November 2017. One possible explanation is that with low administration cost and no matching contribution under the state-run plan, small firms were more willing to enroll their employees into state plan as a means of attaining and/or attracting more workers. Findings from the study further suggests that

disadvantageous workers (older workers, less educated, and lower-income) were more likely to be beneficial from the Program to improve their retirement savings.

Several caveats are noted. First, the outcome measurement of IRA ownership using SIPP data may not be perfect since it is unable to distinguish whether an account is traditional or Roth IRA. Further, the outcome variable measure in the CPS-ASEC derives from question whether the Respondent had an employer-sponsored retirement plan, which also does not isolate whether it is a 401(k) or a new auto IRA plan. Second, in this study the post-treatment period has only two data point. While the short-term effect is of interest to understand the immediate effects of the program, longer post-treatment period will be useful to examine the dynamics of the effects. Finally, this paper only looks at the effect of savings through a workplace retirement plan. It is possible that the increase in savings in work place might be offset by reducing other saving vehicles such as traditional saving accounts or other IRAs. It is also likely that such increase might lead to increase in work hours such that the take home paycheck would not have changed. Understanding further the mechanism under this impact will be explored in the near future. However, as found in Chetty et al., 2014, most of the savers are passive, which means there might be less “crowd-out” effects potentially caused by the OregonSaves. Nevertheless, this first quasi-experiment provides important policy implications not only for Oregon but also for other states that enacted and considered to implement similar programs that significantly boosts retirement savings among uncovered workers in the private sectors.

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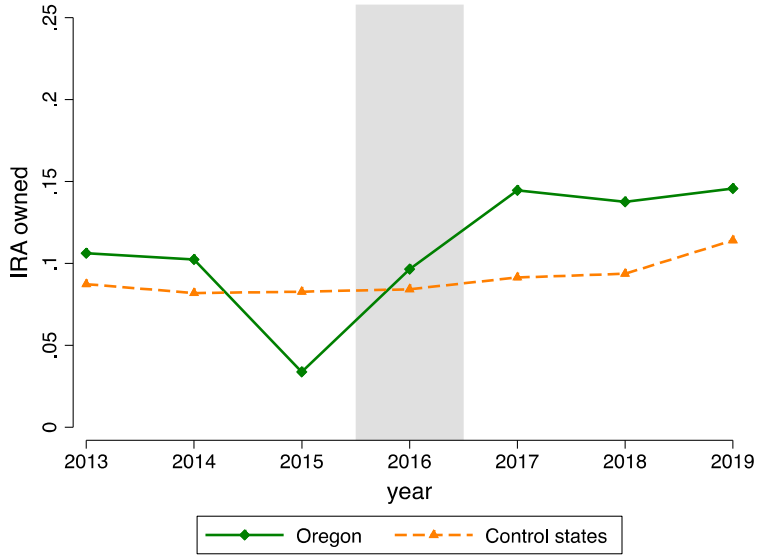
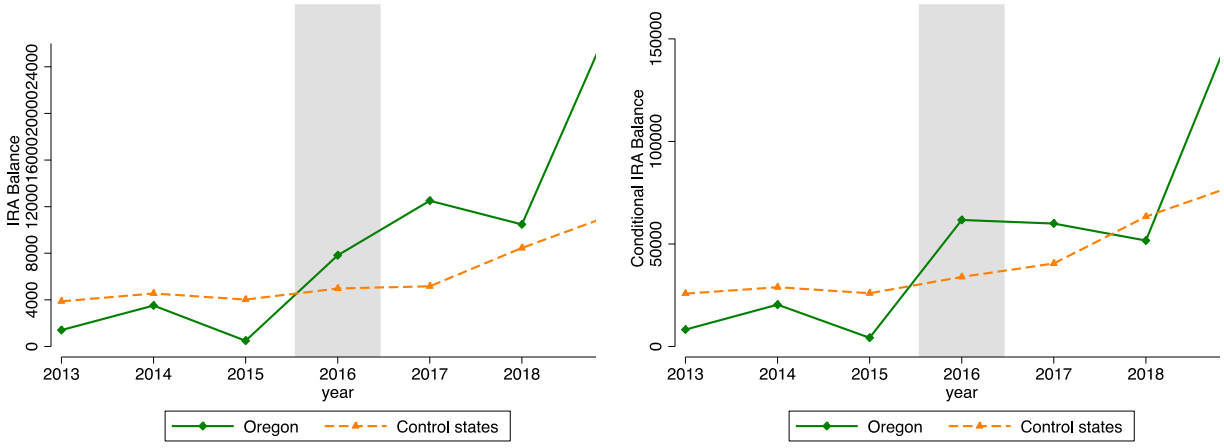
Figures

Figure 1. Time Trends in IRA Participation, 2013 - 2019



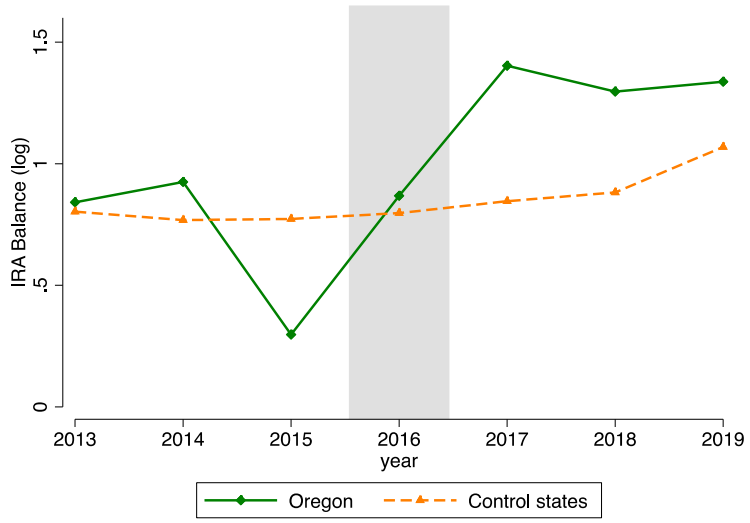
Notes: Data comes from the SIPP panel 2014 and 2018. Outcome variable: IRA participation, aggregated at state-by-year level, and adjusted by sample weights.

Figure 2. Time Trends in IRA Balance, 2013 - 2019



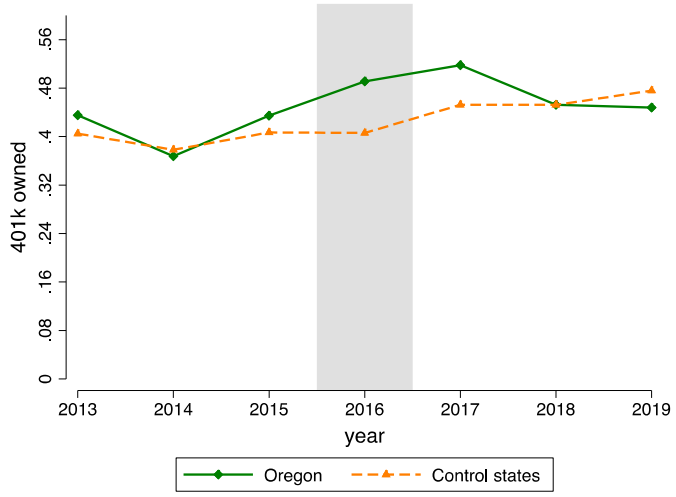
Notes: Data comes from the SIPP panel 2014 and 2018. Outcome variable: IRA Balance and IRA Balance conditioning on IRA ownership, aggregated at state-by-year level. Outcomes are adjusted by 2017 real dollars and by sample weights.

Figure 3. Time Trends in IRA Balance (Log), 2013 - 2019



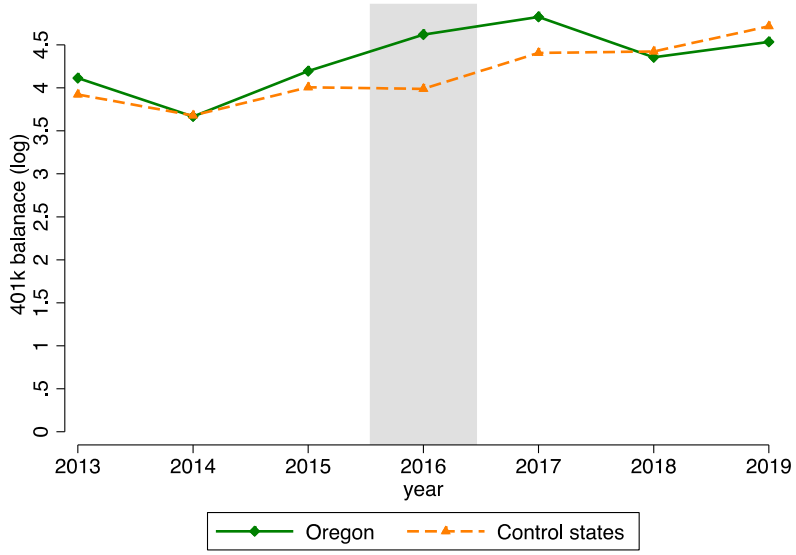
Notes: Data comes from SIPP panel 2014 and 2018. Outcome variable: log of IRA Balance, aggregated at state by year level. IRA balances are adjusted by 2017 real dollars and by sample weights.

Figure 4. Time Trends in 401k plan Participation, 2013 - 2019



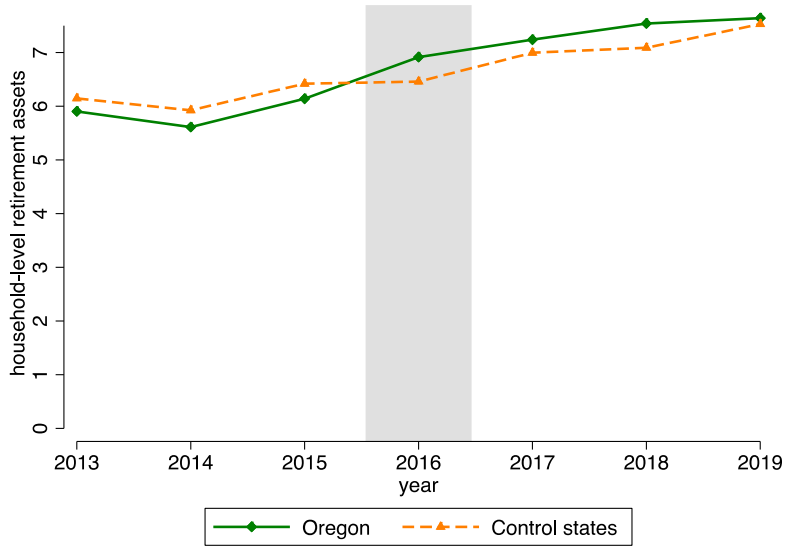
Notes: Data comes from SIPP panel 2014 and 2018. Outcome variable:401k plan participation, aggregated at state by year level, and adjusted by sample weights.

Figure 5. Time Trends in 401k plan Balance, 2013 - 2019



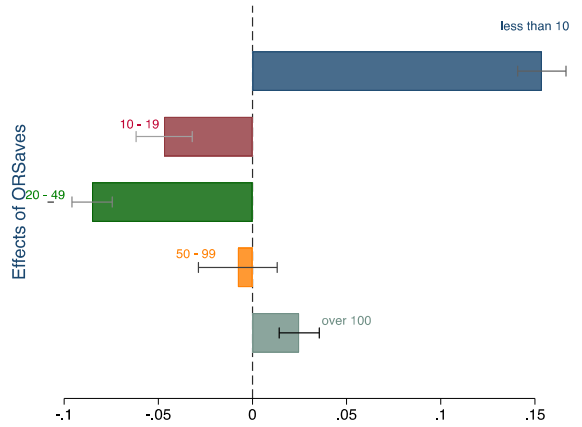
Notes: Data comes from SIPP panel 2014 and 2018. Outcome variable: log of 401k Balance, aggregated at state by year level. 401k balances are adjusted by 2017 real dollars and by sample weights.

Figure 6. Time Trends in House-hold Retirement Assets, 2013 - 2019



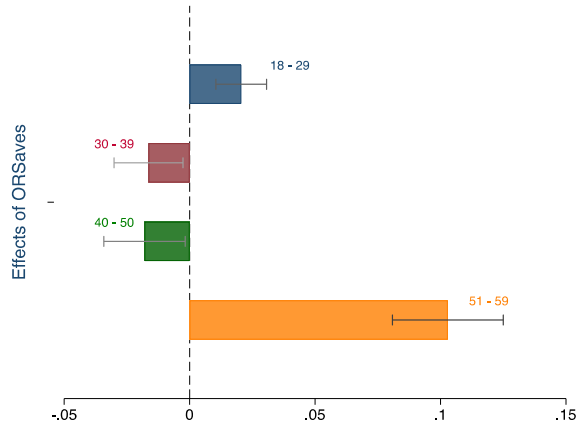
Notes: Data comes from SIPP panel 2014 and 2018. Outcome variable: log of household-level retirement assets, aggregated at state by year level. Household retirement assets are adjusted by 2017 real dollars and by sample weights.

Figure 7: Effects of the ORSaves by Firm sizes



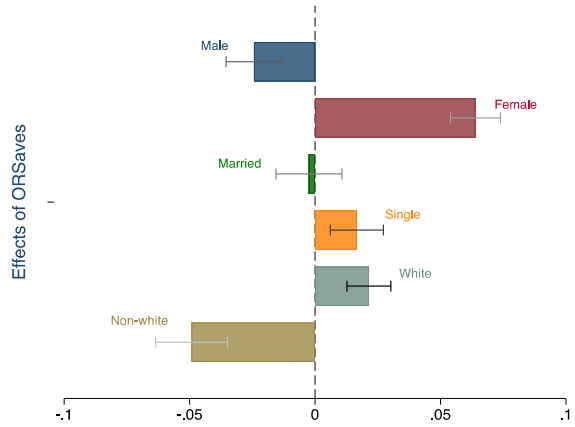
Notes: Data comes from SIPP panel 2014 and 2018. Each column is from a separate DID regression of IRA enrollment for years 2013-2019. Each bar (and its horizontal line) presents the coefficient estimate (and its 95% confidence intervals) from a separate DID model using samples divided by firm sizes: less than 10, 10 – 19, 20 – 49, 50 – 99, and over 100.Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, state-specific linear time trend, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Figure 8. Effects of ORSaves by Ages



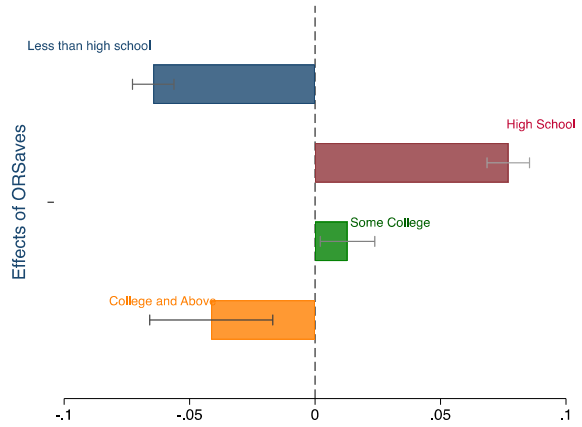
Notes: Data comes from SIPP panel 2014 and 2018. Each bar (and its horizontal line) presents the coefficient estimate (and its 95% confidence intervals) from a separate DID model using samples divided by 4 age groups: 18 – 29; 30 – 39; 40 – 51; and 51 - 59. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, state-specific linear time trend, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Figure 9. Effects of ORSaves by Gender, Marital Status, and Race



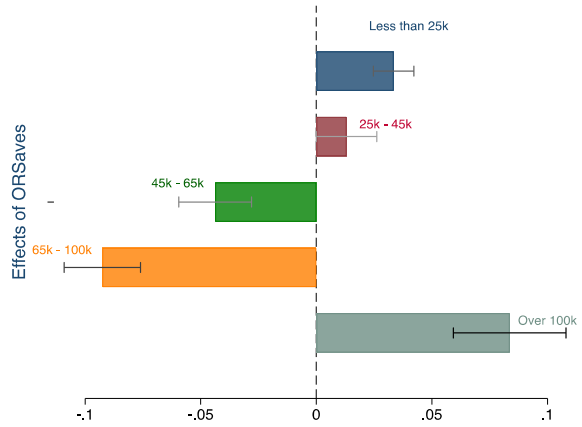
Notes: Data comes from SIPP panel 2014 and 2018. Each bar (and its horizontal line) presents the coefficient estimate (and its 95% confidence intervals) from a separate DID model using samples divided by gender, by marital status (married, single), and by races (white and non-white). Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, state-specific linear time trends, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Figure 10. Effects of ORSaves by Educational Attainment



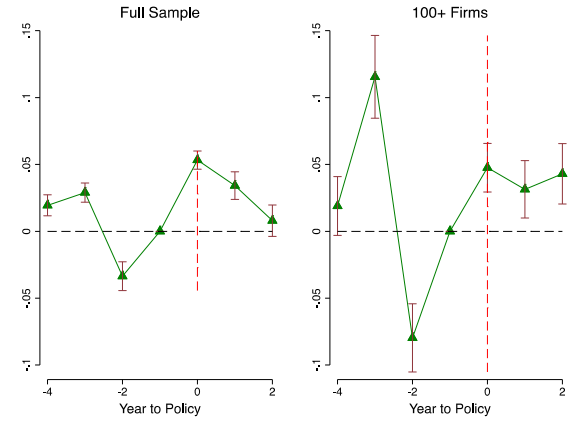
Notes: Data comes from SIPP panel 2014 and 2018. Each bar (and its horizontal line) presents the the coefficient estimate (and its 95% confidence intervals) from a separate DID model using samples divided by 4 groups of educational attainments: less than high school (no degree), high school graduates, some colleges, and college and above. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Figure 11. Effects of ORSaves by Family Incomes



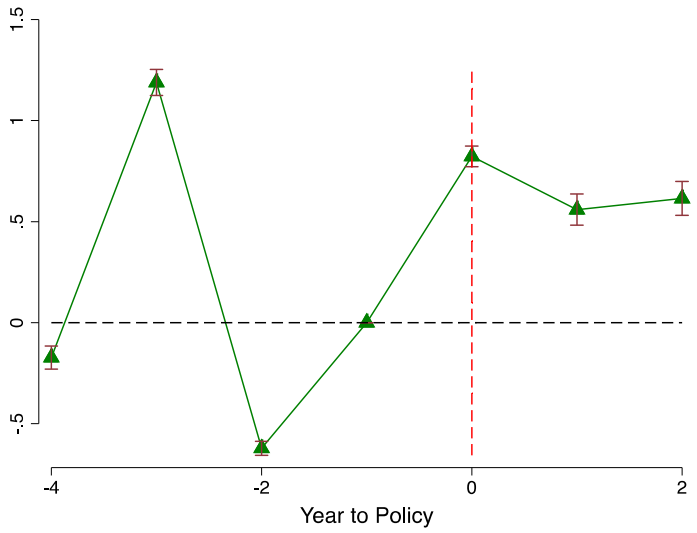
Notes: Data comes from SIPP panel 2014 and 2018. Each bar (and its horizontal line) presents the coefficient estimate (and its 95% confidence intervals) from a separate DID model using samples divided by 5 income groups (annual income): less than 25k, 25 – 45k, 45 – 65k, 65k – 100k, and over 100k. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, state-specific time trends, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Figure 12. Effects of ORSaves on IRA Participation – Event Studies



Notes: Data comes from the SIPP panel 2014 and 2018. Each graph presents each coefficient - βs - for each year (3 years before and 3 years after the Program launched) as specified in Equation (2). Year 2016 is the reference year. The feft graph is for full sample and the right graph is for individuals worked for firms with 100 and over employees. the Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Figure 13. Effects of ORSaves on IRA Balance (log) – Event Studies



Notes: Data comes from the SIPP panel 2014 and 2018. Each graph presents each coefficient - βs - for each year (3 years before and 3 years after the Program launched) as specified in Equation (2). Year 2016 is the reference year. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, state-specific time trends, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level.

Table 1. Summary Statistics

Sample Characteristics	Before 2017		After 2017		Net Difference	P-value
	Oregon	Other states	Oregon	Other states		
Outcome variables						
IRA ownership	0.1602	0.1580	0.1607	0.1289	0.0289	0.000
	(0.3668)	(0.3648)	(0.3674)	(0.3351)		
IRA Balance	4,951	4,563	16,567	8,002	8,050	0.000
	(45,766)	(37,665)	(182,719)	(93,792)		
Conditional IRA Balance	30,906	28,875	103,067	62,070	40,901	0.000
	(110,832)	(90,968)	(446,236)	(254,721)		
IRA Balance (log)	0.9110	0.8468	1.1160	0.8766	0.1632	0.012
	(2.8442)	(2.7810)	(3.1318)	(2.8261)		
401k ownership	0.5510	0.5398	0.5288	0.5198	-0.0036	0.677
	(0.4974)	(0.4984)	(0.4992)	(0.4996)		
401k balance	37,707	34,876	51,573	48,819	-330	0.899]
	(117,321)	(107,270)	(244,733)	(213,368)		
Conditional 401k balance	68,433	64,615	97,526	93,911	191	0.968
	(151,258)	(139,274)	(329,843)	(288,689)		
401k balance (log)	4.3721	4.0787	4.3471	4.4464	-0.4099	0.000
	(5.0800)	(5.1079)	(5.1001)	(5.1491)		
Household-level value of retirement assets	109,486	96,924	169,681	147,523	8,959	0.111
	(289,833)	(233,681)	(543,455)	(462,050)		
Household-level value of retirement assets (log)	6.5508	6.5985	7.2374	7.1494	0.111	0.232

	(5.3662)	(5.4264)	(5.1680)	(5.2911)		
Demographics						
Age	35	36	36	36	1.3033	0.000
	(11.16474)	(12.12826)	(11.8356)	(12.1414)		
Female	0.4922	0.4596	0.4064	0.4595	-0.0851	0.000
	(0.4999834)	(0.4983669)	(0.4912479)	(0.4983617)		
Married	0.3768	0.4058	0.4291	0.3929	0.0654	0.000
	(0.48464)	(0.4910378)	(0.4950242)	(0.4883903)		
Single	0.4723	0.4355	0.4084	0.4584	-0.0870	0.000
	(0.4992779)	(0.495828)	(0.4916273)	(0.4982703)		
Education						
High School or Less	0.4596	0.4632	0.5273	0.4647	0.0674	0.000
	(0.4984077)	(0.4986422)	(0.4993392)	(0.4987561)		
Some College	0.3691	0.3404	0.2677	0.3228	-0.0836	0.000
	(0.4826065)	(0.4738362)	(0.4428075)	(0.4675417)		
College and Above	0.1713	0.1965	0.2051	0.2125	0.0162	0.081
	(0.3768215)	(0.3973167)	(0.4038404)	(0.4090671)		
White	0.8921	0.7597	0.8848	0.7449	0.0087	0.373
	(0.3103173)	(0.4272637)	(0.3193427)	(0.4359323)		
Hispanic	0.2665	0.2506	0.2598	0.2694	-0.0217	0.021
	(0.4421672)	(0.4333719)	(0.4385961)	(0.4436713)		
Family Size	2.4545	2.9782	2.6557	2.9863	0.1954	0.000
	(1.466938)	(1.757597)	(1.508145)	(1.732043)		
Log (Family Income)	13.0247	13.0273	13.0279	13.0296	0.0010	0.007
	(0.0110093)	(0.0144984)	(0.0137114)	(0.0181038)		

Note: Data come from SIPP panel 2014 and panel 2018. Statistics for IRA ownership, IRA Balance, and Demographics are mean and standard deviations (in brackets), coming from a sample of employed private workers ages 18-60 who did not own 401k plans, while statistics for 401k plans (401k ownership and balances) and household-level retirement assets come from the general sample contains employed private workers ages 18-60. All monetary values are deflated in 2017 dollars. All statistics are adjusted by sample weights.

Table 2. Effects of OregonSaves on IRA Participation

	(1) Private Workers	(2) Private Workers	(3) Private Workers (Firms with over 100)
Oregon x After Nov 2017	0.034***	0.028***	0.026***
	{0.003}	{0.003}	{0.005}
Pre-Policy Mean	0.105	0.105	0.108
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Demographics Control	No	Yes	Yes
Adj R-Squared	0.02	0.12	0.10
N	717,917	717,917	191,970

Note: Data comes from the SIPP panel 2014 and 2018. Each column is from a separate DID regression of IRA enrollment for years 2013-2019. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Table 3. Effects of OregonSaves on IRA Ownership – by Firm sizes

Firm Sizes	(1) Less than 10	(2) 10 - 25	(3) 26 - 49	(4) 50 - 99	(5) Over 100
Oregon x After Nov 2017	0.154***	-0.057***	-0.085***	-0.008	0.026***
	{0.006}	{0.007}	{0.005}	{0.010}	{0.005}
Pre-Policy Mean	0.084	0.132	0.125	0.078	0.108
Adj R-Squared	0.155	0.149	0.123	0.122	0.103
N	178,354	158,112	106,802	82,679	191,970

Note: Data comes from SIPP panel 2014 and 2018. Each column is from a separate DID regression of IRA enrollment for years 2013-2019. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, state-specific linear time trends, and demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), and family size). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Table 4. Effects of OregonSaves on IRA Balance

	(1) IRA Balance	(2) Conditional IRA Balance	(3) IRA Balance (log)
Oregon x After Nov 2017	7489***	36839***	0.324***
	{600.986}	{8964.505}	{0.031}
Pre-Policy Mean	*4,548*	*43,419*	*0.9348*
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Demographics Control	Yes	Yes	Yes
Adj R-Squared	0.03	0.08	0.13
N	717,917	64,932	717,917

Note: Data comes from SIPP panel 2014 and 2018. Each column is from a separate DID regression of IRA enrollment for years 2013-2019. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). IRA balances are in 2017 real dollars.

Estimates are weighted using sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Table 5. Effects of OregonSaves on 401k plans

	(1)	(2)	(3)
	401k Participation	401k Balance (log)	Household-level retirement assets (log)
Oregon x After Nov 2017	0.003	0.013	0.426***
	{0.006}	{0.063}	{0.073}
Pre-Policy Mean	*0.461*	*4.444*	*6.595*
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Demographics Control	Yes	Yes	Yes
Adj R-Squared	0.26	0.31	0.30
N	1,248,269	1,248,269	1,248,269

Note: Data comes from SIPP panel 2014 and 2018. Each column is from a separate DID regression of 401k ownership (participation and balance) and total retirement assets for years 2013-2019. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), family size, and firm sizes). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Table 6. Effects of OregonSaves on 401k plans – By Firm Sizes

	(1)	(2)	(3)	(4)	(5)
	Less than 10	10 - 25	26 - 49	50 - 99	Over 100
Oregon x After Nov 2017	0.016*	-0.056***	0.025**	-0.006	0.031***
	{0.009}	{0.012}	{0.009}	{0.013}	{0.00781}
Pre-Policy Mean	*0.2542*	*0.4177*	*0.5626*	*0.6446*	*0.7266*
Adj R-Squared	0.196	0.200	0.218	0.199	0.196
N	241,284	225,619	169,846	148,237	463,283

Note: Data comes from SIPP panel 2014 and 2018. Each column is from a separate DID regression of 401k participation for years 2013-2019, split by 5 firm-size groups: less than 10, 10 – 19, 20 – 49, 50 – 99, and over 100 employees. Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), and family size). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Table 7. The Effects of the OregonSaves, using CPS data

	(1)	(2)	(3)	(4)	(5)
	Pooled Sample	Less than 10	10 - 99	100 - 499	Over 500
Oregon x After 2017	0.019***	0.045***	0.054***	0.017***	-0.015***
	{0.001}	{0.003}	{0.002}	{0.003}	{0.003}
Pre-Policy Mean	0.404	0.116	0.262	0.454	0.571
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Individual Demographic Controls	Yes	Yes	Yes	Yes	Yes
Adj R-Squared	0.192	0.066	0.116	0.130	0.149
N	326,278	46,112	84,289	46,372	149,505

Note: Data comes from CPS-ASEC running from 2013 to 2019. Each column is from a separate DID regression of binary variable whether being included in a pension plan at work place for the pooled sample, and by 4 firm-size groups: less than 10, 10 – 99, 100 – 499, and over 500 employees. Sample included private workers aged 18-60 who currently employed and also work in the previous year. Observations are at individual-year-state-demographic cell level. All models included state fixed effects, year fixed effects, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), and family size). Year 2017 is dropped. Estimates are adjusted by sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Table 8. Effects of OregonSaves – Using Alternative Comparison Groups

Treatment Effects	(1)	(2)	(3)	(4)
	Full Sample		Among Firms with 100 and over employees	
	IRA Participation	IRA Balance (log)	IRA Participation	IRA Balance (log)
OR vs. All other States (IL and WA excluded) - Baseline Model	0.028***	0.324***	0.026***	0.337***
	{0.003}	{0.031}	{0.005}	{0.050}
OR vs. States with similar Programs enacted (not yet implement)	0.038***	0.415***	0.028***	0.374***
	{0.006}	{0.048}	{0.004}	{0.048}
OR vs. States with similar Programs and States considering	0.030***	0.338***	0.028***	0.345***
	{0.007}	{0.066}	{0.007}	{0.075}
OR vs States considering similar Programs	0.020**	0.251***	0.020*	0.253**
	{0.009}	{0.082}	{0.011}	{0.107}
OR vs. Similar States (k-mean clustering)	0.039***	0.430***	0.041***	0.456***
	{0.011}	{0.075}	{0.010}	{0.091}
OR vs. Similar States (PCA clustering)	0.037***	0.403***	0.046***	0.505***
	{0.007}	{0.059}	{0.007}	{0.071}

Note: Data comes from SIPP panel 2014 and 2018. Each cell presents the coefficient estimate - β - as specified in the Equation (1) is from a separate DID regression for years 2013-2019, using full sample and sample consists of workers at firms with more than 100 employees. Outcome variables are IRA participation and IRA balance (log). Observations are at individual-month-year-state-demographic cell level. All models included state fixed effects, year fixed effects, demographics (age, gender, marital status, education, race/ethnicity, metropolitan status, family income (log), and family size). Estimates are weighted using sample weights. Standard errors are robust and clustered at state level, and in parentheses.

Appendix A

Table 1. Summary of State-Run Retirement Programs in Implementation

	California	Illinois	Oregon	Washington	Maryland	Connecticut	New Jersey	Vermont	New York	New Mexico
Date Enacted	September 29, 2016	January 5,2015	June 25,2015	May 18,2015	May 19, 2016	May 27, 2016	January 19,2016	June 8,2017	April 12,2018	February 26,2020
Date Effective/ Implemented	Effective: January 1,2017. Launched in July,2019	Effective: June 1,2017. Pilot Program: May 2018; and statewide phased enrollment began in November 2018	Pilot launched July 1, 2017 for employer with 100 and more employees, phased enrollment for small size employers	January 1,2017. Launched on March 19, 2018.	July 1, 2016 Not yet implemented	Effective date: January 1,2028 Not yet launched	Not specified	Implemented late 2020	Effective date: April 12, 2018. Implementation: within 24 months of the effective date	Effective date: July 1,2021 (Market Place) and January 1,2020 (Work and Save Program
Plan Type	Auto IRA	Roth IRA	Roth IRA	Market place (cover selection of SIMPLE IRAs, myRAs, Auto IRA, and/or life insurance plans for retirement purposes	At least one payroll deposit IRA	Auto IRA	Market place	Voluntary Multiple Employer Plan (MEP)	Roth IRA (Voluntary)	"Hybrid" – Voluntary Marketplace/Auto-enroll payroll deduction Roth IRA
Automatic Enrollment	Yes	Yes	Yes	No required	Yes	Yes	Not required	Yes	Voluntary	Yes
Default Contribution Rate	3% (Board can adjust from 2% to 5%)	3%	Standard – 5%, minimum rate is 1%,	Not specified	3%	3%	Not specified	Not speficied		Not specified

			and no maximum							
Definition of Employer	Employer with 5 or more employees, in-home services employers	Employer with 25 or more employees	Employer with 1 or more employee in 18 weeks of a calendar year or where the payroll amounts to $1,000 or more, not currently offering a retirement plan	Employer with 100 employees or fewer; self-employed individuals; sole proprietors	Business that pays employees through a payroll system or service and does not currently offer a retirement savings	Business with five or more employees not currently offering a retirement plan	Business with fewer than 25 employees, any majority of employees are employed in New Jersey.	Business with 50 employees or fewer not currently offering a retirement plan; self-employed individuals	Any employer without offering retirement plans in the last two years	All
Employer Contribution	No	No	No	No	No	No	No	Future voluntary contributions permitted	No	No

Source: Author's derivation from the AARP's State Retirement Resource Center. Retrieved from https://www.aarp.org/ppi/state-retirement-plans/savings-plans/Notes: Massachusetts limits its state-run retirement savings program to nonprofit organizations with 20 or fewer employees to offer retirement benefits through a 401(k) multiple employer plan (MEP)

Table 2. OregonSaves Implementation Timelines

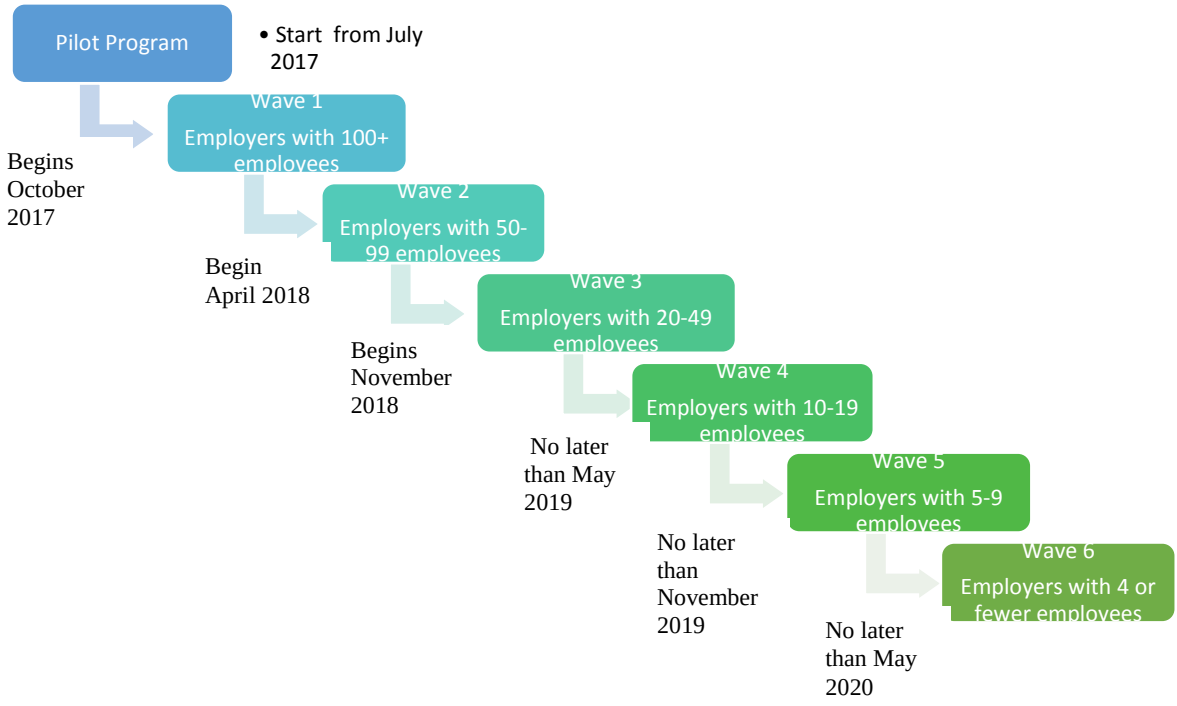


Table 3. Effects of ORSaves by Subgroups

Estimated Effects	Coefficient/SE	Pre-Policy Mean	N	Adj R-Squared
ORSaves Effects By Ages				
Younger than 30 years old	0.021*** {0.005}	0.052	256,758	0.05
30 - 39	-0.016** {0.007}	0.143	169,441	0.12
40 - 50	-0.018** {0.008}	0.137	158,053	0.13
51 - 60	0.103*** {0.011}	0.132	133,665	0.15
ORSaves Effects By Gender				
Male	-0.024*** {0.006}	0.1	371,895	0.12
Female	0.064*** {0.005}	0.109	346,022	0.13
ORSaves Effects By Marital Status				

Married	-0.002	0.173	295,034	0.14
	{0.007}			
Single	0.017***	0.068	303,201	0.09
	{0.005}			
ORSaves Effects By Races				
White	0.021***	0.107	551,159	0.13
	{0.004}			
Non-white	-0.049***	0.091	166,758	0.08
	{0.007}			
ORSaves Effects By Educational Attainment				
Less than high school	-0.064***	0.096	102,261	0.05
	{0.004}			
High School Graduates	0.077***	0.052	239,510	0.08
	{0.004}			
Some College	0.013**	0.061	235,650	0.09
	{0.005}			
College and Above	-0.041***	0.276	140,496	0.11
	{0.012}			
ORSaves Effects By Family Incomes				
Less than 25k	0.033***	0.041	159,511	0.06
	{0.004}			
25k - 45k	0.013*	0.086	162,716	0.07
	{0.007}			
45k - 65k	-0.044***	0.099	117,538	0.1
	{0.008}			
65k - 100k	-0.093***	0.18	128,484	0.12
	{0.008}			
Over 100k	0.084***	0.2	149,668	0.15
	{0.012}			

Appendix B. A Machine Learning Approach to Select Control States

I will use two commonly used methods – k-means clustering and Principal Component Analysis (CPA) to define states are in same cluster with Oregon based on state-level characteristics. I use ACS data in 2014 to collect state-level characteristics including population, share of 16-64-year-old population in labor force, share of uninsured population, share of white population, share of bachelor holders under 25 years old, median housing cost, median household income, poverty rate, employment rate among 16-64-year-old, share of population access to the internet, and share of married couple.

I employ k-mean clustering and CPA methods to divide the U.S states into difference regions based on state characteristics (Bayat et.al,2020). K-means is one of the most popular and efficient unsupervised clustering methods (Ding and He, 2004; Hasite etal.,2001; Hartigan &Wang,1979; MacQueen,1967) to partition data points into groups such that data points belonging to same cluster are similar and dissimilar if groups are in different classes (Panda et.,2006; Hamed,2019). The similarity and dissimilarity are obtained by measuring distances (which are Euclidean or Manhattan) (Kaufman and Rousseeuw,1990). Since 1957, this method of grouping data has been widely used in many fields such as banking, marketing, segmentation engines, healthcare, urban development, and privacy protection (Ghosal et.al.,2020).

The Principal Component Analysis (CPA) is the second technique used to identify control states. This method is used to simplify the complexity in high-dimensional data while assuring the patterns and trends. In order words, when variables in the data are correlated, the higher orders Principal Component (PCs) is able to captured more of the total variability in the data rather than any individual variable, which may reduce the influence of the outliners in the data.This method is applied in various fields as in economy, social issues (socioeconomic vulnerability (Miller,2014), poverty (Howe et.al.,2013), health vulnerability (Fisher et al.,2015; Zhe et al.,2014)), environment, job satisfaction, and energy (Moradi et al., 2018).

In this study, I apply k-means clustering and PCA clustering to select control states. Therefore, in measuring the distances across states, Oregon is considered as the center point (centroid) to determine these distances using state-level variables as mentioned above. The early results suggest the optimal number of clusters is 4. And the control states are selected if they are in Cluster 1 or in Cluster 1 and Cluster together. However, I will also conduct the sensitivity analysis of the results when the number of clusters change.

Table 3. Summarizes the selection of control states using k-means and PCA clustering

	Cluster 1	Cluster 1 and Cluster 2
K-means Clustering	Connecticut, Delaware, Rhode Island, Washington	Cluster 1, Arizona, Colorado, Illinois, Montana, Pennsylvania, and Virginia
Principal Component Analysis	Connecticut, Delaware, Illinois, Virginia	Cluster 1, Arizona, Alaska, Kansas, Michigan, Montana, Nevada, New Jersey, Pennsylvania, Rhode Island, Washington, Wisconsin

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