DEPARTMENT OF ECONOMICS
JOHANNES KEPLER UNIVERSITY OF LINZ

The Incidence of Workplace Pensions: Evidence from the UK's Automatic Enrollment Mandate

by

Rachel SCARFE
Daniel SCHÄFER
Tomasz SULKA

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Johannes Kepler University of Linz
Department of Economics
Altenberger Strasse 69
A-4040 Linz - Auhof, Austria
www.econ.jku.at

daniel.schaefer@jku.at

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Abstract

We examine who bears the costs of mandated workplace pension programs, exploiting the quasi-experimental rollout of automatic enrollment in the UK. Total compensation (take-home pay plus employer contributions) increases, driven by employer contributions, while the amount of take-home pay decreases. These effects differ by employer size, with take-home pay declining to an extent in the largest firms that we can rule out a pass-through to employees of more than 47%, significantly less than in smaller firms. Our findings provide the first evidence that large employers shift the cost of mandated automatic enrollment onto employees.

Keywords: Employer-sponsored retirement savings; Incentive design; Mandated benefits; Staggered difference-in-differences
JEL codes: D21; H22; J32; J38

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*Scarfe: University of Stirling, UK (email: rachel.scarfe@stir.ac.uk).
Schaefer: Johannes Kepler University Linz, Austria (email: daniel.schaefer@jku.at; corresponding author).
Sulka: DICE, University of Düsseldorf, Germany (email: sulka@dice.hhu.de).

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1. Introduction

In the United States, every fourth non-retiree has no savings for their retirement.[1] In response, US lawmakers have recently passed legislation requiring employers to enroll employees in a workplace pension plan unless the employees opt out, starting in 2025. Other countries have already implemented such automatic enrollment programs, with some mandating employers to contribute to pensions as well.[2] This policy intervention is motivated by the seminal work of Madrian and Shea (2001), who showed that requiring employees to make an active decision *not* to join a pension plan can substantially increase enrollment rates. However, the effectiveness of automatic enrollment in improving financial resources for retirement also depends on whether it has unintended consequences on wage rates and hours worked; if employers shift the cost onto employees by lowering wage rates or hours worked, employees will see a decrease in take-home pay. Yet, in stark contrast to other policy interventions, there is a surprising lack of empirical evidence on the incidence of mandated workplace pensions. We fill that gap using payroll-based longitudinal data and a quasi-experimental research design.

In 2012, the UK became one of the first countries to mandate employers to automatically enroll employees into a workplace pension and make employer contributions of at least 1% of their employees' earnings. By 2019, more than ten million employees have been automatically enrolled, which is every third employee.[3] The policy was rolled out based on an employer's number of employees, with the largest employers being required to introduce automatic enrollment first. Using a staggered difference-in-differences research design, we confirm that the policy achieves its goal of increasing workplace pension enrollment. Yet, we also provide the first evidence that automatic enrollment causes a decline in take-home pay (employee's basic pay plus extra pay), mainly driven by extra pay (overtime, shift, incentive, and other pay, e.g., meal allowances). This decrease in take-home pay partially offsets any rise in total compensation (employee's take-home pay plus employer contributions), such that

[1] Board of Governors of the Federal Reserve System (2022) analysis based on the Survey of Household Economics and Decisionmaking.

[2] Countries that have already implemented automatic enrollment programs and require employer contributions are Italy, New Zealand, Poland, and the United Kingdom (OECD, 2021). Ireland's automatic enrollment program is scheduled to start in late 2024.

[3] See Department for Work and Pensions (2020). The figure is based on comparing the number of employees enrolled in a workplace pension plan in 2012 and 2019.

only approximately half of the additional employer contributions are passed through to employees. These effects differ by employer size: we estimate that the pass-through to employees in the largest firm is close to zero and at most 47%, significantly less than in smaller firms where we cannot rule out full pass-through. We do not find evidence that basic hours worked decline, but our results suggest that reduced overtime hours and other pay, such as meal allowances, can partially explain our findings.

Our setting is ideal for analyzing the causal effects of automatic enrollment: The policy was introduced nationwide, such that the self-selection of firms into adopting automatic enrollment is not a concern for our identification strategy. Unlike previous studies discussed below, our estimates also encompass the effects of automatic enrollment on participation and wages in firms that previously did not voluntarily offer workplace pensions, arguably highly relevant for policymakers. We use data from the Annual Survey of Hours and Earnings (ASHE), a 1% random sample of income tax-paying employees in Great Britain. These data provide accurate, payroll-based information on employees' pay components, allowing us to analyze each component of total compensation separately, including basic pay, employer contributions to workplace pension schemes, and extra pay. In addition, the data allow us to track individual employees over time, which is crucial since changes in the sample composition can otherwise mask effects on average wages (Solon et al., 1994).

We propose a stylized contracting model augmented with workplace pension benefits that motivates our empirical approach and interpretation. Our model clarifies when the introduction of mandated benefits lowers the optimal extra pay rate despite dampening the employee's incentive to exert effort. The model can also account for pass-through being the smallest in large employers. First, when an employer's cost of adjusting the compensation package is fixed and positive (e.g., administrative costs of updating the payroll system), only large employers with high enough total labor cost savings find it optimal to implement an extra pay cut (Bloom and Van Reenen, 2007, 2010). Second, sorting of workers with stronger preferences for workplace benefits into large employers increases the magnitude of an optimal pay cut. This is because the additional employer contributions induce an income effect, making the marginal unit of effort more costly. The strength of the income effect increases with the valuation of the workplace pension.

In our empirical analysis, we use a difference-in-differences research design, where timing variation originates from the staggered rollout of automatic enrollment according to firm size (we use the terms employer and firm interchangeably). Each firm was assigned a staging date when automatic enrollment duties would become effective, based on its number of employees as of April 2012. Thus, every year between 2013 and 2016, we observe both firms that have already passed their staging date and firms that have not yet reached their staging date. To address recent econometric concerns with staggered difference-in-differences research designs, we use the estimator proposed by Callaway and Sant'Anna (2021) and discuss this choice below.

First, we focus on employees directly affected by the reform - employees who were not enrolled in a workplace pension plan in the year preceding automatic enrollment. We refer to these as *targeted* employees hereafter. We find that automatic enrollment caused a sharp rise of 75 percentage points in workplace pension participation rates among targeted employees after their firm's staging date relative to targeted employees in other firms. This increase persists up to four years after automatic enrollment is introduced. We do not find evidence that AE affects the hours worked by targeted employees. The policy increases an average targeted employee's total compensation by 0.9%. Decomposing this increase into the individual pay components, we find no evidence that the basic pay of employees responds to the policy. Instead, the primary driver of the growth in an employee's total compensation is the increase in employer contributions. This contribution increases significantly for targeted employees in firms past their staging date relative to those in other firms. However, the effect of automatic enrollment on total compensation is relatively muted due to a decrease of 0.9% in the amount of take-home pay. The implied pass-through to employees is 50%, meaning that only half of the additional pension contributions due to the introduction of AE benefit the employees. The employers recoup the other half of the additional pension contributions by reducing take-home pay. We can rule out both complete pass-through to employees and full cost-shifting by employers; both employees and employers carry some of the burden of mandatory workplace pension enrollment.

In the second part of our empirical analysis, we examine the potential mechanisms of our aggregate findings, specifically whether the response to automatic enrollment varies across different firm sizes. Pension participation rates increase in all firms, although somewhat less in small firms with the lowest enrollment rates before the reform. Contrary to our overall estimates, we estimate a pass-through in the largest firms of only 9.5%, and we can reject that more than 47% of the increase in employer contributions benefits the employees. The pass-through in smaller firms is significantly greater, and we cannot reject approximately full pass-through to employees. We present some evidence that these effects may be driven by reduced overtime hours and other pay, such as meal allowances.

Our paper contributes to several strands of literature. First, we add to the extensive body of research in behavioral economics, which shows that automatic enrollment in workplace pension plans significantly increases participation rates. In their seminal studies, Madrian and Shea (2001) and Choi et al. (2004) analyze how participation rates respond when large US firms voluntarily adopt automatic enrollment, finding that pension participation rates almost double. More recently, Chalmers et al. (2021, 2022) study the effects of a pension reform in Oregon, OregonSaves, which requires firms that do not offer a workplace pension to automatically enroll employees in a statewide pension plan. OregonSaves increases participation rates to between 34% and 62%, which is lower than among US firms that voluntarily adopt automatic enrollment and lower than our estimates. This prior work does not investigate the effects on participation rates of simultaneously mandating firms to automatically enroll employees and to make minimum pension contributions. In an earlier study of the UK's automatic enrollment reform, Cribb and Emmerson (2020) show that the reform substantially increases pension enrollment rates. Reassuringly, although we use a different estimation strategy that takes into account recent developments in the literature on difference-in-differences estimators, we can replicate their findings in our first-stage analysis. Finally, using a calibrated life-cycle model, Choukhmane (2021) finds that automatic enrollment has only minor long-run effects on US employees' wealth. His analysis focuses on the cumulative employee 401(k) pension contributions, but it is silent about the incidence of automatic enrollment costs and, thus, the policy's impact on disposable income.

Second, we contribute to the literature on the incidence of mandated benefits. Summers (1989) argues that if wages are not fully rigid, the cost to the firms of providing the benefits may be shifted onto employee wages. For the US, Gruber and Krueger (1991) find empirical evidence that a significant portion of the cost to the firm of providing workers' compensation insurance is largely shifted onto employees in the form of lower wages. Similarly, Gruber (1994) finds that the costs of health insurance coverage for maternity are shifted onto the employees who are most likely to benefit from the coverage. Gruber (1997) shows that the reduced costs of payroll taxation to employers are mainly passed on to employees through higher wages in Chile. In contrast, the study by Buchmueller et al. (2011) does not find that mandated health benefits have measurable effects on wages; rather, the mandate is associated with an increased reliance on exempt part-time workers. More recently, Saez et al. (2019) analyze the effects of payroll tax rate cuts for young workers in Sweden and find that firms increase the wages of all their workers collectively, both young and old, consistent with rent sharing of the cost reduction. None of these earlier papers analyze the incidence of the costs associated with providing a workplace pension plan and making contributions to it. A very relevant study is Bozio et al. (2023), who highlight that the incidence of payroll taxes depends on the tax-benefit linkage status. They show that employer social security contributions associated with expected employee benefits (such as public pensions) are shifted onto employees, consistent with our findings here. Relative to their insightful study, we provide three contributions: first, we study a different framework where employees can opt out from automatic enrollment, unlike with payroll taxes; second, we identify that extra pay is the main channel of the pass-through; third, we provide evidence that the difference in incidence depends on the size of the employer. Our finding that extra pay declines highlights the importance of studying the potential unintended consequences of automatic enrollment beyond its direct effect on workplace pension enrollment.

Finally, Bosch et al. (2022) find that wages tend to be lower when employers' pension contribution rates are higher in the Netherlands. However, Dutch legislation neither requires employers to set up a pension scheme nor prescribes contribution rates. Instead, these decisions are made through collective bargaining agreements. This makes it difficult to interpret their finding because relatively high pension contribution rates may be intended to compensate employees with relatively low wages. Perhaps the most closely related study is provided by Oleksiyenko (2021), who does not find any impact of the UK's automatic enrollment mandate on wages. However, she analyzes a dataset different from ours that only contains average annual earnings at the firm level, restricting her from controlling the changing composition of workers within firms over time.

The article is organized as follows. In Section 2, we provide further details on pension policy in the UK. We present our theoretical framework in Section 3.

Section 4 discusses the ASHE data we use. In Sections 5 and 6, we discuss our empirical approach and present our results in detail. Section 7 concludes.

2. Institutional Background

2.1. The UK Pension System

The current UK pension system comprises three tiers. The first tier is the state pension, which has traditionally been less generous than in other OECD countries. In the past three decades, public expenditure on old-age benefits expressed as a percentage of GDP has been approximately two percentage points lower than the OECD average of 6 to 8% and about one percentage point lower than that of the US (OECD, 2021). To receive this minimum, retirees must have paid national insurance (broadly equivalent to social security) contributions for at least ten years. However, the time is reduced for those who have been caring for children or receiving unemployment benefits. Individuals who have paid national insurance contributions for more years are eligible to receive a state pension of up to £185.15 per week. These figures mean that the UK state pension is less generous than that of other OECD countries: the maximum amount of £185.15 per week corresponds to a replacement rate of 22% of gross average earnings, compared to an OECD average of 42%.

The second tier of the UK pension system consists of mandatory, earnings-related pensions. The OECD considers the pension contributions required by automatic enrollment (AE) to fall into this category. Before 2016, there was a different earnings-related component of the UK state pension, which has since been phased out. The third tier comprises voluntary, earnings-based pensions. Given the low state pension in the UK, workplace pensions are an essential source of funds for many retirees. While 88% of those employed in the public sector had a voluntary workplace pension in 2012, only 42% of private sector employees in the UK participated in a workplace pension plan, with participation rates declining (Department for Work and Pensions, 2020). This is similar to the US, where only 48% of private sector employees participated in a workplace pension plan in 2012 (Bureau of Labor Statistics, 2012). Although the UK Welfare Reform and Pensions Act 1999 required employers to offer employees an optional stakeholder pension, it did not require employers to contribute. Consequently, a substantial share of employees had no financial resources to support them in later life: 19% in 2012, up from 15% in 2009 (MacLeod et al., 2012).

2.2. The Automatic Enrollment Mandate

In 2002, the UK government established an independent Pensions Commission to evaluate whether the current pension system was sufficient in light of concerns that workers were not saving enough for retirement. After three reports, the Pensions Commission concluded that current levels of saving were inadequate and recommended that the government require employers to automatically enroll their employees in a workplace pension scheme, with mandatory employer contributions. In response, the UK Parliament passed the Pensions Act in 2008, which introduced AE. Firms could choose whether to set up a new workplace pension scheme for AE or automatically enroll their employees in an already existing opt-in plan. Despite a change in the governing party in 2010, the implementation of AE began in 2012, reflecting the concern across all political parties that workers were not saving enough for retirement. The Pensions Act also established a non-profit pension scheme funded by a government loan (National Employment Savings Trust, NEST). This scheme was designed to reduce the costs of setting up a workplace pension scheme for small employers with low-paid employees.

AE was introduced gradually between October 2012 and February 2018 based on employer size, beginning with the largest employers. Initially, the minimum default contribution was set at 2% of the employee's qualifying gross earnings, of which at least 1% had to be the employer's contribution.[4] This was raised to 5% (2%) in April 2018 and to 8% (3%) in April 2019 (Department for Work and Pensions, 2020). Details of the staging dates by which firms were required to introduce AE are provided in Appendix Table B1. The staging date for employers with 30 or more employees was determined by the number of employees on the Pay-As-You-Earn (PAYE) income tax scheme in April 2012. The employer sizes relevant to staging dates were frequently changed before April 2012, with the final update being announced as late as January 2012. For employers with 29 or fewer employees in April 2012, staging dates were determined according

[4]Qualifying earnings is the band of earnings used to calculate contributions relevant for AE. For the 2022/23 tax year, this is between £6,240 and £50,270 a year. The following wage components are included in qualifying earnings: basic wages, extra pay, statutory sick pay, statutory maternity/paternity pay, and statutory adoption pay.

to the randomly allocated last two digits of the employer's PAYE tax number. These employers were assigned staging dates from June 2015 to April 2017 (see Appendix Table B2). Employers could choose to postpone the enrollment of their employees in a workplace pension by up to three months after their respective staging date. We do not observe which employers did so, but survey evidence suggests that most employers opted to postpone the enrollment of their employees (Department for Work and Pensions, 2016).

Employees eligible for AE are at least 22 years old but below the State Pension Age, earn at least £10,000 per year (gross), and are not already members of a qualifying pension scheme. Additionally, employees must work for their current employer for at least three months before becoming eligible. If an employee holds multiple jobs, the eligibility for AE is considered separately for each job based on the same criteria. Employees not eligible for automatic enrollment must be given the choice to join a workplace pension plan, but their employer does not have to provide contributions. Although employees can opt out of the pension scheme or stop contributing later, their employer must automatically re-enroll them every three years. The UK government encourages enrollment through tax incentives that take the form of favorable tax treatment for the automatic enrollment pension plan as compared to most other savings vehicles. Specifically, contributions are typically deducted from earnings by the employer before income tax is calculated, meaning that automatic enrollment contributions carry a tax relief of 20%, the basic rate that applies to qualifying earnings. Any returns on invested pension funds are also tax-exempt. However, withdrawals are subject to income tax. This means that some employees with particularly steep life-cycle income profiles may pay higher taxes on pension contributions in later life than those saved while being younger.

3. Theoretical Framework

In this section, we provide a model that motivates our empirical approach and interpretation. The starting point is a classical problem of designing workplace incentives in the spirit of a "firm sets wages" framework (e.g., Lazear, 2000; MacLeod and Malcomson, 1989). We incorporate workplace pension benefits into this framework to study the effects of the AE mandate on different components of compensation.

We first derive the condition for the firm to offer no benefits in *laissez-faire*, which allows us to analyze the effects of AE on the targeted employees' pay. We show that when basic pay is downward rigid, the firm's optimal response to the benefits mandate is to reduce extra pay. Additionally, we examine how the effects of AE on extra pay might vary with firm size, either due to adjustment costs or worker sorting.[5]

3.1. Environment

Consider a risk-neutral firm hiring a worker of known productivity. The firm's compensation package may consist of basic pay $w \geq 0$, workplace pension benefits $b \geq 0$, and an extra pay rate $x \geq 0$ per unit of worker's effort $e \geq 0$ (e.g., overtime hours, sales). All benefits up to a ceiling $\overline{b} > 0$ receive tax relief of $\tau \geq 0$, but offering positive benefits imposes a fixed setup cost of $\kappa > 0$ (e.g., administrative costs). Thus, the firm's profit function is:

$$\Pi(w,b,x,e) = y + ze - w - xe - (1-\tau)b - \kappa \mathbf{1}\{b > 0\}, \tag{1}$$

where $y > 0$ is the baseline productivity of the match and $z > 0$ is the marginal productivity of effort.

Once agreed upon by the firm and the worker, we assume that basic pay is downward (nominally) rigid, as suggested by the recent empirical evidence (Grigsby et al., 2021; Schaefer and Singleton, 2023).[6] In contrast, interim adjustments to the extra pay rate are possible but impose a fixed cost, which is motivated by the literature on implementing complex management practices (Bloom and Van Reenen, 2007, 2010). This may reflect the cognitive cost of re-optimizing the compensation package, the administrative cost of updating the payroll system, or the manager's disutility from communicating cuts in extra pay.

The worker has a strictly increasing, strictly concave, and differentiable utility from total compensation $u(\cdot)$ and a strictly increasing, strictly convex, and

[5] Here, we focus on a model of incentive pay. However, alternative theories of extra pay exist that can deliver contrasting predictions. For example, the extra pay response would be independent of an individual worker's pension enrollment status prior to the mandate in a model of profit-linked bonus pay (Oyer, 2004). Below, we show that this prediction is not supported empirically.

[6] In a dynamic environment, a weaker assumption of "relative wage stickiness", arising for example due to a social norm about basic pay (Hall, 2005), could be invoked to capture a constraint on the minimal growth rate of basic pay.

differentiable cost of effort $c(\cdot)$. For simplicity, we adopt a linear formulation as in Gruber (1997), such that the worker's valuation of workplace pension benefits is $q \times b$, with parameter $q > 0$ capturing the worker's preference for benefits relative to take-home pay.[7] Thus, the worker's utility function is:

$$U(w, b, x, e) = u(w + qb + xe) - c(e). \tag{2}$$

3.2. *Laissez-Faire*

The firm optimally offers workplace benefits in *laissez-faire* whenever the monetary cost of providing the benefit $\overline{b}$ is lower than the worker's valuation:

$$\kappa < [q - (1 - \tau)]\overline{b}. \tag{3}$$

Interpreting the fixed cost κ as independent of the number of participants in the workplace pension plan, this condition is more likely to hold in larger firms that can spread the setup cost across a greater number of employees, all else equal. Additionally, a higher average valuation of benefits in a firm would also increase the likelihood that the firm offers a workplace pension scheme. We discuss parameterizations under which the firm offers workplace benefits in *laissez-faire* at the end of this section, but for now, abstract from voluntary workplace pension schemes.

When effort cannot be contractually agreed, for any given extra pay rate x, the worker would choose the amount of effort that maximizes their utility. The firm anticipates the worker's effort choice (ICC) and participation constraint (PC) when designing the optimal compensation scheme:

$$\max_{w,x}\, y + ze(x) - w - xe(x), \text{ s.t.:}$$

(i) $u'(w + xe) \times x = c'(e)$

(ii) $u(w + xe(x)) - c(e(x)) \geq \underline{u}$,

[7]While $q = 1$ would imply that the worker treats take-home pay and benefits as perfectly fungible, our framework also spans parameterizations with either $q > 1$ (e.g., due to the value of commitment) or $q < 1$ (e.g., due to myopia).

where $\underline{u}$ denotes the worker's outside option. The profit-maximizing extra pay rate is:

$$x^* = z. \tag{4}$$

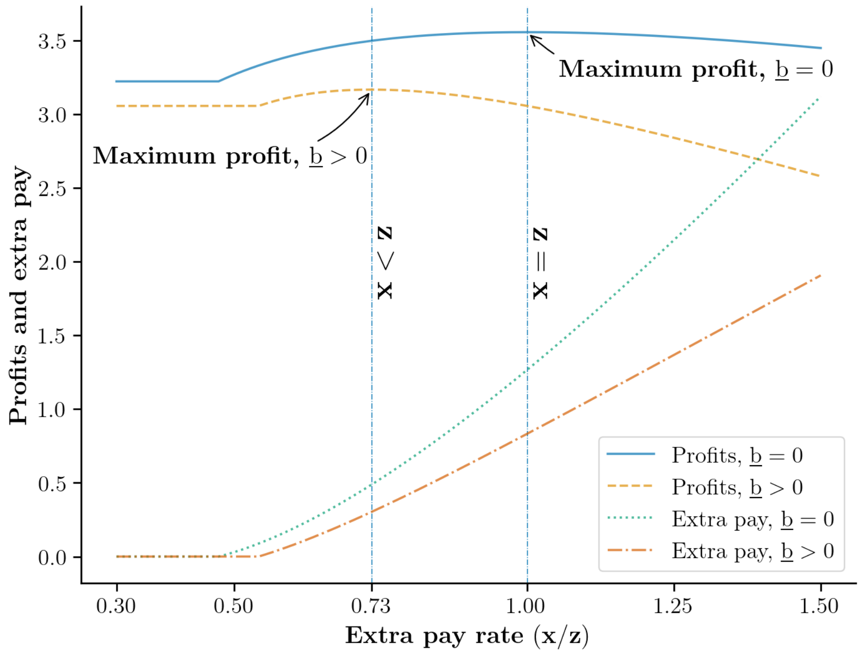

Figure 1: Profits and extra pay as functions of the normalized extra pay rate x/z

Notes: Numerical simulations of how the introduction of mandated benefits, $\underline{b}/w^* = 0.35$. We assume $q = 1$, $c(e) = \exp(\gamma e) - 1$ with $\gamma = 0.2$, and $u(w + b + xe) = ((w + b + xe)^{1-\eta} - 1)/(1 - \eta)$ with $\eta = 1.5$ (Groom and Maddison, 2019). The other parameters are $\underline{u} = 1/2$, $y = 5$, and $z = 1$. We set $\kappa = 0$ and $\tau = 0$ because these parameters only shift the profit function and do not affect the optimal x, conditional on an interior solution.

This solution is presented graphically in Figure 1 as the case without any mandated benefits ($\underline{b} = 0$). Since the worker's utility function is concave in extra pay and the cost of effort is convex, the marginal monetary cost of incentivizing effort is increasing. Beyond the point $x/z = 1$, the marginal cost exceeds the marginal product of effort and the firm's profit function becomes downward sloping in extra pay. In turn, the optimal basic pay is set to make the worker's participation constraint bind:

$$w^* = u^{-1}(\underline{u} + c(e(z))) - ze(z). \tag{5}$$

3.3. Mandated Benefits

Now, suppose that having previously contractually agreed to w^*, the employer is mandated to provide benefit $\underline{b} > 0$ to the worker. We interpret $\underline{b}$ as corresponding to the minimum firm contribution under the AE mandate.

Given that w^* is rigid, how might the extra pay rate respond to the mandate? On the one hand, with a compensation package consisting of w^*, x^*, and $\underline{b}$, the worker's PC becomes slack. This allows the firm to reduce extra pay while still retaining the worker. On the other hand, the provision of $\underline{b}$ generates an income effect that disincentivizes the worker to exert effort for any extra pay rate x:

$$\frac{d\,e(x,\underline{b},q)}{d\,\underline{b}} = -\frac{u''(\cdot)xq}{u''(\cdot)x^2 - c''(\cdot)} < 0. \tag{6}$$

To induce the previously optimal level of effort, the firm would need to raise the extra pay rate. This mechanism is apparent in Figure 1, which shows that holding the extra pay rate x constant, the introduction of mandated benefits ($\underline{b} > 0$) lowers the amount of extra pay $x \times e(x,\underline{b})$ at any x.

Which of these two opposing forces on x dominates? If the firm decides to adjust its compensation scheme, the optimal extra pay rate solves the following:

$$\max_x \; y + ze(x,\underline{b},q) - w^* - xe(x,\underline{b},q) - (1-\tau)\underline{b} - \kappa, \text{ s.t.:}$$

$$\text{(i)} \;\; u'(w^* + q\underline{b} + xe(x,\underline{b},q)) \times x = c'(e(x,\underline{b},q))$$

$$\text{(ii)} \;\; u(w^* + q\underline{b} + xe(x,\underline{b},q)) - c(e(x,\underline{b},q)) \geq \underline{u}.$$

The derivative of the firm's profit function evaluated at x^* is strictly negative:

$$\left.\frac{d\,\Pi}{d\,x}\right|_{x=x^*} = -e(x^*,\underline{b},q) + \underbrace{(z-x^*)}_{=0}\frac{d\,e(x^*,\underline{b},q)}{d\,x} < 0. \tag{7}$$

This is just an application of the envelope theorem - given that the worker responds optimally to a given level of incentives, the firm's desire to exploit the slack PC dominates at the margin. Nevertheless, the extent to which extra pay should be reduced would also reflect the fact that mandated benefits blunt the incentive power of extra pay. Figure 1 displays the outcome.

The incentive to reduce extra pay reflects the fact that the minimum benefits mandate simultaneously relaxes the worker's PC and generates an effort-reducing income effect. Importantly, the worker's valuation of the benefits, q, affects the relative magnitude of these effects. First, the PC implies that the firm's room to lower the worker's extra pay is increasing in the valuation of the benefits. Second, the ICC implies that the distortionary income effect is also increasing in magnitude in q, see equation (6). While it can be shown that the optimal effort level $e(x,\underline{b},q)$ is decreasing in q due to an increase in the marginal cost of inducing effort, our general model does not deliver a sharp prediction regarding how the optimal extra pay rate x varies with q. If the firm's profit-maximizing response to the introduction of mandated benefits is to raise x sufficiently, then this might overcompensate the decline in effort, leading to an increase in extra pay. If the firm does not increase the extra pay rate sufficiently, extra pay will decrease.[8] Figure 2 displays the results of numerical simulations, suggesting that the optimal amount of extra pay is indeed decreasing in the worker's valuation q for an entire range of minimum benefit levels $\underline{b}$. Thus, at least in this setting, the simulations confirm the intuitive notion that the magnitude of the optimal extra pay cut should be increasing in q.[9]

3.3.1 Heterogeneous Response Across Firm Sizes

While the income effect induced by mandated benefits should be present in all firms, we now explore two possible mechanisms that can generate a differential response of extra pay to the introduction of a minimum benefit mandate across firm sizes: fixed adjustment costs and sorting of employees with heterogeneous benefits valuations.

Fixed adjustment costs. Larger firms might use better management practices to minimize the costs associated with implementing AE. This idea is motivated by the concept of "X-efficiency" (Leibenstein, 1966) and supported empirically by Bloom and Van Reenen (2007, 2010). Suppose that adjusting the extra pay rate imposes a fixed adjustment cost of $\alpha \geq 0$ and let the improvement to the firm's

[8] Differentiating the firm's objective shows that the sufficient condition for x to be decreasing in q is:

$$\frac{d\,e(x,\underline{b},q)}{dq} \times \frac{d\,e(x,\underline{b},q)}{dx} \geq e(x,\underline{b},q) \times \frac{d^2\,e(x,\underline{b},q)}{dx\,dq}. \tag{8}$$

[9] The kink in the profit-maximizing extra pay cut appears where the worker's PC begins to bind. Beyond that point, larger cuts in extra pay are no longer feasible.

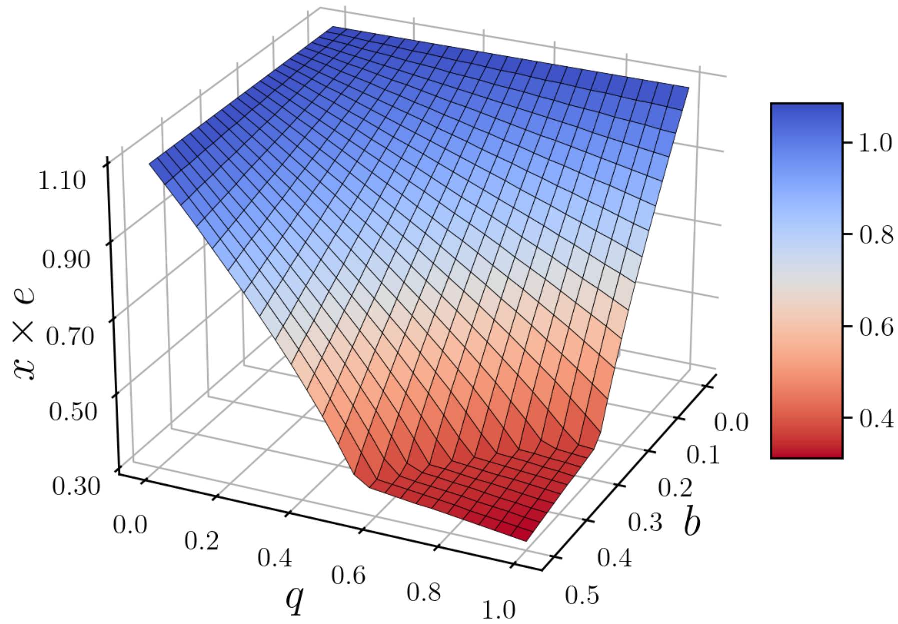

Figure 2: Profit-maximizing extra pay

Notes: Numerical simulations of how the introduction of mandated benefits affects the profit-maximizing extra pay ($x \times e$) for different values of q. See Figure 1 for more details on the underlying functional forms and parameterization.

profit associated with adjusting the extra pay rate be denoted by $\Delta\Pi > 0$. A firm employing $N > 0$ homogeneous workers bears this adjustment cost and lowers the extra pay rate if and only if:

$$N \times \Delta\Pi \geq \alpha, \tag{9}$$

which requires N to be large enough. Condition (9) has a natural economic interpretation: when the firm's cost of adjusting the compensation package is positive, only large firms with high enough total labor cost savings find it optimal to reduce their workers' extra pay in addition to the income effect. Without imposing stronger assumptions, however, it is not possible to find an analytic expression for the magnitude of the optimal decrease in extra pay, and hence $\Delta\Pi$. Numerical simulations suggest that adjusting the extra pay rate reduces the firm's profit loss relative to *laissez-faire* especially for moderate levels of mandated benefits, see Appendix Figure C1.

Sorting So far, we have taken the valuation parameter q of a representative worker as exogenously given. Because the firm's optimal response to the introduction of AE may depend on q, we now consider how worker sorting can affect the distribution of q across firms. For illustration, assume that in large firms, the setup costs are approximately zero so that every worker with $q > (1-\tau)$ participates in a workplace pension in *laissez-faire*, see condition (3). In small firms, by contrast, the setup costs are prohibitively high, so no worker is offered a workplace pension. To focus on situations where both small and large firms exist before the mandate, suppose that there is a continuum of workers with valuations of benefits distributed over the interval $[\underline{q}, \overline{q}]$ with $\underline{q} < (1-\tau) < \overline{q}$ and continuous density $f(q)$.

Without sorting, workers are assigned randomly to firms, which implies:

$$\mathbf{E}^{\text{rand}}[q \,|\, \text{T \& small}] = \mathbf{E}[q] > \mathbf{E}[q \,|\, q \leq (1-\tau)] = \mathbf{E}^{\text{rand}}[q \,|\, \text{T \& large}],$$

whereby $\mathbf{E}^{\text{rand}}[q \,|\, \text{T \& small}]$ is the expected value of q of a targeted employee in small firms under random assignment (analogously for large firms). Thus, under random assignment of workers into firms, the average q of NPP employees in small firms is relatively large, such that we expect to observe extra pay cuts of a greater magnitude in small firms. Intuitively, by opting out, the targeted workers in large firms reveal themselves to have low values of q, which limits the scope for reducing their extra pay.

Now, suppose that workers sort into firms based on their valuations of benefits. The workers with the highest q have the greatest incentive to find employment in large firms because these are more likely to offer benefits in *laissez-faire*, and the workers with the lowest q have the weakest incentive. Suppose that all workers with $q \geq (1-\tau)-\epsilon$ are employed in large firms, and all workers with $q < (1-\tau)-\epsilon$ are employed in small firms, for some $\epsilon > 0$. Then, we can illustrate perfect sorting by $\epsilon \to 0$, which implies:

$$\mathbf{E}^{\text{sort}}[q \,|\, \text{T \& small}] = \mathbf{E}[q \,|\, q < (1-\tau)] \; < \; (1-\tau) = \mathbf{E}^{\text{sort}}[q \,|\, \text{T \& large}],$$

whereby the expected value under perfect sorting is $\mathbf{E}^{\text{sort}}[\cdot]$. Perfect sorting implies that any targeted employee observed in large firms must have a value of q very close to $(1-\tau)$, while targeted employees in small firms have values of q from the entire range of the distribution below $(1-\tau)$. Thus, under perfect sorting, we expect to observe the opposite from random assignment: extra pay cuts among targeted employees should be of greater magnitude in large firms. This argument shows that the extent to which workers sort based on their valuations for workplace benefits can be crucial for the incentives of small and large firms to reduce targeted employees' extra pay.

Taken together, the model clarifies when firms might primarily respond to mandated workplace pensions by reducing extra pay, with the magnitude of extra pay cuts possibly differing across firms. Overall, we obtain the following result:

Proposition 3.1. *In response to the introduction of a minimum benefits mandate* $\underline{b} > 0$*, the following statements hold for targeted employees:*

(a) The optimal extra pay rate decreases below x^**.*

(b) The magnitude of an optimal extra pay cut is increasing in the worker's valuation of benefits q*, with* (8) *being a sufficient condition.*

(c) If q *is common across workers and the cost of adjusting the compensation package is positive, then only sufficiently large firms for which* (9) *holds reduce extra pay.*

(d) If q *differs across workers, (b) applies, and (3) holds more likely in large firms, then sufficiently strong sorting based on workers' valuations implies that the magnitude of an optimal extra pay cut is greater in large firms.*

Thus, while observing smaller extra pay cuts among large firms requires both negligible adjustment costs and sufficiently weak sorting, observing greater extra pay cuts among large firms would be consistent with significant adjustment costs, sufficiently strong sorting, or both.

4. Data and Descriptive Statistics

4.1. The Annual Survey of Hours and Earnings

The Annual Survey of Hours and Earnings (ASHE) (Office for National Statistics, Released 28 September 2022) is an ongoing panel study based on a 1% random sample of income tax-paying employees in Great Britain, who are tracked longitudinally. The survey questionnaire is sent to employers who are legally obliged to respond. Information is provided concerning the pay period that includes a specific survey reference date in April. Although the usual pay period is a calendar month, other pay periods, such as weekly or bi-weekly, are also possible. We do not observe the reported totals for these periods; instead, the dataset provides weekly averages of variables.

The design of the ASHE implies that we only have data when the individual was employed at the survey reference date. The longitudinal aspect of the ASHE allows us to track employees over time and link them to their respective firms using the firm identifiers provided in the ASHE. The ASHE is particularly suitable for our analysis because firms report employee earnings with reference to their payroll, which makes the data more accurate than household surveys (Elsby et al., 2016). We have access to detailed information on both basic pay and extra pay, such as overtime pay, incentive pay, shift-premium pay, and other forms of pay, including meal allowances, as well as hours of work. The ASHE also provides separate reports on the firm's and the employee's contribution to a workplace pension. Table 1 summarizes the pay variables.

Table 1: Overview of variables

	Description
ASHE variables	
Basic pay	All basic pay, excl. any extra pay, before deductions
Employer contributions	Employer's contributions to the employee's pension
Overtime pay	Overtime pay
Shift premium pay	Premium payments for shift, night, and weekend work
Incentive pay	Incentive pay received for work carried out in the pay period
Other pay	Pay received for other reasons, e.g., meal allowances
Basic hours worked	Hours relating to basic pay, incl. hours paid at shift premium
Overtime hours worked	Hours relating to overtime pay
Total hours worked	Sum of basic and overtime hours
Derived variables	
Enrollment rate	Share of employees on payroll with positive pension contributions
Extra pay	Sum of overtime, shift, incentive, and other pay
Take-home pay	Sum of basic and extra pay
Total compensation	Sum of take-home pay and employer contribution

Notes: We provide each variable's exact definition as in the ASHE questionnaire in Appendix A.

Another feature of the ASHE is its accurate information on a firm's total employment on the reference date in April, which is obtained from the UK government's interdepartmental business register and is added to the ASHE dataset. This information is essential in identifying when employees are affected by the pension reform, as a reliable measure of firm size is needed to determine staging dates. The ASHE data also include supplementary information regarding an employee's characteristics, such as age, gender, occupation at a 4-digit level, full-time status, type of contract (permanent or temporary), employment start date, whether pay is determined based on any form of a collective agreement, and the location of the employee's workplace. On the firm side, we observe the industry at a 4-digit level, whether the company is a private or public sector firm, and non-profit status.

4.2. Sample Construction and Descriptive Statistics

We define an employee as participating in a workplace pension plan if we see a positive value for the employee or employer contribution to a workplace pension in a given year. We keep only private sector firms since employees in the public sector typically had workplace pensions before the AE reform. We focus on employees targeted by the reform: those who were not participating in a workplace pension plan in the year immediately before the mandatory introduction of AE, hereafter referred to as *targeted* employees. In our baseline analysis, we consider employees who remain in the same firm from one year to the next (job stayers), preventing changes in the sample of employees from affecting the measurement of average wages (Solon et al., 1994). In our empirical analysis, we focus on employees who meet the criteria for automatic enrollment, which include being aged 22-64, having earnings of at least £10,000 per year (gross), and having worked for their current firm for at least three months.[10]

Table 2 presents descriptive statistics of our baseline analysis sample of targeted employees as of April 2012, before the implementation of AE (see Appendix Table B4 for statistics on other employees). The participation rates in workplace pension schemes are shown for employees with and without prior pension schemes under the category "All employees". Pension enrollment rates decline when firms have fewer employees. We observe high enrollment rates among employees in firms with AE staging dates by April 2013 and 2014, at 49% and 52%, respectively. Employees in firms where AE was introduced after April 2016, referred to as the "Not treated" group, have the lowest pension participation rate, with less than 22% in 2012. Viewed through the lens of our theoretical framework, this may be explained if workplace pension plans require a fixed setup cost or workers with high valuations of benefits sort into large firms.

[10]We provide further details of the data and sample construction in Appendix A.

The share of employees whose pay is set with reference to any form of a collective agreement, such as a national or industry agreement, is generally small and declines with the AE staging date. To compute an employee's total compensation, we sum their extra pay and basic pay (the firm's contribution to the workplace pension scheme in 2012 was initially zero in our sample by construction). Targeted employees in the largest firms receive the lowest total compensation: £451.7 per week on average. Total weekly compensation is notably higher among employees in smaller firms with later staging dates. Employees in the largest firms are the most likely to receive some extra pay (53.5%), and this likelihood declines as firm size decreases.

Table 2: Descriptive statistics as measured in April 2012, private sector

	Date when AE became mandatory				
	2013	2014	2015	2016	Not treated
	(1)	(2)	(3)	(4)	(5)
Firm size band (employees)	6,000+	160-5,999	50-159	5-49	5-30
All employees					
Share with workplace pension (%)	49.4	52.3	37.0	28.3	21.7
Targeted employees					
Share full-time contract (%)	81.3	88.8	90.3	88.0	85.4
Share permanent contract (%)	92.6	93.3	96.1	96.8	97.5
Share collective agreement (%)	8.5	5.4	3.1	2.1	1.6
Share men (%)	54.3	59.2	59.7	60.0	60.5
Age (years)	38.5	39.1	40.7	41.5	39.8
Basic pay (weekly, £)	409.5	461.7	471.2	478.6	473.1
Extra pay (weekly, £)	41.8	48.9	43.4	37.1	28.4
Ratio extra pay to basic pay (%)	10.3	10.6	9.2	7.9	6.0
Total compensation (weekly, £)	451.7	511.2	515.0	517.9	502.8
Share with positive extra pay (%)	53.5	44.9	40.3	32.7	26.8
Extra pay, if positive (weekly, £)	67.4	96.5	95.2	98.2	89.5
N (Employees)	9,591	12,961	5,099	3,982	6,074

Notes: All values refer to April 2012 before the introduction of AE. Basic and extra pay are converted to 2020 values using the UK consumer price index. Workplace pension participation implies a positive value for the employee's or firm's contribution to a workplace pension plan in a given year. "Not treated" are those employees of firms that are not required to introduce AE by April 2016.

5. Empirical Framework

Firms that existed in 2012 were required to introduce AE between 2013 and 2017, and their staging dates were determined based on the number of employees in April 2012 (see Appendix Table B1). This staggered rollout means that we observe both firms that have already passed their staging date and those that have not yet done so, for each year between 2013 and 2016. We identify the causal effects of AE on targeted employees' wages, hours, and workplace pension participation using a difference-in-differences research design. We only include targeted employees in the treatment and control groups because some firms already had workplace pension schemes before the mandatory introduction of AE, and targeted employees opted not to participate in those schemes. As we discuss in our theoretical framework, this self-selection into workplace pension participation may suggest that NPP employees' valuation of benefits systematically differs from those of employees who chose to join an available workplace pension scheme.

Table 3: Allocation to treatment and control groups based on the firm size in April 2012

Firm size in April 2012	Allocation to treated or not-yet-treated (control) groups				
	April 2012	April 2013	April 2014	April 2015	April 2016
30,000+	Control	Treated	Treated	Treated	Treated
6,000 - 29,999	Control	-	Treated	Treated	Treated
350 - 5,999	Control	Control	Treated	Treated	Treated
160 - 349	Control	Control	-	Treated	Treated
58 - 159	Control	Control	Control	Treated	Treated
50 - 57	Control	Control	Control	-	Treated
30 - 49	Control	Control	Control	Control	Treated
Fewer than 30	Control	Control	Control	Control	Control/Treated*

Notes: *Whether firms with fewer than 30 employees had to introduce AE by April 2016 was determined by the randomly allocated last two digits of an employer's Pay-As-You-Earn tax code, see Appendix B for details.

Table 3 presents the four treatment groups indexed according to the year when the treatment occurred first, $g = 2013, ..., 2016$. Firms were allowed to postpone the introduction of AE up to three months after their assigned staging date, so we classify employees of firms with a staging date between February and April of each year as neither in the treatment group nor in the control group and exclude them from our analysis for that particular year. In the subsequent year, when their treatment status is no longer ambiguous, we include such employees in our study. For example, we exclude firms with 6,000-29,999 employees from treatment and control groups when estimating the effect of AE between 2012 and 2013. However, we include 6,000-29,999 firms in the treatment group when estimating the effects over 2012 and 2014. We have verified that none of our coefficient estimates changes notably when we use only firms with fewer than 30 employees that were not treated during our sample period as the control group.

Figure 3 displays the average workplace pension participation rates of all job stayers, including targeted employees and others, across different firm size bands over time, updating Figure 2 in Cribb and Emmerson (2020) to 2016.

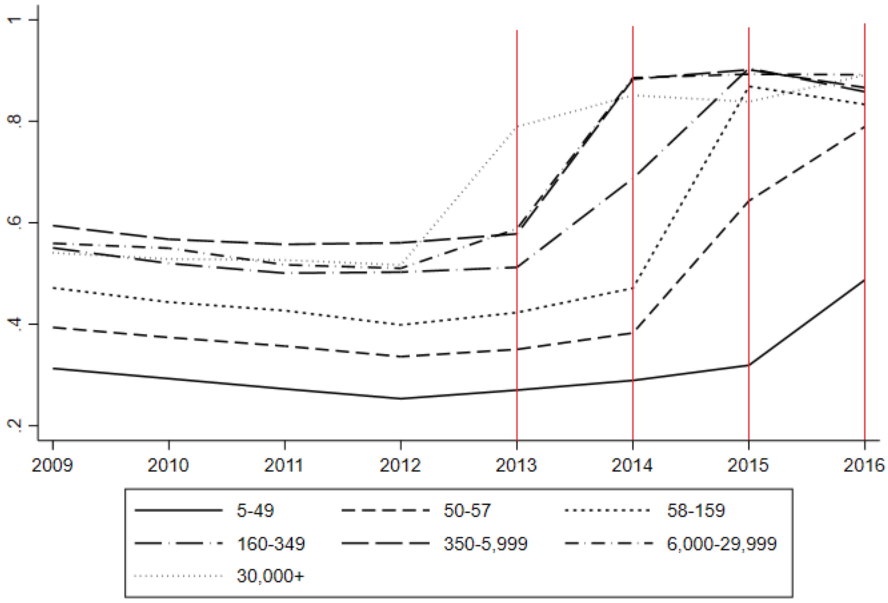

Figure 3: Enrollment rates of job stayers in the private sector, targeted and other employees

Notes: Average workplace pension enrollment rates of job stayers within each firm size band. Values are given for April. Vertical lines indicate periods of treatment for some treatment groups; see Table 3 for the allocation to treatment groups based on firm size and Appendix Table B1 for the exact staging dates.

The figure shows a significant increase in enrollment rates following the introduction of AE. For example, firms with 30,000 or more employees in April 2012 had to introduce AE by April 2013, and the pension participation rates among job stayers in this firm size band increase from 49% in April 2012 to around 80% in April 2013. There is evidence that firms use the option to postpone the introduction of AE. For example, firms with 50-57 employees in April 2012, which have their staging dates between March and April 2015, display a partial increase in workplace pension enrollment rates by April 2015, with a notable further increase by April 2016. Some firms' enrollment rates increase the year before the staging dates. Our sample definition excludes these "early-adopting" employees from the treated and control groups because only employees not participating in a workplace pension scheme in the year immediately before the staging date are included. However, if not-yet-treated firms started lowering wages before their AE staging date while also not enrolling their employees in a workplace pension, then our estimates of targeted employee wage changes in treated firms compared to not-yet-treated firms would likely provide a lower bound of the effects of AE.

To implement the outlined difference-in-differences method econometrically, we use the estimator proposed by Callaway and Sant'Anna (2021) (hereafter CS). This estimator addresses the recent concerns about the reliability of results obtained using staggered difference-in-differences research designs.[11] We estimate each group-time average treatment effect on the treated (ATT) of a group g at time t ATT(g,t) with its sample analog, $\widehat{\text{ATT}}(g,t)$, see Appendix E for details on the method. This process yields many $\widehat{\text{ATT}}(g,t)$, which we aggregate into three summary measures. The first measure is a weighted average of all $\widehat{\text{ATT}}(g,t)$ for $g \leq t$, where the weights are proportional to the treatment group size. This *overall ATT* is:

$$\hat{\theta}_O = \frac{1}{\omega} \sum_{g=2013}^{2016} \sum_{t=2013}^{2016} \mathbf{1}\{g \leq t\} \widehat{\text{ATT}}(g,t) P(AE = g), \tag{10}$$

whereby $\mathbf{1}\{x\}$ is an indicator variable that equals one if the condition in curly brackets is met and that equals zero otherwise, and $\omega = \sum_{g=2013}^{2016} \sum_{t=2013}^{2016} \mathbf{1}\{g \leq t\} P(AE = g)$ guarantees that the sum of the weights is one.

To consider the heterogeneous effects of AE across firm sizes, we introduce a second summary measure, the *group ATT* of participating in the treatment among employees in group g, across all their post-treatment periods:

$$\hat{\theta}_{\text{group}}(g) = \frac{1}{2016 - g + 1} \sum_{t=g}^{2016} \widehat{\text{ATT}}(g,t). \tag{11}$$

[11] In settings where a policy is rolled out in a staggered design, the standard in applied work has long been to estimate treatment effects using the two-way fixed effects (TWFE) model. However, recent papers show that TWFE models can yield biased coefficient estimates when treatment effects vary across units or time or both (De Chaisemartin and d'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). In Appendix F, we provide evidence that the treatment effects of AE vary across units and time.

For the final summary measure, let $e = t - g$ denote event-time, the elapsed time since treatment occurred. The *event study ATT* is the average effect on outcome variables e periods after AE became mandatory, computed across all employees who have ever been employed in a firm under treatment for exactly e periods:

$$\hat{\theta}_{\text{es}}(e) = \sum_{g=2013}^{2016} \sum_{t=2008}^{2016} \mathbf{1}\{t-g=e\}\widehat{\text{ATT}}(g,t)P(AE=g|t-g=e). \tag{12}$$

The impact treatment effect is $\theta_{\text{es}}(0)$. For all three summary measures, we follow CS and use a multiplier bootstrap procedure to construct simultaneous confidence intervals to account for multiple estimates of the ATTs in $\hat{\theta}_{\text{O}}$, $\hat{\theta}_{\text{es}}(e)$, and $\hat{\theta}_{\text{group}}(g)$.

The CS estimator has a major advantage in our setting, as it requires a weaker identifying assumption than most other difference-in-differences estimators. Other estimators rely on a parallel trends assumption, which states that, in the absence of treatment, the trends in outcome variables would have been identical in both the treatment and control groups. This assumption poses a potential problem in our setting since the rollout of AE was based on firm size, and wages, hours, and pension participation rates might have trended differently in firms of different sizes for reasons other than the introduction of AE. In contrast, the CS estimator relies on a *conditional* parallel trends assumption, which requires that trends in outcome variables of employees with similar covariates would have been the same if AE had not been introduced. The vector of covariates X, measured in the year before treatment, includes binary dummy variables for full-time status (at least 30 hours per week), employee gender, whether pay was set with reference to a collective agreement, and non-profit employer. These control for potentially different pay-setting arrangements with respect to contract type and also discrimination. In addition, X includes dummy variables for the 11 UK regions (e.g., Scotland, London) and one-digit industries using the UK SIC 2007 codes to control for regional and industry-specific shocks and pay-setting practices. We include dummies for two-digit occupations using the UK SOC 2010 codes to control for different seniority levels and skill requirements across jobs. Finally, X includes a cubic polynomial of an employee's age and tenure at their firm, normalized by subtracting the respective average values across employees.

Our conditional parallel trends assumption might be violated if other policies were introduced alongside or after AE that affected outcomes along the firm size distribution systematically differently. Other policies introduced at the same time as automatic enrollment include an increase in the Annual Investment Allowance (AIA) - a tax deduction for capital investment. The changes in AIA applied to all firms, regardless of their size. The UK government also implemented a policy that targeted smaller firms by committing to prioritize SMEs (firms with fewer than 250 employees) in government procurement. According to the UK National Audit Office (National Audit Office, 2016), direct spending on SMEs did not change from 2011 to 2015. Still, indirect spending, accounting for over 60% of all government spending on SMEs, increased notably. However, indirect spending refers to spending on a small number of large firms that subcontract SMEs in their supply chains, whereby UK government departments have to rely on the goodwill of the large firms to report spending accurately as departments usually have no way to verify the accuracy of the figures (National Audit Office, 2016). Finally, EU legislation limiting the ratio of bonuses compared to basic salary in the financial industry came into effect on January 1, 2014. This is likely to decrease extra pay in the financial industry relative to other industries at the same time as the workplace pension reform is introduced, which would violate the conditional parallel trends assumption.[12] We have verified that none of our results change notably when we exclude banks and other financial institutions from the sample and repeat the estimation. Although we cannot rule out with absolute certainty that these policies affected some employees' wages and hours through general equilibrium effects, it seems reasonable that any such effects would be minor compared to the direct effects of the mandatory introduction of AE, on average. As a further check, we analyze employees who were already enrolled in a workplace pension before the AE reform and find no significant evidence that the wages of those employees respond to the policy (Appendix G). We discuss these results in more detail in the next section.

Finally, we require that for every observation in the treatment group, there must be at least some observations in the control group with similar covariates. To check this, we estimate a logistic regression model to predict each employee's probability of being enrolled in a workplace pension, their propensity score, and not being enrolled based on their observed covariate values before the first treatment occurred in 2013. Density plots show no evidence that this assumption is violated, see Appendix Figures C3 and C4.

[12]We thank John Gathergood for insightful discussions on the EU legislation.

6. Results and Discussion

6.1. Pension Participation Rates

We begin by estimating the effect of mandating firms to introduce AE on the pension participation rates of targeted employees, the first-stage analysis. We show that targeted employees in firms that have passed their AE staging date experience a significant increase in the likelihood of being enrolled in a workplace pension compared to targeted employees in other firms. Employees who are automatically enrolled in a workplace pension scheme can choose to opt-out, which means that any observed effect of AE on pension participation rates is a combination of firms automatically enrolling their employees and some employees subsequently opting out of the scheme.[13]

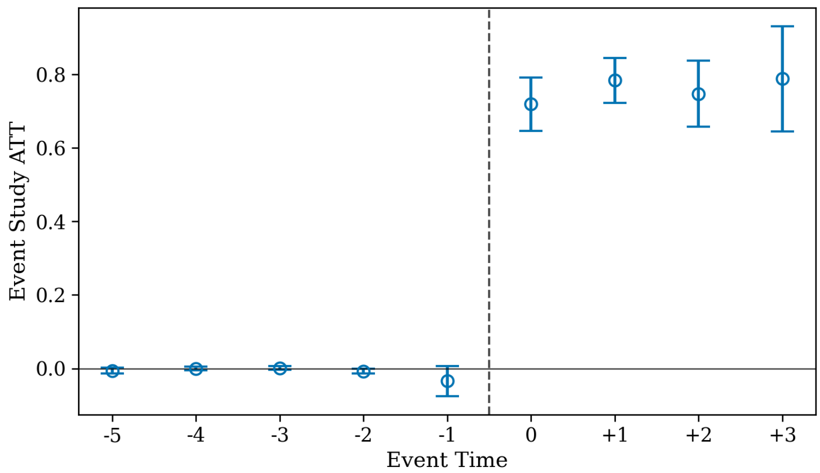

Figure 4: Effect of AE on enrollment rates of targeted employees

Notes: Event-study estimates from (12) for pension enrollment rates. Event Time is defined relative to the staging date in years. The estimates show the relative change in the outcome of targeted employees from the year before their firm's respective staging date, compared to NPtargetedP employees in other firms that are not yet past their staging date. Capped bars indicate the bootstrapped 95% confidence bands.

The results are displayed in Figure 4 and the first column of Table 4. We find a substantial increase in pension participation rates when AE is introduced. The overall effect is 75 percentage points, with pension enrollment rising between 72

[13]Non-compliance with automatic enrollment duties by firms is rare. The UK government introduced a "whistleblower facility" allowing anonymous reporting of non-compliant firms. At the end of our study period in April 2016, approximately 2.4% of companies had received small fines (£400), and less than 0.1% received more significant fines for persistently failing to comply with the pensions regulations. (The Pensions Regulator, 2016).

and 79 percentage points in each post-staging-date year compared to the year just before AE becomes mandatory and relative to targeted employees in firms not yet past their staging date. The immediate impact of AE on pension participation rates of targeted employees is 72 percentage points in the year when it becomes mandatory (event time 0). According to UK population estimates, 58% of eligible employees in the private sector had no workplace pension in 2012, numbering around 8.1 million employees.[14] Our overall ATT estimate suggests that out of those employees, approximately 6.1 million became enrolled in a workplace pension plan due to AE by 2016.

Our first-stage results confirm the earlier findings by Cribb and Emmerson (2020). These authors analyze the same data as in our study but employ a two-way-fixed-effect estimation strategy. They find that automatic enrollment increases workplace pension enrollment by 36 percentage points across all employees between 2012 and 2015. Their estimate is a weighted average of the effects on both employees who were and were not enrolled in a workplace pension plan before the introduction of AE. Based on a back-of-the-envelope calculation, their results imply that enrollment among employees who were not enrolled in April 2012 increases by around 74 percentage points, which closely matches our estimate.[15]

While not directly comparable, our results also show similarities to the evidence from the United States. Studying the voluntary adoption of AE by a large US company, Madrian and Shea (2001) document that enrollment in the workplace pension plan increases substantially among employees that are enrolled automatically, with 86% of employees enrolled in the employer-sponsored 401(k) plan after 3-15 months, compared with only 37% of employees who are not subject to automatic enrollment. In Oregon, the statewide introduction of OregonSaves increases participation rates to between 34% and 62% (Chalmers et al., 2021, 2022). This is lower than the previously discussed effect on employer-sponsored 401(k) plan enrollment rates and the effect documented here. A possible explanation is that OregonSaves does not require employer contributions, thus providing fewer incentives for employees not to opt out.

[14] See Department for Work and Pensions (2019).

[15] Table 4 in Cribb and Emmerson (2020) shows that the share of employees without a workplace pension in 2012 was 48.6%. The coefficient estimate is 36.1%, implying a scaled effect of 74.3%.

Table 4: Effect of AE on pension enrollment and wages of targeted employees: Difference-in-differences estimates

	Enrollment (1)	Log(total compensation) (2)	Log(basic pay) (3)	Log(basic + pension) (4)	Log(take-home pay) (5)
Overall ATT, $\hat{\theta}_O$					
Estimate	0.753*	0.009*	−0.002	0.016*	−0.009*
95% confidence bands	[0.701, 0.806]	[0.001, 0.016]	[-0.008, 0.004]	[0.007, 0.025]	[-0.016, -0.002]
Event study model, $\hat{\theta}_{es}(e)$					
Year 3	0.788*	0.005	0.009	0.024	−0.016
	[0.645, 0.931]	[-0.028, 0.038]	[-0.023, 0.041]	[-0.013, 0.060]	[-0.049, 0.018]
Year 2	0.747*	0.007	−0.002	0.014	−0.012
	[0.657, 0.837]	[-0.009, 0.024]	[-0.018, 0.013]	[-0.009, 0.037]	[-0.027, 0.004]
Year 1	0.784*	0.009	−0.003	0.017*	−0.010
	[0.723, 0.845]	[-0.005, 0.022]	[-0.015, 0.008]	[0.004, 0.030]	[-0.022, 0.002]
Year 0	0.719*	0.010*	−0.001	0.015*	−0.004
	[0.646, 0.792]	[0.002, 0.019]	[-0.008, 0.005]	[0.004, 0.025]	[-0.013, 0.004]
Year -1	−0.034	−0.001	0.002	0.000	0.000
	[-0.075, 0.007]	[-0.007, 0.006]	[-0.003, 0.007]	[-0.006, 0.006]	[-0.006, 0.006]
Year -2	−0.007*	0.002	0.005	0.002	0.002
	[-0.014, -0.001]	[-0.007, 0.010]	[-0.001, 0.009]	[-0.004, 0.008]	[-0.006, 0.010]
Year -3	0.002	0.003	0.005	0.008*	0.003
	[-0.003, 0.006]	[-0.003, 0.010]	[-0.001, 0.009]	[0.001, 0.015]	[-0.002, 0.009]
Year -4	0.000	0.005	0.005	0.003	0.005
	[-0.005, 0.005]	[-0.003, 0.014]	[-0.002, 0.011]	[-0.006, 0.012]	[-0.003, 0.012]
Year -5	−0.006	0.001	−0.002	−0.002	0.001
	[-0.014, 0.003]	[-0.013, 0.014]	[-0.012, 0.009]	[-0.015, 0.010]	[-0.013, 0.014]
N Observations (jobs × years)	169,355	169,355	169,355	169,355	169,355

Notes: Overall average treatment effect estimates from equation (10), $\hat{\theta}_{\text{O}}$, show the change in the outcome variable of targeted employees from the year before their firm's respective staging date, compared to targeted employees in other firms that are not yet past their staging date. Event-study estimates from equation (12), $\hat{\theta}_{\text{es}}(e)$, show the estimated relative change in the outcome variables of targeted employees from the year immediately before their firm's respective staging date to the four years before and after the staging date, compared to targeted employees in other firms that are not yet past their staging date. See Table 1 for the description of each outcome variable.
Bootstrapped 95% confidence bands are shown in brackets (999 samples). We allow for clustering at the firm level. * indicates significance at the 5% level.

6.2. Wages

In this section, we examine how AE affects the wages of targeted employees. To do this, we repeat the previous estimations but separately for four different measures of wages. First, we examine changes in total compensation, which comprises an employee's basic pay, their employer's workplace pension contribution, and extra pay. Next, we analyze the responses of each of the three components of total compensation. We include targeted employees enrolled in a workplace pension post-AE introduction and those not enrolled but whose employer has passed its staging date. The results are displayed in Figure 5 and columns two to five of Table 4.

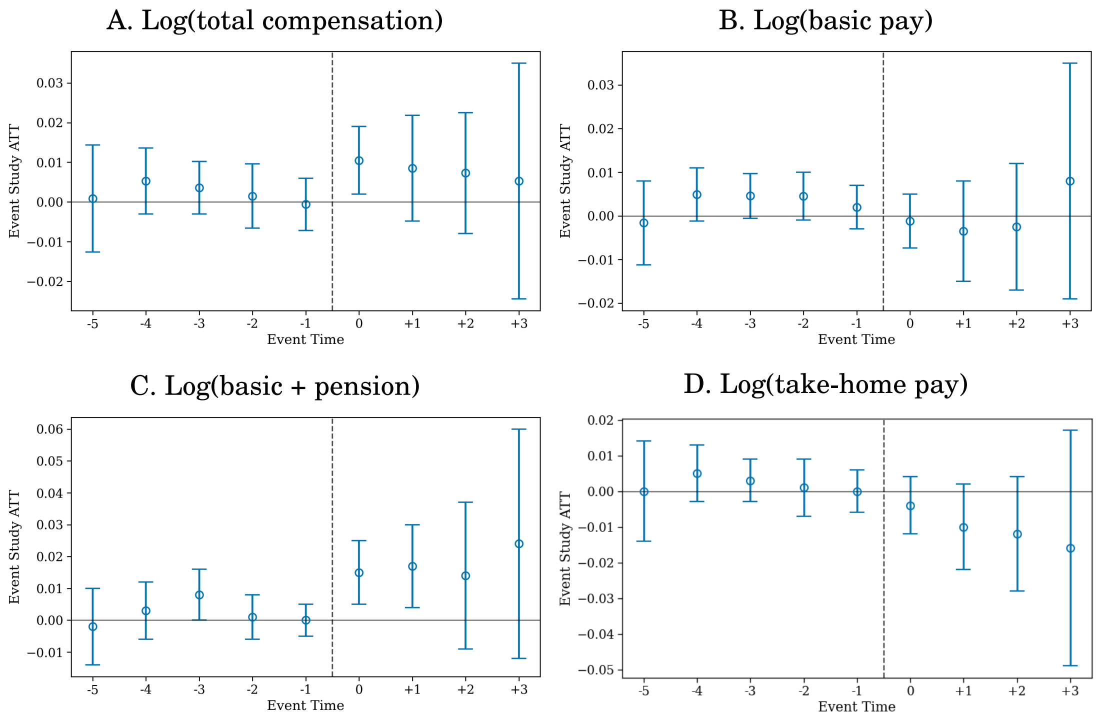

Figure 5: Effect of AE on different pay components of targeted employees

Notes: Event-study estimates from (12). Note that scales differ across the panels. See Figure 4 for more details and Table 1 for the descriptions of each outcome variable.

Before the mandatory introduction of AE, there were no notable differences in wage trends across the firm size groups; pre-staging date event-study estimates are typically close to zero and statistically insignificant for our four pay measures. Moving to the top left panel, we see that log total compensation increases significantly among targeted employees in firms post-staging date compared to targeted employees in other firms. In the year when AE is introduced, total compensation increases relatively by 1.0% among targeted employees in post-staging date firms (column (2) of Table 4). In the following years, the coefficients are of a similar magnitude but no longer statistically significant. The overall effect of AE on the total compensation of an average targeted employee is 0.9%.

We now focus on the first component of total compensation, basic pay, which constitutes the vast majority of labor income in the UK (Schaefer and Singleton, 2023). Panel B of Figure 5 displays the estimates for the change in log basic pay between the year immediately before the staging date and the years after AE became mandatory. We find no significant evidence that the basic pay of targeted employees is affected by automatic enrollment. Neither the overall ATT estimate nor the event-study estimates are statistically significant. We can reject that targeted employees' basic pay declines by more than 0.8% or increases by more than 0.4% in response to the introduction of AE.

It is not possible to measure the response of employer contributions as a percentage of their pre-AE value since these equal zero by construction. Instead, we estimate the response of the sum of employer contributions and the employee's basic pay. The log of this sum significantly increases among targeted employees after the introduction of AE compared to targeted employees in other firms, with an overall ATT estimate of 1.6%. The immediate effect of AE in the year of its introduction raises the sum of firms' pension contributions and basic pay by 1.5%, which further rises to 1.7% in the following year. Both estimates are significantly different from zero. The positive effect of AE on firms' pension contributions appears to persist in years 2 and 3 but is too imprecisely estimated to be statistically significant. The overall estimate is 1.6%, with the confidence bands covering the minimum mandated contribution level of 1%.

Our theoretical model suggests that firms may respond to the introduction of AE by reducing the extra pay of targeted employees. To check this prediction, we combine basic pay with extra pay and call this sum *take-home pay*.[16] We find that the AE reform decreases log take-home pay of targeted employees by 0.9% compared to not-yet-treated targeted employees. We can reject that take-home pay remains unchanged and may fall by as much as 1.6% due to the reform. In the next section, we investigate the channels through which AE impacts employees' take-home pay.

[16] Already before the reform, employees frequently transitioned between years from receiving some extra pay and not receiving any extra pay. This is why we cannot directly assess the effect of the reform on extra pay.

The results presented in this section suggest that AE significantly increases the total compensation of employees who did not have a workplace pension before its implementation. The average increase in total compensation resulted from two opposing effects: a substantial decline in take-home pay, likely driven by extra pay, and an increase in firms' pension contributions sufficient to offset this decline. The *pass-through* of the reform to employees can be measured by how much of the employer pension contributions end up at the employees. We compute this as the total compensation coefficient divided by the total compensation coefficient net of the take-home pay coefficient (Saez et al., 2019). We find that the overall pass-through is 50%, meaning that half of the additional contributions due to the introduction of AE increase the employees' total compensation. The employers recoup the other half of the additional pension contributions by reducing take-home pay. The 95% confidence bands of pass-through are 19% and 81%, meaning that we can reject both complete pass-through to employees and full cost-shifting by employers; both employees and employers carry some of the burden of mandatory workplace pension enrollment. To give an idea of the scale of the policy, we use the average compensation amounts in 2012 (as shown in Table 2). An approximate 1.6% increase in basic pay plus pension contributions for 8.1 million targeted employees would amount to additional pension contributions of £2.9 billion per year. However, this would be offset by a decrease in take-home pay of £1.45 billion per year.

None of our findings change notably when we analyze wage rates per basic hour instead of weekly pay. In Appendix D, we analyze the response of basic hours worked per week and find no significant response to the introduction of AE. Assuming a 40-hour work week, we can reject changes exceeding 15 minutes. As an additional placebo falsification check, we analyze the wages of employees already enrolled in a workplace pension before the reform (Appendix G). Consistent with the predictions of our model, we find no significant response of these employees' wages to the introduction of AE, with point estimates close to zero and confidence bands mostly within a range of less than one percent around zero across different specifications.

6.3. Heterogeneous Effects Across Treatment Groups

We now look at the effects of AE on targeted employees' pension enrollment and wages within each treatment group g. To do this, we use the estimated average treatment effects on targeted employees, $\widehat{\text{ATT}}(g,t)$, but instead of analyzing all treatment groups, we focus on a particular treatment group g and examine the effects of the introduction of AE over time t. In addition, we use equation (11) to calculate the average effect of AE among targeted employees in group g across all their post-treatment periods. Again, we begin by analyzing relative changes in workplace pension participation among targeted employees in firms that have passed their staging date compared to those in other firms to show the first-stage impact at the group level. Table 5 shows the estimated group ATTs for targeted employees employed in firms with staging dates in 2013, 2014, 2015, and 2016 (Appendix Figures C6-C10 display the event-study estimates by treatment group).

Workplace pension participation significantly increases after the introduction of AE across all treatment groups in all post-AE years. The estimate for the treatment group 2013 (6,000+ employees) is 72 percentage points (column one of Table 5). The group ATT of the treatment group 2014 (160-5,999 employees) is the highest among all groups at 79 percentage points. The smallest effect of AE introduction is among employees in the treatment group 2016 (fewer than 50 employees), with the lowest workplace pension enrollment, at 29% of employees before AE.

Table 5: Effect of AE on pension enrollment and wages across treatment groups: Difference-in-differences estimates

	Enrollment (1)	Log(total compensation) (2)	Log(basic pay) (3)	Log(basic + pension) (4)	Log(take-home pay) (5)	Pass-through (6)
Group ATT, $\hat{\theta}_{group}$						
Group 2013	0.719^*	0.002	0.000	0.016	-0.017^*	0.095
	[0.553, 0.885]	[-0.015, 0.019]	[-0.013, 0.013]	[-0.005, 0.038]	[-0.032, -0.001]	[-0.278, 0.468]
Group 2014	0.789^*	0.011^*	-0.005	0.016^*	-0.007	0.605^*
	[0.774, 0.803]	[0.001, 0.021]	[-0.015, 0.004]	[0.006, 0.025]	[-0.018, 0.003]	[0.214, 0.996]
Group 2015	0.760^*	0.022^*	0.007	0.017^*	0.010	1.870^*
	[0.741, 0.780]	[0.011, 0.034]	[-0.002, 0.016]	[0.006, 0.029]	[-0.001, 0.022]	[1.154, 2.585]
Group 2016	0.587^*	0.009	0.001	0.012	0.000	1.007
	[0.551, 0.623]	[-0.006, 0.023]	[-0.012, 0.015]	[-0.002, 0.025]	[-0.014, 0.015]	[-3.510, 2.366]
N Observations						
(jobs × years)	169,355	169,355	169,355	169,355	169,355	169,355

Notes: Group treatment effect estimates from equation (11), which show the relative change in the outcome variable of targeted employees in group g from the year immediately before that group's respective staging date, compared to targeted employees in groups that are not yet past their staging date. See Table 1 for the description of each outcome variable. Pass-through is computed as the change in log total compensation over the change in log total compensation net of the change in log take-home pay.
Bootstrapped 95% confidence bands are shown in brackets (999 samples). We allow for clustering at the firm level. * indicates significance at the 5% level.

As column (2) of Table 5 shows, the effect on log total compensation in the treatment group 2013 is not statistically significant. The coefficient estimate is close to zero, but the confidence bands do not allow us to reject the possibility of coefficients between -1.5% and 1.9%. Contrary to this, the treatment groups 2014 and 2015 (50-5,999 employees) show significant positive effects of AE. Total compensation increases by 1.1% and 2.2% post-AE introduction compared to not-yet-treated firms. The estimated group ATT for treatment group 2016 is imprecisely estimated and not significant.

We also estimate the effect of AE introduction on log basic pay for each treatment group separately (column (3) of Table 5). The results reveal no meaningful heterogeneity compared to the aggregate event-study findings in the previous section: All event-study estimates for log basic pay are insignificant in pre- and post-AE introduction years, but confidence bands are comparatively large. For log basic pay plus pension contributions, we find significantly positive group ATTs for treatment groups 2014 and 2015 at 1.6% and 1.7%, respectively. The estimate for the treatment group 2016 has a similar magnitude (1.2%), but it is not statistically significant due to relatively wide confidence bands. For log take-home pay, the estimated group ATT 2013 is significantly negative at -1.7%. Among targeted employees in other treatment groups, we find no significant estimates, whereby the confidence bands for groups 2015 and 2016 do not include the coefficient of treatment group 2013. This suggests that the decrease in the overall ATT for take-home pay discussed in the previous section is primarily driven by substantially declining take-home in firms with 30,000 or more employees. In the Appendix, Table B5 presents some evidence that the relative decline in take-home pay is due to lower nominal pay growth among treated employees. In comparison, not-yet-treated employees experience greater nominal pay growth. This may explain why targeted employees' take-home pay declines gradually over time after the introduction of AE rather than dropping sharply immediately after the staging date.

In the last column of Table 5, we present results for the pass-through of employer contributions to employees within each treatment group. Full pass-through (suggesting that total compensation increased by the same amount as employer contributions) would correspond to a coefficient of one. If, instead, firms fully recoup the mandated employer contributions by reducing take-home pay by the same amount, then our estimate of pass-through would be zero. The pass-through in the largest firms is only 9.5%, and we can rule out that more than 47% of the increase in employer contributions benefits employees. We cannot reject the possibility that the largest firms fully recoup the mandated employer contributions. In the treatment group 2014, we estimate a pass-through of 60.5%, and we can only marginally reject a full pass-through to employees. For the treatment group 2015, we can rule out that employer contributions are offset by take-home pay, implying that targeted employees in firms with 50-159 employees benefit most from the AE reform in terms of increased total compensation. Importantly, we can reject the hypotheses that the pass-through in the treatment groups 2013, 2014, and 2015 are equal. The pass-through to employees of the automatic enrollment reform depends on firm size, with larger firms showing a significantly lower pass-through than smaller firms.

Considering the results in this section, we uncover significant heterogeneity in the response to the introduction of AE across treatment groups. The most significant effect of the AE introduction is on pension enrollment rates among firms with 50 or more employees, corresponding to firms with the highest share of employees in workplace pensions before the reform. Conversely, the smallest AE effect is observed in firms with 49 or fewer employees, which is the firm size group with the lowest pre-AE pension participation rates. Although pension enrollment increases across all groups, we find no effect on total compensation in very large firms. This outcome can be explained by the substantial decrease in take-home pay in these firms. Our findings are consistent with significant fixed adjustment costs of the compensation package and relatively strong worker sorting.

6.4. Examining the Decline in Take-Home Pay

To better understand the mechanisms that lead to the decline in take-home pay, we first consider the effect of AE among targeted employees who received a positive amount of extra pay in 2012 before the introduction of AE, which is the case for around 45% of the targeted employees in our sample.[17] Table 6, column (1), displays the ATT estimates for these targeted employees. The results confirm our findings in the main text: the overall coefficient for take-home pay is significantly negative at -2.2%, with treatment group 2013 showing the largest decline. While the confidence bands for group 2016 are relatively wide, for group 2015 we can reject the hypothesis of a decrease in take-home pay of more than

[17]The smaller sample size leads to some covariate cells being empty. Therefore, we use a smaller set of covariates: dummy variables for full-time status and sex, a cubic polynomial in age and tenure, and one-digit industry and occupation code indicator variables.

1.1%, which is only a third of the estimate for group 2013. Column (2) displays our estimates of the effect of AE on the likelihood of receiving any extra pay. We find an overall decline of 2.9 percentage points, with 4.1 percentage points in treatment group 2013 and 2.8 percentage points in treatment group 2014, the latter being statistically significant.

Table 6: Effect of AE on extra pay components of targeted employees

	Log(take-home pay) extra pay > 0 in 2012 (1)	Likelihood of receiving extra pay (2)	Log(incentive pay) (3)	Log(shift pay) (4)	Log(other pay) (5)	Log(overtime pay) (6)
Overall ATT, $\hat{\theta}_O$						
Coefficient estimate	-0.022^{*}	-0.029^{*}	-0.040	-0.083	-0.095^{*}	-0.091^{*}
	[-0.033, -0.010]	[-0.051, -0.007]	[-0.120,0.039]	[-0.218, -0.051]	[-0.176, -0.013]	[-0.155, -0.027]
Group ATT, $\hat{\theta}_{group}$						
Group 2013	-0.033^{*}	-0.041	-0.097	-0.030	-0.105	-0.131
	[-0.056, -0.010]	[-0.104, 0.022]	[-0.356, 0.072]	[-0.299, 0.238]	[-0.297, 0.087]	[-0.286, 0.024]
Group 2014	-0.012	-0.028^{*}	-0.003	-0.120	-0.115	-0.111^{*}
	[-0.025, 0.002]	[-0.048, -0.008]	[-0.119, 0.105]	[-0.316, 0.077]	[-0.244, 0.014]	[-0.217, -0.005]
Group 2015	0.009	0.002	0.051	0.013	0.058	0.081
	[-0.011, 0.030]	[-0.025, 0.030]	[-0.086, 0.190]	[-0.283, 0.309]	[-0.196, 0.314]	[-0.038, 0.201]
Group 2016	-0.011	-0.015	-0.013	–	0.027	-0.021
	[-0.040, 0.018]	[-0.054, 0.024]	[-0.244, 0.218]	[–, –]	[-0.348, 0.402]	[-0.180, 0.222]
N Observations (jobs × years)	69,384	169,355	11,602	9,550	11,924	31,304

Notes: Incentive pay includes bonus or incentive pay received for work carried out in the pay period; Shift pay gives premium payments for shift, night, and weekend work; Other pay is pay received for other reasons, e.g., meal allowances; Overtime pay refers to paid overtime work. See Tables 4 & 5 for more details and Table 1 for the descriptions of each outcome variable.

Next, columns (3)-(6) of Table 6 show the results when we split extra pay into its components - incentive pay, shift pay, other pay, and overtime pay - and then repeat our estimation separately for each component. We include only targeted employees in this analysis who received a positive amount of the relevant extra pay component in 2012 before the introduction of AE. Requiring a positive amount of the relevant extra pay component before the staging date decreases the sample sizes notably, increasing the standard error estimates and even leaving cells in the treatment group 2016 with too few observations to obtain estimates. This explains the relatively wide confidence bands that prevent us from making any statement about the effects of AE on incentive pay and shift pay. Nevertheless, column (5) shows a significantly negative coefficient of log other pay of 9.5%. Several different payments are aggregated into other pay, for example, car allowances paid through the payroll, on-call and standby allowances, clothing, first-aider or firefighter allowances, and meal allowances. The point estimates suggest that the 2013 and 2014 treatment groups may drive the decline in other pay, but no group coefficient is statistically significant. In addition, log overtime pay decreases by 9.1%, and the coefficient of overtime pay in the treatment group 2014 is statistically significantly negative at -11.1%. Note that only *paid* overtime is recorded by the ASHE questionnaire.

The ASHE data provide information on overtime hours, allowing us to further investigate the relative decline in overtime pay. The results in Table 7 show that overtime pay per hour (overtime rate) is mainly unaffected by the pension reform; we can rule out that the overtime rate declines by more than 1.1%. However, we find that the likelihood of overtime decreases significantly by 1.6%. The results for individual treatment groups are too noisy to be conclusive. Still, again, the point estimates suggest a larger decline in the likelihood of receiving overtime pay among larger firms.

Our theoretical framework suggests that AE may distort the previously agreed-upon compensation package between the employee and the firm, and the extra pay adjustments aim to prevent larger profit losses. Our findings regarding the heterogeneous responses of firms of different sizes to the AE mandate are consistent with two explanations: First, large firms may be more likely to optimize their compensation packages. This would align well with survey results concerning the adoption of AE in firms of different sizes: to comply with the AE mandate, larger firms with 250 or more employees set up customized workplace pension schemes with the help of external consultants, while smaller firms tended

Table 7: Effect of AE on overtime of targeted employees

	Log(overtime rate) overtime > 0 in 2012 (1)	Likelihood of receiving overtime pay (2)
Overall ATT, $\hat{\theta}_O$		
Coefficient estimate	-0.002	-0.016^{*}
	[-0.011, 0.009]	[-0.030, -0.001]
Group ATT, $\hat{\theta}_{group}$		
Group 2013	-0.003	-0.021
	[-0.020, 0.015]	[-0.062, 0.021]
Group 2014	0.000	-0.016
	[-0.008, 0.008]	[-0.033, 0.001]
Group 2015	0.005	0.001
	[-0.002, 0.012]	[-0.023, 0.024]
Group 2016	0.007	-0.009
	[-0.004, 0.018]	[-0.041, 0.023]
N Observations (jobs × years)	36,304	169,355

Notes: Overtime rate gives the sum of overtime pay and basic per total hour worked. See Tables 4 & 5 for more details and Table 1 for the descriptions of each outcome variable.

to use the UK National Employment Savings Trust (NEST), a not-for-profit provider of a standardized workplace pensions scheme (Department for Work and Pensions, 2018). Second, our results are consistent with a labor market in which workers may sort into firms according to the valuation of benefits. Suppose targeted employees in large firms have higher benefits valuations on average than those in small firms. In that case, the income effect of the employer contributions is relatively strong in the former group. This means that large firms optimally reduce extra pay by a larger amount by lowering other pay and canceling paid overtime hours.

7. Conclusion

Despite the growing popularity among policymakers of automatic enrollment in workplace pensions, there is currently a lack of empirical evidence on the effects of this policy on employee wages. To address this issue, we analyze the most significant change to the UK pension system in recent history, affecting every third employee. While introducing automatic enrollment with employer contributions leads to a significant rise in workplace pension enrollment, it also affects wages. Specifically, we estimate that pension enrollment rates increase by 75 percentage points among previously not enrolled employees, and this effect persists for up to four years. However, this increase in enrollment is accompanied by a decline of over 0.9% in take-home pay, partially offsetting any gains in compensation resulting from the additional employer contributions. We present evidence that reductions in overtime hours and allowances can account for some of the decline in take-home pay. The aggregate pass-through of employer contributions to employees' total compensation is around 50%; both employees and employers carry the burden of mandatory workplace pension enrollment. Our findings suggest that the cost of an approximate annual increase in employer pension contributions of £2.9 billion is partially offset by a £1.45 billion loss in take-home pay. Larger firms rather than smaller firms drive this reduction in take-home pay.

Previous studies have not addressed who bears the costs of AE, which is essential for evaluating the overall impact of this policy. On the one hand, employees in smaller firms gain access to workplace pensions without any significant reduction in other pay components, resulting in higher total compensation. On the other hand, employees in larger firms receive a higher share of the same total compensation in the form of pension contributions rather than take-home pay. This demonstrates that the incidence of mandated benefits is not necessarily equal across employers, and some employees benefit more than others. We argue that this is consistent with large firms optimizing their pay structures more than small firms or employees in large firms having higher valuations of employer-provided benefits on average or both. In either scenario, only a paternalistic motive for increasing pension savings could justify automatic enrollment as a welfare-improving reform for these employees. On the other hand, if employees in large firms are liquidity-constrained and optimally smooth their consumption by planning to save more later, AE would only distort their choice set, leaving the workers worse off. More research is needed to understand which of the two cases is more likely to apply.

Our study has important implications for other countries with a pension system similar to the UK, such as the United States, which relies heavily on private pension savings and is scheduled to introduce automatic enrollment in 2025. Future research should explore how wage responses to automatic enrollment may vary across economies. Such research would be crucial for policymakers who want to understand the potential impacts of automatic enrollment policies and ensure that these policies do not have unintended consequences for employees' financial well-being.

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The Incidence of Workplace Pensions: Evidence from the UK's Automatic Enrollment Mandate

Rachel Scarfe Daniel Schaefer Tomasz Sulka[†]

Online Appendix

Appendix A. Further Details of the Data

Sample selection. From the sample of targeted employees described in the main text, we only keep employee-year observations without loss of pay in the April reference period (e.g., unpaid sick leave), and that are not paid at an apprenticeship or a trainee rate. This is to be able to compare like-for-like measures of pay across firms. Additionally, we impose a minimum firm size requirement in 2012 of at least five employees to exclude sole proprietors and small family employers because the incentives to provide workplace pensions and adjust wages likely differ from incorporated firms. We drop employee-year observations with likely erroneous information, i.e., if an employee is reported as working on average less than one or more than 100 hours during the reference week in April or is reported as being paid less than 80% of the age-relevant statutory National Minimum Wage.

Definitions. The key earnings variables that we analyze are the answers to the following questions in the ASHE questionnaire, whereby monetary values are measured in Pound sterling (GBP), including pence:

> **Basic pay** (BPAY):
> *"How much basic pay, before deductions, did the employee receive in the pay period? Include: all basic pay, relating to the pay period, before deductions for PAYE, National Insurance, pension schemes, student loan repayments and voluntary deductions. Include paid leave (holiday pay), maternity/paternity pay, sick pay and area allowances (e.g., London).*
> *Exclude: pay for a different pay period, shift premium pay, bonus or incentive pay, overtime pay, expenses and the value of salary sacrifice schemes and benefits in kind."*
>
> **Overtime pay** (OVPAY):
> *"How much overtime pay did the employee receive for work carried out in the pay*

[†]Schaefer: daniel.schaefer@jku.at; corresponding author. This work is based on the Annual Survey of Hours and Earnings Dataset (Crown copyright 2020), having been funded, collected, and deposited by the Office for National Statistics (ONS) under secure access conditions with the Research Accreditation Service (SN:6689). Neither the ONS nor the Research Accreditation Service bear any responsibility for the analysis and discussion of the results in this paper.

period?
Exclude: any basic, shift premium and bonus or incentive pay in this period, as well as overtime pay from the previous pay period."

Shift premium pay (SPPAY):
"How much shift premium pay did the employee receive in the pay period?
Include: the element of shift premium pay. For example, for a 35 hour pay period, if the basic rate is £10 per hour and the premium rate is £12 per hour, multiply the difference of £2 by the hours worked (i.e. 35 multiplied by 2). The shift premium pay reported would therefore be £70.
Exclude: any basic, overtime and bonus or incentive pay."

Incentive pay (IPAYIN):
"How much [bonus or incentive payments did the employee receive,] related to work carried out in the pay period?
For example, if [an annual bonus was paid], the value should be divided by 12 if the employee was paid on a calendar month basis.
Include: profit sharing, productivity, performance and other bonus or incentive pay, piecework and commission.
Exclude: basic, overtime and shift premium pay."

Other pay (OTHPAY):
"How much pay did the employee receive for other reasons in the pay period?
Include: for example, car allowances paid through the payroll, on call and standby allowances, clothing, first aider or fire fighter allowances.
Exclude: paid leave (holiday pay), basic, overtime, shift premium, maternity/paternity, sick, bonus or incentive pay, redundancy, arrears of pay, tax credits, profit share and expenses."

Employer contribution (COMPAY):
"How much did the employer contribute to the employee's pension?
Exclude: any lump sum contributions that cover more than one employee and exclude any employee contributions made through salary sacrifice."

Basic hours worked (BHR):
"How many basic hours does [basic pay] relate to?
If your pay period is calendar month and hours are weekly, multiply the weekly hours by 4.348 to get calendar month hours. If the employee uses a decimal clock, please convert to hours and minutes. For example, 4.3 hours should be 4 hours and (0.3 multiplied by 60) minutes = 4 hours 18 minutes.
Include: any hours paid at shift premium and paid hours even if not worked.
Exclude: any hours paid as overtime."

Overtime hours worked (OVHR):

"How many overtime hours does [overtime pay] relate to?

If the employee uses a decimal clock, please convert to hours and minutes. For example, 4.3 hours should be 4 hours and (0.3 multiplied by 60) minutes = 4 hours 18 minutes.

Include: the actual number of hours. For example, for 4 hours paid at time and a half, enter 4 not 6. Include any paid meal breaks taken during a period of overtime.

Exclude: any hours paid at the basic or shift premium rate."

Appendix B. Additional Tables

Table B1: Staging dates of automatic enrollment duties based on firm size

Number of employees in April 2012	Staging date	Treatment observed
120,000 or more	October 1, 2012	April 2013
50,000-119,999	November 1, 2012	April 2013
30,000-49,999	January 1, 2013	April 2013
20,000-29,999	February 1, 2013	Partial 2013
10,000-19,999	March 1, 2013	Partial 2013
6,000-9,999	April 1, 2013	Partial 2013
4,100-5,999	May 1, 2013	April 2014
4,000-4,099	June 1, 2013	April 2014
3,000-3,999	July 1, 2013	April 2014
2,000-2,999	August 1, 2013	April 2014
1,250-1,999	September 1, 2013	April 2014
800-1,249	October 1, 2013	April 2014
500-799	November 1, 2013	April 2014
350-499	January 1, 2014	April 2014
250-349	February 1, 2014	Partial 2014
160-249	April 1, 2014	Partial 2014
90-159	May 1, 2014	April 2015
62-89	July 1, 2014	April 2015
61	August 1, 2014	April 2015
60	October 1, 2014	April 2015
59	November 1, 2014	April 2015
58	January 1, 2015	April 2015
54-57	March 1, 2015	Partial 2015
50-53	April 1, 2015	Partial 2015
40-49	August 1, 2015	April 2016
30-39	October 1, 2015	April 2016
Fewer than 30	June 1, 2015 to April 1, 2017	Partial 2016
New employer	May 1, 2017 to February 1, 2018	Partial 2018

Notes: The staging dates for firms with fewer than 30 employees in April 2012 are shown in Appendix Table B2.

Table B2: Staging dates of automatic enrollment duties based on the PAYE number for firms that had fewer than 30 employees in April 2012

Last two digits of PAYE tax number	Staging date	Treatment observed
92, A1-A9, B1-B9, AA-AZ, BA-BW, M1-M9, MA-MZ, Z1-Z9, ZA-ZZ, 0A-0Z, 1A-1Z, 2A-2Z	June 1, 2015	April 2016
BX	July 1, 2015	April 2016
BY	September 1, 2015	April 2016
BZ	November 1, 2015	April 2016
02-04, C1-C9, D1-D9, CA-CZ, DA-DZ	January 1, 2016	April 2016
00, 05-07, E1-E9, EA-EZ	February 1, 2016	Partial 2016
01, 08-11, F1-F9, G1-G9, FA-FZ, GA-GZ	March 1, 2016	Partial 2016
12-16, 3A-3Z, H1-H9, HA-HZ	April 1, 2016	Partial 2016
I1-I9, IA-IZ	May 1, 2016	April 2017
17-22, 4A-4Z, J1-J9, JA-JZ	June 1, 2016	April 2017
23-29, 5A-5Z, K1-K9, KA-KZ	July 1, 2016	April 2017
30-37, 6A-6Z, L1-L9, LA-LZ	August 1, 2016	April 2017
N1-N9, NA-NZ	September 1, 2016	April 2017
38-46, 7A-7Z, O1-O9, OA-OZ	October 1, 2016	April 2017
47-57, 8A-8Z, Q1-Q9, R1-R9, S1-S9, T1-T9, QA-QZ, RA-RZ, SA-SZ, TA-TZ	November 1, 2016	April 2017
58-69, 9A-9Z, U1-U9, V1-V9, W1-W9, UA-UZ, VA-VZ, WA-WZ	January 1, 2017	April 2017
70-83, X1-X9, Y1-Y9, XA-XZ, YA-YZ	February 1, 2017	Partial 2017
P1-P9, PA-PZ	March 1, 2017	Partial 2017
84-91, 93-99	April 1, 2017	Partial 2017

Table B3: Qualifying earnings band

Year	Lower limit (£)	Upper limit (£)
2013	5,564	42,473
2014	5,720	41,450
2015	5,772	41,865
2016	5,824	42,385
2017	5,824	43,000
2018	5,876	45,000
2019	6,032	46,350
2020	6,136	50,000
2021	6,240	50,000

Notes: The following wage components are included in qualifying earnings: basic wages, extra pay, statutory sick pay, statutory maternity/paternity pay, and statutory adoption pay.

Table B4: Descriptive statistics for subsamples, as measured in April 2012, private sector

	Date when AE became mandatory				
	2013	2014	2015	2016	Not treated
	(1)	(2)	(3)	(4)	(5)
Firm size band (employees)	6,000+	160-5,999	50-159	5-49	5-30
A. Pension before AE					
Share full-time contract (%)	89.2	91.5	91.6	83.6	79.8
Share permanent contract (%)	95.8	97.3	98.9	98.7	98.7
Share collective agreement (%)	23.2	13.6	4.7	5.9	4.9
Share men (%)	61.0	61.7	61.7	53.6	51.0
Age (years)	42.5	43.4	43.5	44.3	44.7
Basic pay (weekly, £)	673.3	716.3	695.6	635.2	614.8
Extra pay (weekly, £)	75.8	56.6	47.4	36.4	32.7
Ratio extra pay to basic pay (%)	11.3	7.8	6.8	5.7	5.3
Pension contributions (weekly, £)	121.1	116.9	86.5	74.4	89.0
Total compensation (weekly, £)	874.6	889.8	831.5	747.0	739.5
Share with positive extra pay (%)	55.4	40.8	34.2	31.6	30.0
Extra pay, if positive (weekly, £)	134.7	136.1	136.1	113.2	106.4
N (Employees)	9,381	15,182	3,228	1,616	1,635
B. No pension before and after AE					
Share full-time contract (%)	77.9	86.6	88.7	87.5	85.4
Share permanent contract (%)	87.9	91.1	94.9	96.7	97.5
Share collective agreement (%)	6.4	5.7	3.0	1.8	1.7
Share men (%)	53.2	57.1	57.2	59.2	60.5
Age (years)	38.5	38.8	40.1	41.6	41.3
Basic pay (weekly, £)	385.7	453.6	462.1	474.8	473.1
Extra pay (weekly, £)	36.3	43.0	41.0	36.0	28.7
Ratio extra pay to basic pay (%)	9.9	9.7	8.9	7.6	6.1
Total compensation (weekly, £)	421.5	499.0	509.0	510.5	500.7
Share with positive extra pay (%)	53.3	41.8	39.6	32.9	27.4
Extra pay, if positive (weekly, £)	67.6	102.4	103.1	108.9	104.4
N (Employees)	3,403	5,847	2,960	3,045	5,900

Notes: All values are for the year 2012. Pension contributions include employee and employer contributions to a workplace pension plan. See notes in Table 2.

Table B5: Average change in log(take-home pay) of targeted employees

	Date when AE became mandatory				
	2013	2014	2015	2016	Not treated
Period					
2012 - 2013 (in %)	2.8	2.9	2.8	2.8	2.7
2012 - 2014 (in %)	6.5	6.5	6.7	7.2	6.5
2012 - 2015 (in %)	10.2	10.7	10.7	10.1	9.6
2012 - 2016 (in %)	14.1	13.5	14.4	13.5	12.8
N	21,217	27,601	11,081	9,140	14,466

Notes: Simple averages computed across all targeted employees. Unlike in the main text, we do not control for observable employee and firm characteristics here.

Appendix C. Additional Figures

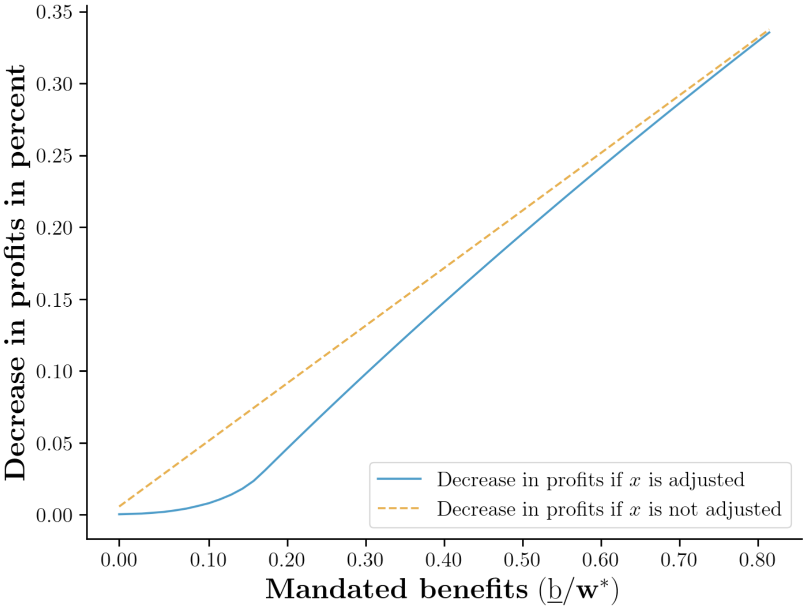

Figure C1: Decrease in profits due to mandated benefits

Notes: This figure displays numerical simulations of the decline in profits (in % of profits with $\underline{\mathrm{b}} = 0$) depending on whether the firm adjusts x in response to the mandated benefits. The difference between the two functions gives $\Delta\Pi$ (expressed in % of pre-AE profits). See Figure 1 for more details on the functional forms and parameterization.

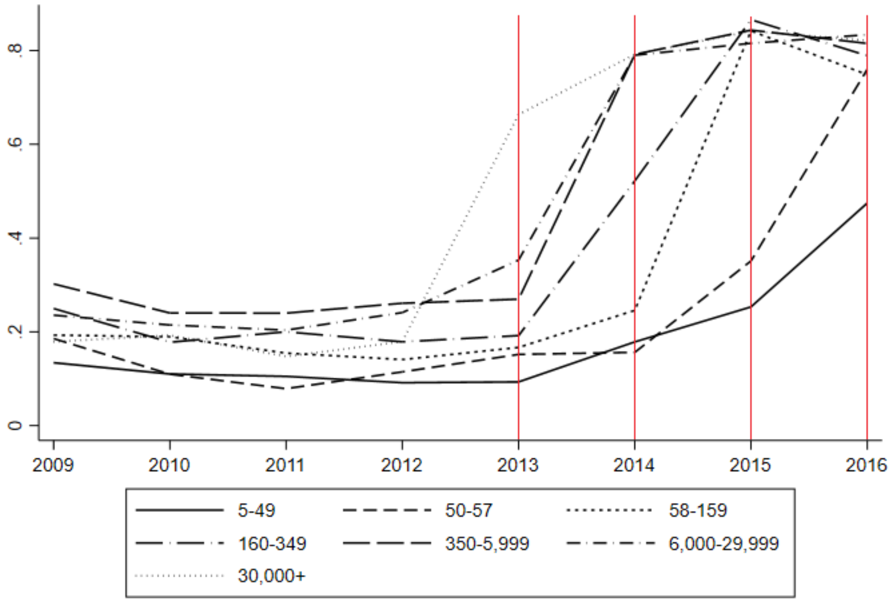

Figure C2: Pensions participation rates in the private sector, new hires

Notes: New hires have been employed at the firm for less than 12 months. See Figure 3 for additional notes.

Figure C2 displays new hires' average pension participation rates in different firm size bands. We define an employee as participating in a pension if we see a positive value for the employee's or firm's contribution to a workplace pension in a given year. We compute this variable separately for each year and firm size band by first summing all employees who participate in a pension in the Annual Survey of Hours and Earnings (ASHE) and then dividing that number by the total number of observations. Looking at the data for 2012, before the implementation of AE, we observe that pension participation rates are around 20% across all firm size bands, with slightly higher rates in larger firms. At the staging date for each firm size band, we see a sharp increase in pension participation rates. For instance, firms with 350-5,999 employees in 2012 were required to introduce AE by April 2014 (see Appendix Table B1). The data reveal a jump in pension participation rates for this group from 20% in 2013 to over 80% in 2014.

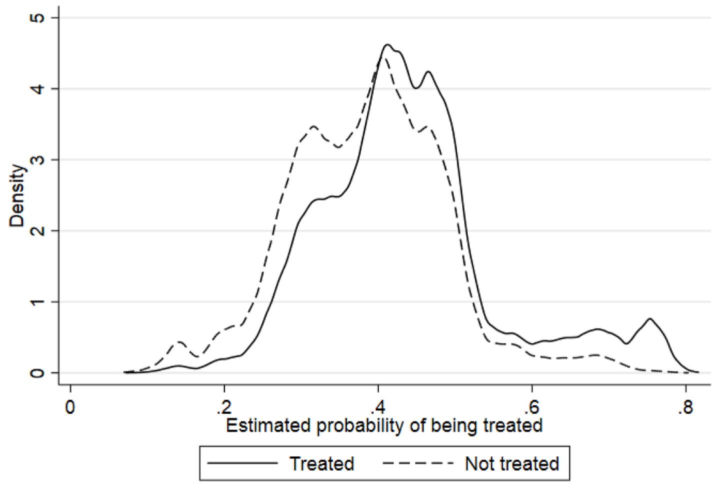

Figure C3: Estimated densities of the predicted probability of getting automatically enrolled in a workplace pension - targeted employees

Notes: "Treated": Estimated density of the propensity score that a targeted employee who is in fact enrolled in a workplace pension is not enrolled. "Not treated": Estimated density of the propensity score that a targeted employee who is in fact not enrolled in a workplace pension is enrolled. Data pooled across employees in all years 2012-2016. Predicted probabilities based on a logistic regression model. The kernel estimator uses the triangle function and optimal Silverman bandwidth.

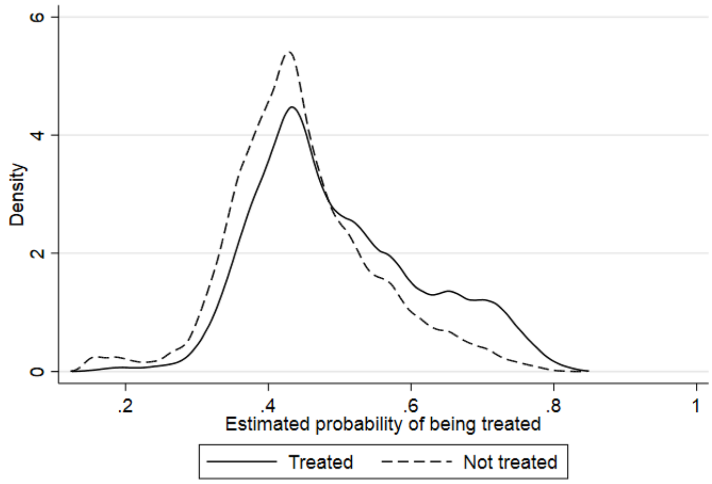

Figure C4: Estimated densities of the predicted probability of getting automatically enrolled in a workplace pension - PP employees

Notes: "Treated": Estimated density of the propensity score that an employee who is in fact enrolled in a workplace pension is not enrolled. "Not treated": Estimated density of the propensity score that a PP employee who is in fact not enrolled in a workplace pension is enrolled. Data pooled across employees in all years 2012-2016. Predicted probabilities based on a logistic regression model. The kernel estimator uses the triangle function and optimal Silverman bandwidth.

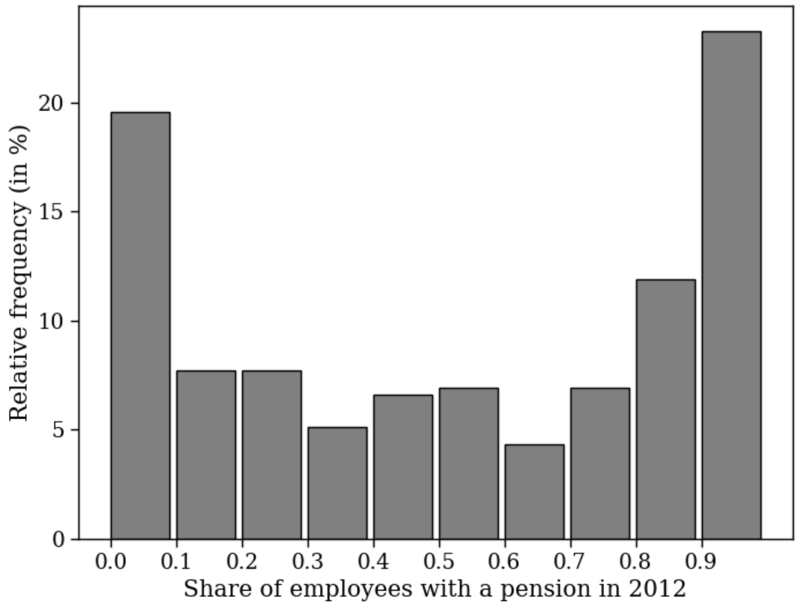

Figure C5: Distribution of shares of employees who had a workplace pension in 2012 within one-digit occupation-firm cells with at least ten employee observations

Notes: Data pooled across employees in all years 2012. We first group all observations by firm/one-digit SOC occupation code pairs and then calculate the proportion of employees in each cell participating in a workplace pension scheme. We exclude any cell with fewer than ten observations in 2012.

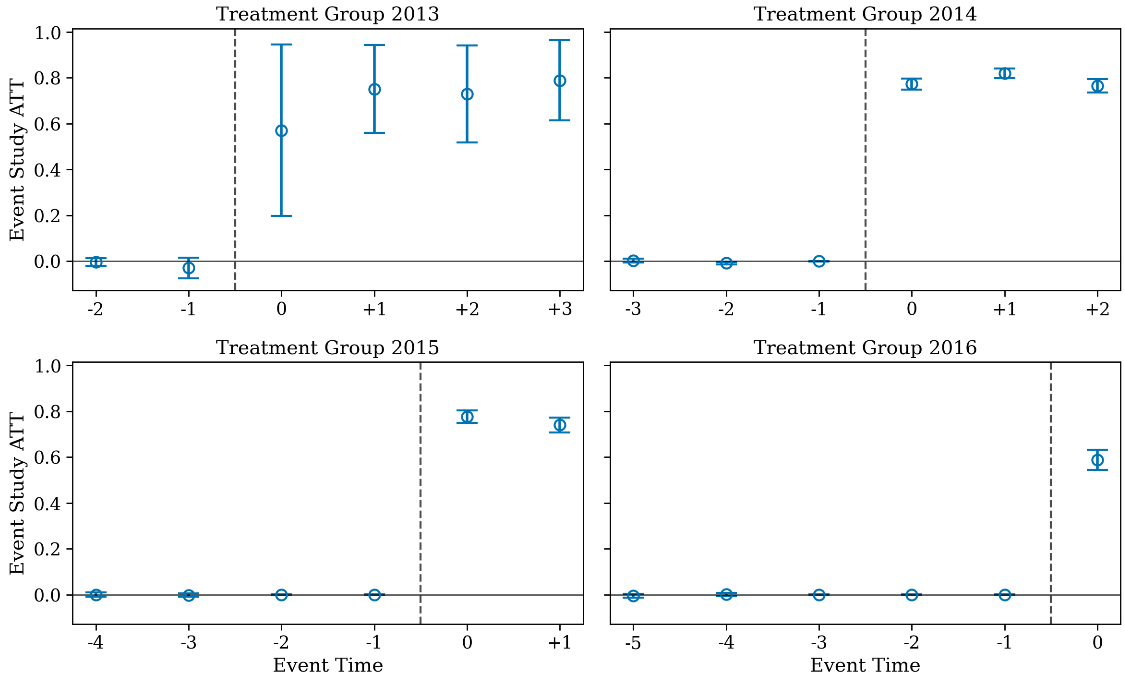

Figure C6: Effect of AE on pension enrollment rates of targeted employees by treatment group

Notes: Event-study estimates from (12) for enrollment rates. See Figure 4 notes for more details.

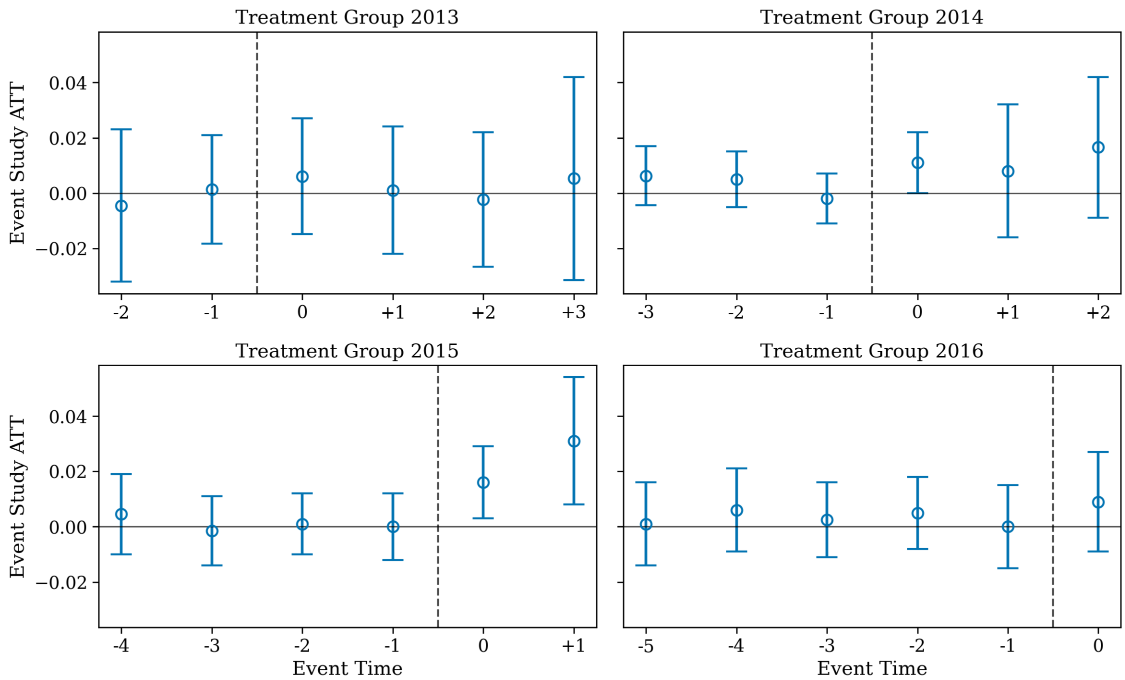

Figure C7: Effect of AE on log(total compensation) of targeted employees by treatment group

Notes: Event-study estimates from (12) for log total compensation. See Figure 4 for more details.

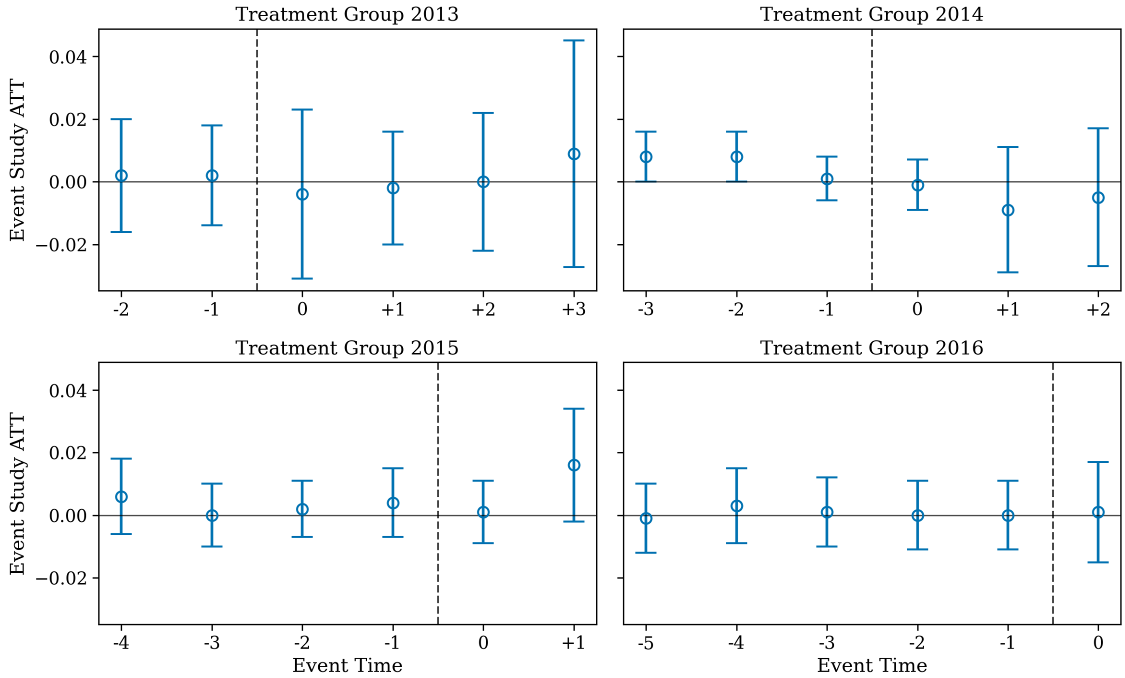

Figure C8: Effect of AE on log(basic pay) of targeted employees by treatment group

Notes: Event-study estimates from (12) for log basic pay. See Figure 4 for more details.

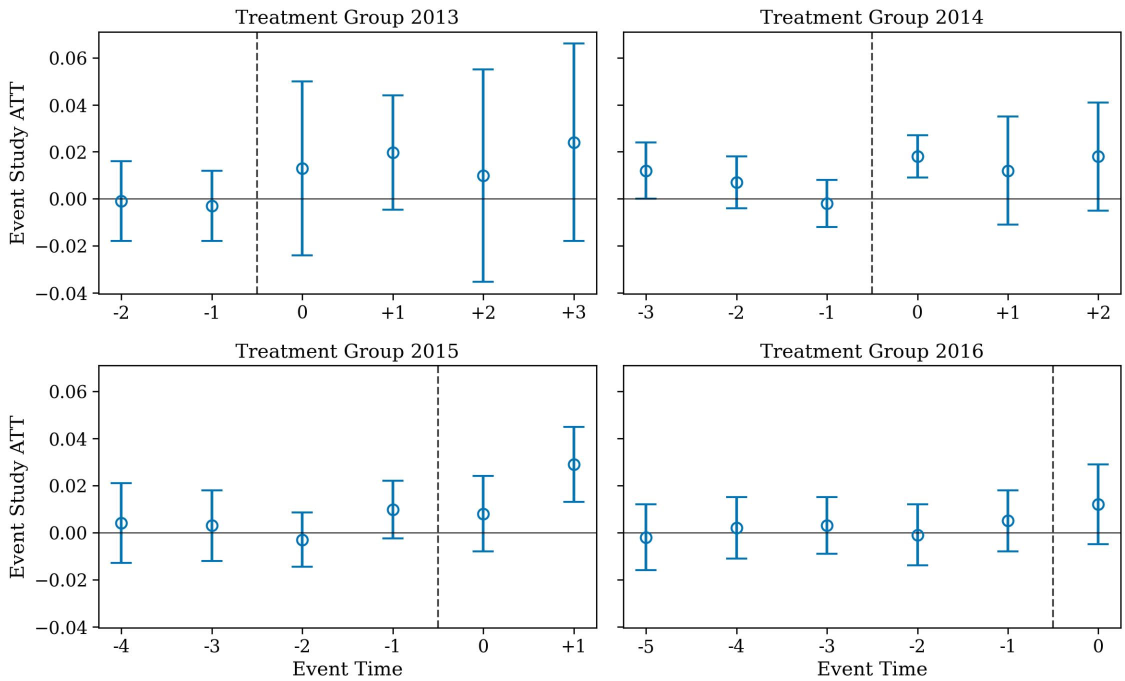

Figure C9: Effect of AE on log(basic + pension) of targeted employees by treatment group

Notes: Event-study estimates from (12) for the log of the sum of firms' pension contributions and employees' basic pay. See Figure 4 for more details.

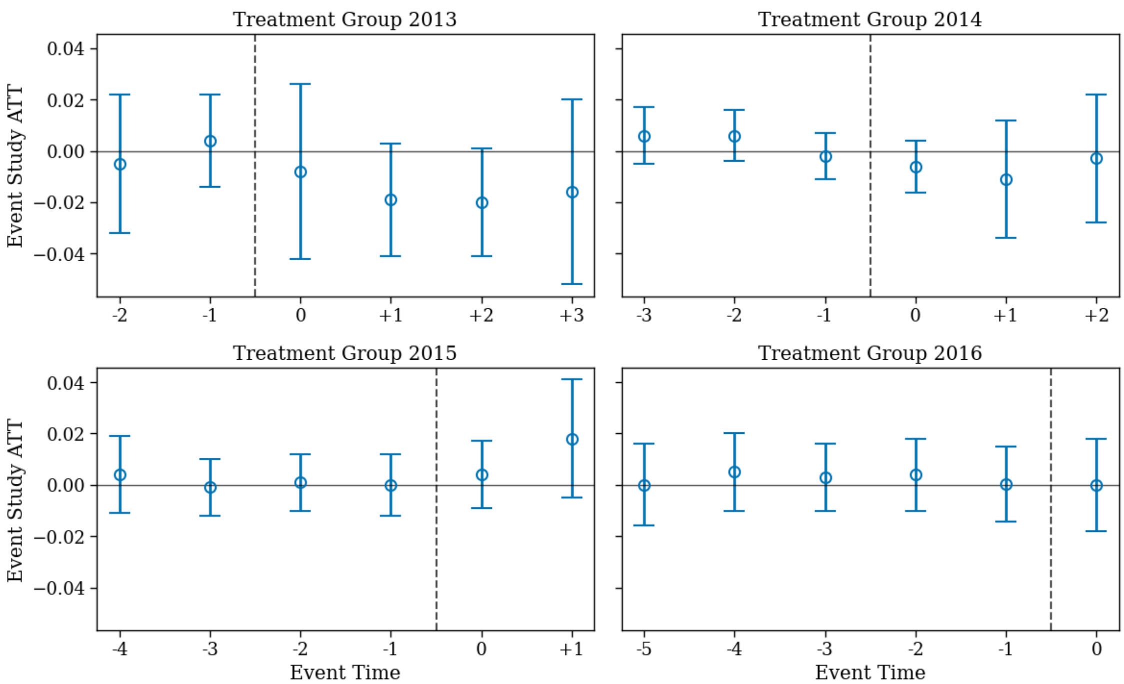

Figure C10: Effect of AE on log(take-home pay) of targeted employees by treatment group

Notes: Event-study estimates from (12) for the log of the sum of basic pay and extra pay. See Figure 4 for more details.

Appendix D. More Details on Hours Worked

Here, we analyze the response of basic hours and overtime hours worked by targeted employees to the introduction of the AE mandate. The ASHE questionnaire asks employers to provide the actual number of hours worked within the reference pay period in April. However, if a firm does not track how many basic hours the employees are working, the firm might provide contracted basic hours instead of actual basic hours. We repeat our estimations from the main text but use the log of basic hours and the log of basic plus overtime hours as dependent variables.

Table D1: Effect of AE on hours worked by targeted employees

	Log(basic hours) (1)	Log(total hours) (2)
Overall ATT, $\hat{\theta}_O$		
Coefficient estimate	0.000	−0.004
	[-0.006, 0.006]	[-0.011, 0.002]
Group ATT, $\hat{\theta}_{group}$		
Group 2013	−0.002	−0.013
	[-0.019, 0.014]	[-0.028, 0.001]
Group 2014	0.000	0.000
	[-0.075, -0.076]	[-0.008, 0.008]
Group 2015	0.005	0.008
	[-0.002, 0.012]	[-0.001, 0.017]
Group 2016	0.007	0.006
	[-0.003, 0.018]	[-0.005, 0.017]
N Observations (jobs × years)	169,355	169,355

Notes: Basic hours include any hours paid at shift premium and paid hours even if not worked. Overtime hours give the number of paid overtime hours.
See Table 5 for details.

Table D1 shows no significant evidence that overall basic hours respond to the pension reform. Looking at the group-level effects, the confidence bands become too large to exclude large responses, but the coefficient estimates are close to zero. In contrast, the estimates for log total hours suggest possible declines among the largest firms, with coefficients of -0.013 and confidence bands just covering the null effect. As basic hours are relatively constant, this suggests that overtime hours may respond to the reform. Our results discussed in the main text (Table 7) indicate that the likelihood of working (paid) overtime hours significantly declines among targeted employees due to the pension reform.

Appendix E. Further Details of the Empirical Method

Here, we provide some more background on the estimation approach proposed by Callaway and Sant'Anna (2021). The approach of CS consists of three steps: First, isolate the comparisons between firms that are treated by time t and a control group consisting of both never-treated firms and firms that have not yet been treated by time t. Second, use this sample to estimate the average treatment effects for each year and comparison. Finally, aggregate the estimated ATTs to economically meaningful parameters using appropriately chosen weights, which reflect the precision of the estimates and the number of underlying observations.

Under the assumptions discussed in the main text, the group-time ATT of group g at time t is given by

$$\text{ATT}(g,t) = E\left[\underbrace{\left(\frac{AE_g}{E[AE_g]} - \frac{c_{gt}(X)}{E\left[c_{gt}(X)\right]}\right)}_{\text{Inverse probability weight}}\underbrace{\left(W_t - W_{g-1} - m_{gt}(X)\right)}_{\text{Outcome regressions}}\right] \tag{13}$$

$$c_{gt}(X) = (1-D_t)(1-AE_g)\frac{p_{gt}(X)}{1-p_{gt}(X)} \tag{14}$$

$$p_{gt}(X) = P\left(AE_g = 1|X, AE_g + (1-D_t)(1-AE_g) = 1\right) \tag{15}$$

$$m_{gt}(X) = E\left[W_t - W_{g-1}|X, D_t = 0, AE_g = 0\right], \tag{16}$$

where W_t is an outcome variable (targeted employees' wages, hours worked, and pension participation) at time t, W_{g-1} is the average outcome the year before AE became mandatory, AE_g is a dummy that equals one for employees in treatment group g, and D_t is a dummy that equals one for employees treated at time t. The generalized propensity score, p_{gt}, is the probability that an employee is in treatment group g, conditional on pre-treatment covariates X (discussed below) and on either being a member of group g (in this case, $AE_g = 1$) or being a member of a different group than g that has not yet been treated by time t (in this case, $D_t = 0$). By using the inverse of the selection probability, this estimator aims to correct for non-random selection into treatment (Abadie, 2005). If this weighting is successful, the estimator compares targeted employees who, based on covariates, were equally likely to be employed by treated firms, even though those employees differ by actual treatment status. This means that the only difference between employees is the treatment, so any observed difference in outcome variables is caused by the treatment. The second component in (13) is the population outcome regression, $m_{gt}(X)$, see Heckman et al. (1998). First, we estimate a regression model for the outcome variables using the sample of the not-yet-treated targeted employees. Second, we use the fitted regression model to predict the counterfactual change in average outcome variables from year $g-1$ to t for the treated employees. This predicted change is then subtracted from the observed average change over the same period. As CS explain, the above estimator (13) is "doubly-robust" in the sense that it only requires us to specify correctly either, but not necessarily both, the outcome regression for the control group or the propensity score.

Appendix F. Diagnostics for the Two-Way Fixed Effects Model

As previously described, firms had to adopt AE from 2013 to 2016. In such settings, the standard in applied work has long been to estimate the average treatment effect on the treated (ATT) using the two-way fixed effects (TWFE) model. However, recent work has shown that TWFE models can yield severely biased coefficient estimates when treatment effects vary across either treatment groups or time.[3] Here, we apply the diagnostic procedure proposed by Jakiela (2021), documenting evidence that coefficient estimates from the TWFE model are likely biased in our setting. First, we show that the TWFE model places negative weights on some observations of earlier treated targeted employees. Second, we show that our data reject the hypothesis that treatment effects are constant over time.

Suppose we want to estimate the ATT of the introduction of AE on outcome Y_{it}, where i denotes a targeted employee and t denotes the year. We use data for the period 2010 to 2016. Treatment varies at the employee-year level, and treatment is indicated by $AE_{it} = 1$, zero otherwise. Once an employee is treated, they remain treated. The standard TWFE regression, in this case, is

$$Y_{it} = \alpha + \lambda_i + \gamma_t + AE_{it} \times \beta_{post} + \varepsilon_{it} \tag{17}$$

where λ_i denotes the employee fixed effect, γ_t denotes the year fixed effect.[4] If all employees had the same average treatment effect in the k-th year post-AE, $\text{ATT}_k = \text{ATT}$, then the population regression coefficient β_{post} equals the ATT under the usual difference-in-differences assumptions of parallel trends and no anticipation effects (Borusyak et al., 2023). However, the coefficients from the TWFE model may be severely biased if the treatment effects vary over time across treatment groups.[5] Intuitively, the OLS estimate of β_{post} is a weighted average of all possible 2×2 comparisons on the data. This also includes comparisons that use targeted employees treated earlier as the 'control group' for employees treated later. For example, employees employed in firms that had to introduce AE in 2013 may be the "control group" for employees who had to introduce AE in 2015. If earlier-treated employees are sufficiently often the 'control group' for later-treated employees, the k-th period treatment effect of earlier-treated employees may

[3] See, for example, De Chaisemartin and d'Haultfœuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021), Borusyak et al. (2023), and the survey by Roth et al. (2023).

[4] In the estimation, we also include a vector of time-varying controls, $\mathbf{X}_{it}$, that is exogenous to the treatment. The controls are employee age, age squared, and firm tenure squared. We center each control by subtracting their sample means.

[5] For example, treatment effects would vary over time across treatment groups if the ATT of AE in 2014 of the 2013 treatment group differed from the ATT in 2016 of the 2015 treatment group.

receive a negative weight in the computation of the aggregate estimate of β_{post}, see Sun and Abraham (2021).

Based on these TWFE mechanics, Jakiela (2021) proposed a two-step diagnostic procedure for assessing the likely severity of bias in the TWFE estimates: In the first step, check whether some treated employees receive negative weights, and, if that is the case, test in the second step for heterogeneous treatment effects across groups. If both negative weights and heterogeneous treatment effects are detected, then TWFE estimates of the ATT are likely biased. The weights are proportional to the treatment indicator after the estimated employee and year fixed effects have been subtracted (see also Sun and Abraham, 2021):

$$\widetilde{AE}_{it} = AE_{it} - (\hat{\lambda}_i + \hat{\gamma}_t) \tag{18}$$

whereby the estimates $\hat{\lambda}_i$ and $\hat{\gamma}_t$ are obtained from the auxiliary regression $AE_{it} = \alpha + \lambda_i + \gamma_t + u_{it}$. If the predicted value $(\hat{\lambda}_i + \hat{\gamma}_t)$ is greater than one, $\widetilde{AE}_{it}$ will be negative even when an employee is treated, and so that employee's outcome will receive a negative weight in $\hat{\beta}_{post}$. This is the well-known issue of the OLS estimator when predicting binary outcomes; predictions may lie outside of the unit interval.

Figure F1 displays the weights of employee-year observations when estimating the TWFE coefficients for the ATT of introducing AE on pension participation in our targeted employee sample described in the main text. Some treated employee-year observations receive a negative weight, while some not-yet-treated targeted employees receive a positive weight. Similar results hold for all other outcome variables discussed in the main text: there are always some treated employee-year observations that receive a negative weight.

That some targeted employees receive a negative weight in the estimation of the coefficient $\hat{\beta}_{post}$ is not a problem for the validity of the TWFE estimator as long as treatment effects are homogeneous across groups and time. Therefore, we now test the hypothesis of homogeneous treatment effects as suggested by Jakiela (2021). We run the following regression

$$\widetilde{Y}_{it} = \widetilde{AE}_{it} + AE_{it} + \delta \times (\widetilde{AE} \times AE)_{it} + e_{it} \tag{19}$$

whereby the dependent variable is the residualised outcome $\widetilde{Y}_{it} = Y_{it} - (\tilde{\lambda}_i + \tilde{\gamma}_t)$, obtained in a similar way as the residualised treatment indicator in step one. We are interested in the coefficient estimate $\hat{\delta}$, which indicates whether the estimated relationship between $\widetilde{Y}_{it}$ and $\widetilde{AE}_{it}$ is significantly different across treatment and control groups. The estimation results in Table F1 show clear evidence against the hypothesis of homogeneous treatment effects: For all outcome variables, we find that the coefficient estimates of the interaction term are statistically significant, rejecting the assumption of constant treatment effects.

Taking together the evidence presented in this appendix, we conclude that negative weights and heterogeneous treatment effects likely lead to a severe bias of any

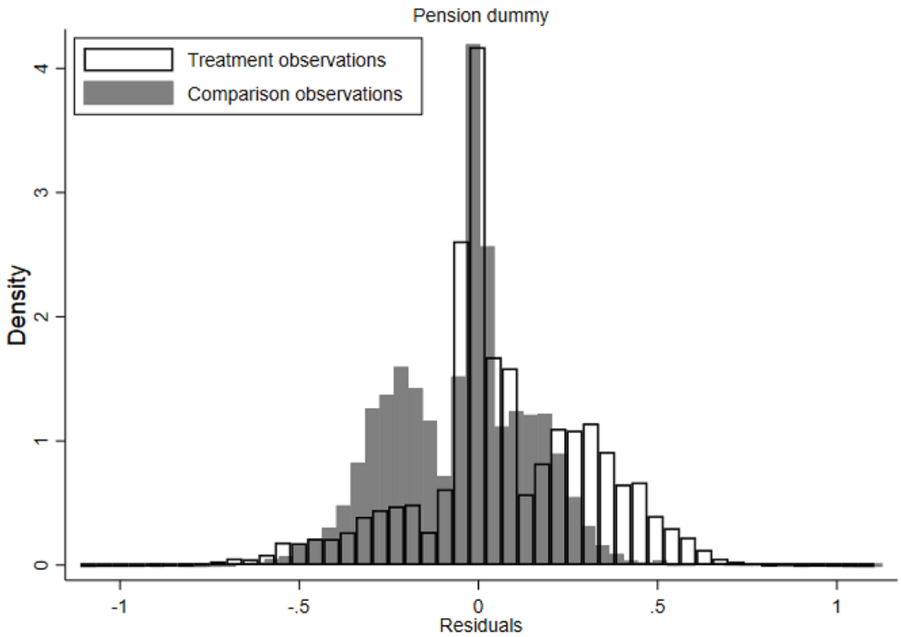

Figure F1: Two-Way Fixed Effect Estimation Weights

Notes: The weights equal the residualised treatment indicator, (18), divided by $\sum_{it} \widetilde{AE}_{it}^2$, shown separately for treatment and control groups.

Table F1: Testing for heterogeneous treatment effects

	Pension participation (1)	Log total compensation (2)	Log basic pay (3)	Log pension contribution (4)	Log extra pay (int. margin) (5)	Log extra pay (ext. margin) (6)
Estimate $\hat{\delta}$	-0.028^{***}	-0.011^{***}	-0.022^{***}	-0.021^{***}	0.122^{***}	0.025^{***}
	(0.004)	(0.003)	(0.003)	(0.003)	(0.025)	(0.007)
N Observations (jobs × years)	244,337	244,337	244,337	244,337	106,516	227,850

Notes: Estimates from regression (19).
Standard errors in parentheses, allowing for clustering at the firm level.
*, **, and *** indicate significance at the 10, 5, and 1% levels, respectively.

TWFE estimates of the ATT of the introduction of AE on targeted employees' pension participation rates and wages.

Appendix G. The Effects of Automatic Enrollment on Employees Who Were Enrolled in a Workplace Pension Before the Mandate

We estimate the effect of AE on employees with existing pension by repeating the same analysis as for targeted employees. Table G1 shows the overall and group ATT coefficient estimates for those employees. Enrollment in workplace pensions declines by 2.3 percentage points from full participation, and this effect is similarly strong across all treatment groups. A survey of UK employers found that before the reform, 3% of firms planned to reduce contribution levels for existing workplace pension plans to absorb the increased contribution costs for newly enrolled employees, and 12% of firms intended to modify the existing workplace pension plan (Department for Work and Pensions, 2016). It seems reasonable that some employees with existing pensions may have considered the new workplace pension plan or lower contribution rates less attractive, resulting in their decision to opt out after the introduction of AE. All of the other coefficient estimates show no evidence for an effect of AE.

Table G1: Effect of AE on pension enrollment and wages of employees with existing workplace pensions before AE: Difference-in-differences estimates

	Enrollment (1)	Log(total compensation) (2)	Log(basic pay) (3)	Log(basic + pension) (4)	Log(take-home pay) (5)
Overall ATT, $\hat{\theta}_O$					
Coefficient	-0.022^{*}	-0.002	0.003	0.001	0.000
	[-0.027, -0.018]	[-0.010, 0.006]	[-0.003, 0.009]	[-0.007, 0.008]	[-0.008, 0.008]
Group ATT, $\hat{\theta}_{group}$					
Group 2013	-0.028^{*}	-0.013	-0.002	-0.003	-0.013
	[-0.043, -0.011]	[-0.032, 0.005]	[-0.016, 0.012]	[-0.019, 0.013]	[-0.033, 0.006]
Group 2014	-0.019^{*}	0.003	0.006	0.003	0.007
	[-0.023, -0.016]	[-0.008, 0.015]	[-0.003, 0.016]	[-0.007, 0.014]	[-0.005, 0.018]
Group 2015	-0.019^{*}	0.009	0.005	0.003	0.009
	[-0.025, -0.016]	[-0.004, 0.023]	[-0.006, 0.015]	[-0.008, 0.013]	[-0.004, 0.023]
Group 2016	-0.027^{*}	0.009	0.006	0.000	0.010
	[-0.044, -0.011]	[-0.009, 0.027]	[-0.008, 0.020]	[-0.019, 0.020]	[-0.007, 0.028]
N Observations (jobs × years)	163,384	163,384	163,384	163,384	163,384

Notes: Group treatment effect estimates from equation (11), which show the relative change in the outcome variable of employees with existing workplace pension in group g from the year immediately before that group's respective staging date, compared to employees in groups that are not yet past their staging date.
Bootstrapped 95% confidence bands are shown in brackets (999 samples). We allow for clustering at the firm level. * indicates significance at the 5% level.