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Does Pension Automatic Enrollment Increase Debt? Evidence from a Large-Scale Natural Experiment
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ABSTRACT

Does automatic enrollment into retirement saving increase household debt? We study the randomized roll-out of automatic enrollment pensions to ~160,000 employers in the United Kingdom with 2-29 employees. We find that the additional savings generated through automatic enrollment are partially offset by increases in unsecured debt. Over the first 41 months after enrollment, each additional month increases the average automatically enrolled employee's pension savings by £33-£39, unsecured debt (such as personal loans and bank overdrafts) by £7, the likelihood of having a mortgage by 0.05 percentage points, and mortgage balances by £120. Automatic enrollment causes loan defaults to fall and credit scores to rise modestly.

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1 Introduction

Individuals in many countries are automatically enrolled to save in retirement pensions unless they opt out—perhaps the most widespread policy implementation of nudging (Thaler and Sunstein, 2009).[1] Automatic enrollment is intended to raise household net wealth during the accumulation phase of retirement saving (Thaler, 1994; Beshears et al., 2006). Previous research has shown that the policy substantially increases pension participation rates, leading to higher average saving within the pension (Madrian and Shea, 2001; Choi et al., 2002, 2004; Beshears et al., 2009; Cribb and Emmerson, 2021; Blumenstock et al., 2018). However, the effects of automatic enrollment could be offset on other margins.

In this paper, we examine how automatic enrollment pensions affect the borrowing behavior of households, using linked administrative pension and credit file data. We obtain causal estimates by exploiting the randomized timing of when automatic enrollment pensions were rolled out to approximately 160,000 employers with 2-29 employees in the UK. Initially, 2% of qualifying earnings were contributed by default to the pension, of which at least 1% of qualifying earnings had to come from the employer. We show that the additional observed pension balances created by the introduction of automatic enrollment pensions are accompanied by significant increases in debt during the first 41 months after enrollment.

The average automatically enrolled employee accrues an additional £33-£39 of observed pension savings per month within the automatic enrollment pension, of which £16-£19 are employer contributions, £13-£15 are employee contributions, and £3-£4 are tax credits deposited to the pension. But this average employee simultaneously accrues an additional £7 of unsecured debt (such as personal loans and bank overdrafts) per month of enrollment, which is 18-21% of the increase in total pension savings and 37-44% of the increase in employee contributions. Further, the probability of having a mortgage increases by 0.05 percentage points per month of enrollment (a cumulative 2 percentage points at 41 months after enrollment), against a baseline prevalence of 38%, and the average mortgage balance correspondingly increases by £120 per month of enrollment. We find no statistically significant effects on vehicle loan balances. Surprisingly, time under automatic enrollment progressively reduces loan defaults and increases credit scores, so that by 41

[1] Automatic enrollment is legally required of employers in New Zealand, Poland, Quebec Province, and Turkey. Chile automatically enrolls self-employed individuals, and Brazil automatically enrolls federal government civil servants. In 2019, 40% of US private industry workers and 28% of US state and local government workers participating in a savings and thrift plan did so in one with automatic enrollment (Zook, 2023), and most 401(k) and 403(b) plans established from year-end 2022 onwards will be required to automatically enroll employees by 2025.

months after enrollment, the probability of having defaulted within the previous six years has fallen by 1.6 percentage points (13% of the baseline rate) and credit scores have increased by 0.07 standard deviations. We estimate no statistically significant effect on the likelihood of bankruptcy.

Our research design is enabled by the UK Pensions Act 2008. The Act obliged all firms to offer automatic enrollment pensions, a policy designed to address the fact that approximately 10 million UK adults were in employment but not enrolled into a workplace pension. To support implementation, the UK established the National Employee Savings Trust (Nest) to provide low-cost pensions, with a public service mandate to serve all eligible firms. Due to the large number of firms and employees to be enrolled, the deadlines by which firms were required to introduce automatic enrollment pensions were staggered over time. In particular, firms with 2-29 employees were randomly assigned an introduction date that lay within a two-year window from June 2015 to April 2017.

We link together individual-level pension contribution records from Nest, employer data on the firm's Nest pension introduction date, and credit file data from Experian. The merged data cover a three-year period, allowing us to estimate the effects of automatic enrollment over an extended period of time. Because we only observe pension contributions and credit outcomes of individuals who enroll in the Nest pension, our estimates are "treatment on the treated" effects obtained by comparing individuals who, by virtue of their employer's deadline, enrolled earlier versus later.

Although we are able to provide credible estimates of automatic enrollment's effect on borrowing, our empirical setting is less well-suited to distinguish among many possible interpretations of why these effects arise. Borrowing may increase because individuals are rationally choosing to consume today some of the new wealth they are receiving via employer retirement contributions and tax credits, because individuals are incurring expenses associated with new home purchases, or because inattentive and inertial individuals fail to adjust their spending in response to lower take-home pay caused by their own retirement contributions. Our results also may not speak to how individuals would respond if the default employee retirement contribution increased while the employer contribution remained unchanged. In the setting we study, consenting to being automatically enrolled was necessary to receive any employer contribution, which was large relative to the default employee contribution. Individuals might reasonably decide that borrowing to receive the employer contribution is worthwhile, but would not borrow to accept a default increase in their own contribution that did not earn them additional employer contributions.

Our study is related to Beshears et al. (2022), who study the effects of automatic enrollment using a natural experiment created when the U.S. Army began automatically enrolling its newly hired civilian employees into a defined contribution pension plan. They find that automatic enrollment causes no statistically significant change in debt balances. Although our study's estimates come from a different setting, they are qualitatively consistent with those of Beshears et al. (2022), whose much smaller sample leads to wider confidence intervals. Beshears et al. (2022) cannot reject at the 5% significance level the hypothesis that, four years after the introduction of automatic enrollment, the policy increases debt excluding vehicle loans and first mortgages by up to 28% of the cumulative increase in total contributions (83% of the cumulative increase in employee contributions), increases first mortgage debt by up to 231% of the cumulative increase in total contributions (675% of the cumulative increase in employee contributions), and decreases the prevalence of severely late balances by 1.4 percentage points.

Our study is related as well to Choukhmane and Palmer (2024), who estimate, using data from a single bank, the effects of increasing the minimum permissible non-zero total pension contribution rate in the UK from 2% to 5% (with at least 2% coming from the employer) on April 2018 and to 8% (with at least 3% coming from the employer) on April 2019. Comparing how behavior evolved among individuals who were already contributing above the new minimum to those who were compelled to either raise their contributions or opt out, they find that for every £1 reduction in take-home pay, consumers cut their spending by £0.34 and increase their balances on credit cards issued by the bank by £0.79. They do not observe credit scores or debt balances aside from credit card balances at their partner bank. In contrast, we do not estimate a statistically significant increase in credit card debt. The difference in the credit card responses may be due to the fact that we study a different population than Choukhmane and Palmer (2024) or because opting out when the minimum contribution rate is 8% is much more costly in terms of foregone employer contributions than when the minimum contribution rate is 5%, which in turn is more costly to opt out of than when the minimum contribution rate is 1%.

The effects we observe are an example of a policy nudge yielding unintended consequences, in this case on a wide scale. Other researchers have documented unintended consequences from nudges. Choukhmane (2024) finds that automatic enrollment in the current employer's pension reduces employees' contributions to their next employer's pension if that future pension does not also have automatic enrollment. Choi et al. (2023) find that employees subject to automatic enrollment or default contribution auto-escalation subsequently withdraw a higher fraction of their 401(k) balances upon separating from their jobs than employees not subject to these policies. Chetty et al. (2014) estimate that about 30% of *compulsory* retirement savings in Denmark is undone via increased debt and reduced saving in non-retirement accounts. Beyond the domain of pensions, Medina (2021) shows that credit card payment reminders reduce credit card late-payment fees, but increase checking account overdraft fees. Guttman-Kenney et al. (2023) find that shrouding the option to automatically make only the minimum required credit card payment each month causes cardholders to sign up for higher automatic monthly payments, but has no effect on medium-term debt because it reduces subsequent manual payments.

Our findings also relate to the large literature on the tendency of households to accumulate assets and debts simultaneously, first documented by Morrison (1998) and Gross and Souleles (2002).[2] That literature focuses on households co-holding low-yielding liquid assets and high-interest debt simultaneously. A variety of explanations for such co-holding have been offered in the literature, including demand for the liquidity services of cash, precautionary saving to guard against involuntary credit line decreases, limited self-control, and low financial sophistication. Our analysis suggests that a policy designed to induce higher *illiquid* saving increases debt within household balance sheets. Relatedly, Medina and Pagel (2024) find that bank customers who were encouraged to save via text messages increase their savings while maintaining their levels of consumer debt.

The paper proceeds as follows. Section 2 provides background on the policy design and experiment. Section 3 describes the data. Section 4 outlines the econometric model. Section 5 contains the main results. Section 6 discusses explanations for the effects we find. Section 7 concludes the paper.

2 Policy Background and Empirical Strategy

The UK Pensions Act 2008 introduced an obligation on firms with at least two eligible employees to automatically enroll all of their eligible employees into a workplace pension. Eligible employees are aged between 22 and the State Pensions Age (during the roll-out period for the sample of firms

[2]Subsequent studies on this topic include Telyukova and Wright (2008), Bertaut et al. (2009), Telyukova (2013), Gathergood and Weber (2014), Druedahl and Jørgensen (2018), Gorbachev and Luengo-Prado (2019), Choi and Laschever (2018), Vihriälä (2022), and Choi (2022).

we study, this age was 65 for men, and in the 62-64 range for women, depending upon their month and year of birth), employed continuously for at least three months, earning a minimum amount (currently £10,000 per annum), and not already participating in a qualifying pension scheme.[3] The Act includes escalating penalty notices for employer non-compliance, with a maximum penalty of £10,000 for each day the firm does not offer a pension to eligible employees. Willful failure to put eligible employees into a pension scheme and knowingly and falsely declaring compliance can result in two years in prison or a fine for the company directors.

Because this policy would involve the enrollment of more than 10 million individuals into new pensions, its roll-out was spread out over time, beginning with the largest firms, defined by their size as of April 1, 2012. The roll-out occurred between October 1, 2012, and April 1, 2017, for firms already in existence before April 2012, and between April 1, 2017, and February 1, 2018, for firms established afterwards. The regulatory body in the UK, The Pensions Regulator (TPR), assigned a "staging date" on which automatic enrollment of all eligible employees must ordinarily take place at each firm not currently offering an employer-provided pension. Employers could postpone automatically enrolling employees for up to three months after the staging date, but they were required to inform employees of the delay and accept opt-in enrollments between the staging date and the actual automatic enrollment date. It was illegal to automatically enroll employees before one's staging date, but employers could apply to TPR to move their staging date earlier than their assigned date, or allow their employees to opt into making Nest contributions before their staging date.[4]

For larger firms, staging dates were assigned based upon size only. Firms with 120,000 or more employees were obliged to start offering the scheme by October 1, 2012; for those with 50,000-119,999 employees, November 1, 2012; for those with 30,000-49,999 employees, January 1, 2013; and so on down to firms with 30-39 employees, whose staging date was October 1, 2015.[5]

Our study focuses on the set of firms that already existed in April 2012 with 29 or fewer employees. In this group, assigned staging dates were randomized because of the large number of employers and employees who would be enrolled. TPR allocated these firms to staging dates

[3]Over the period of our study, the State Pension Age for women was increased so as to equalize State Pension Ages for men and women at 65 by November 2018.

[4]It was possible for an employer to do something similar to automatic enrollment for any newly joining worker before its staging date by including pension scheme membership into the employment contract. This is referred to as "contractual enrollment." We are not aware of the smallest employers ever doing this, as it was legally complex and brought risks with it. The Nest system is not set up to allow for contractual enrollment.

[5]Appendix Table A1 provides the staging dates by employer size.

(shown in Table 1) between June 1, 2015, and April 1, 2017, using the last two digits of their Pay-As-You-Earn (PAYE) number. PAYE numbers are the unique payroll tax identifier for firms in the UK. Assignment of the final two digits of the PAYE number, which is given at firm birth by the UK tax authority, is as-good-as-random. Appendix A1 provides details on the assignment of these numbers. The consequent randomization to staging date allows us to exploit the roll-out as a natural experiment, as at any point after June 2015, employees of some firms had been exogenously subject to automatic enrollment for longer than others.

Employees were notified of their enrollment. This began with a public information campaign in advance of the rollout, involving TV and radio advertising. Employees automatically enrolled into the Nest pension were informed of their enrollment, first in a written communication from their employer, then by Nest via a letter and brochure sent to their home address (as provided by the employer). These communications included details of the pension and information on how to opt out. The UK government's evaluation report for the automatic enrollment policy indicates a 74% awareness of the introduction of auto enrollment among the target population.[6]

Employees had one month after enrollment to opt out and obtain a full refund of any contributions. For employees who did not opt out, employers were required to make a minimum pension contribution of 1% of qualifying earnings. Qualifying earnings in tax year 2015–2016 were those between £5,824 and £42,385 per year; this band is reviewed each year. For those who did not opt out, the total minimum employee contribution plus employer contribution plus tax relief (the government contribution to the Nest account that equals the reduction in tax liability granted due to the employee's contribution—typically 20% of the employee contribution) was initially 2% of qualifying earnings. This minimum rose to 5% of qualifying earnings (with at least 2% from the employer) in April 2018, and to 8% (with at least 3% from the employer) in April 2019. Employers can set a default contribution rate higher than these minimums. Employees can also choose to save a different amount than the default employee contribution, although contributions that exceed 100% of their earnings in the year or a certain pound threshold in the year (£40,000 for most of our study period) cannot be made using before-tax money.[7]

We draw data from Nest, which is the largest provider of automatic enrollment pensions in the UK. Nest's offering is a defined contribution scheme, including a choice of investment funds and a default target retirement date fund. It is free for employers to use and has a public service

[6] https://www.gov.uk/government/publications/automatic-enrolment-evaluation-report-2014

[7] This pound contribution threshold is reduced for high-income individuals.

obligation, whereby any employer can use Nest to meet its mandatory automatic enrollment obligations. Firms are not required to use Nest. They can meet their mandatory automatic enrollment obligations by using their own existing scheme, setting up a new one, or outsourcing provision to an external provider such as an insurer or a multi-employer mastertrust. However, the vast majority of small firms that have introduced pensions as a result of the automatic enrollment mandate have chosen to use the Nest scheme due to its low cost and public service obligation. As of March 31, 2021, Nest managed pensions of 9.9 million members on behalf of 881,000 employers, accounting for approximately one in three working-age individuals in the UK.[8]

Department for Work & Pensions (2020) reports that in April 2015, just before the first staging date in our sample of randomized firms with 29 or fewer employees, workplace pension participation rates were about 10% among employers with 1 to 4 employees, and about 25% among employers with 5 to 49 employees. These numbers are calculated over firms that both did and did not later use Nest. It is likely that Nest adopters are disproportionately drawn from firms that did not offer pensions prior to their automatic enrollment staging date.

3 Data

3.1 Sample Selection

Our empirical strategy is based upon the staging dates assigned to firms with 2–29 employees that incorporated on or before April 1, 2012. (We exclude employees of firms created after April 1, 2012.) Our main analysis uses all eligible workers who were employed by these firms on or up to three months after the firm-reported staging date, whose birth date made them eligible to be auto-enrolled at any of the staging dates we include in our sample, and who were enrolled into the Nest pension scheme.[9] We exclude employees who joined the firm more than three months after the firm-reported staging date because their decision to join the firm may have been influenced by the treatment.

Some employees choose to opt out of the Nest pension scheme before the one-month opt-out deadline. The opt-out rate in our sample of auto-enrollment-eligible employees is 14%.[10] Nest

[8]Figures sourced from https://www.nestpensions.org.uk/schemeweb/nest/nestcorporation/news-press-and-policy/press-releases/Nest-10-million-members-10-years-of-investing.html

[9]The employer was required to make an assessment of which employees were eligible for the scheme.

[10]We do not count employees who later change to a non-default contribution rate as having opted out. Both the numerator and denominator of the opt-out rate exclude employees who were already participating in a workplace pension. Our opt-out rate is in the neighborhood of the 10-12% opt-out rates found for employers with 1-49 employees by Department for Work & Pensions (2018) in a sample that includes firms not using Nest. It is considerably lower than

holds some basic data on individuals who opt out, including their age. Opt-out rates increase with age, from 7.6% among the under-30s to 31.4% among those aged 60 and over (see Appendix Table A2). We also observe a small decline in the opt-out rate over the sample period, from 16% in the June 2015 staging date cohort to 13% in the April 2017 staging date cohort (see Appendix Table A3). We do not observe pension contributions or credit records for employees who opt out, so they play no further role in our analysis.

We received employee records from Nest for all eligible workers at our sample of companies who were employed on or up to three months after their firm-reported staging date and were enrolled into the Nest pension scheme, together with linked employer records. We apply four additional sample restrictions.

First, we drop employees of firms that first registered for a PAYE number before April 1, 2000, as these firms received a PAYE number using a different format that did not result in as-good-as-random assignment of the last two digits. Second, we keep only employees for whom a match to a credit file could be achieved.[11] We provided the name, residential postcode, and date of birth of each employee to the credit reference agency Experian, which used this information to match individuals to their credit file. We received from Experian credit files as of three dates: November 2016, November 2017, and November 2018. November 2016 is the earliest data available due to the UK limiting the storage of credit data to a maximum of six years.[12] Third, we drop the small number of firms that reported a staging date to Nest that is not among the feasible staging dates listed in Table 1. Finally, we keep only those individuals whose birth date made them eligible to be auto-enrolled at every possible firm staging date (individuals aged at least 22 on the first staging date, and less than 65 for men and 63 years and 9 months for women on the final staging date).[13]

Our initial sample from Nest includes 712,818 employee records across 173,570 firms. The four sample selection steps reduce the number of employees to 91% of the starting sample, and the

the 30% non-participation rate among the 291 employees at small firms studied by Cribb and Emmerson (2021).

[11]Failures to match to a credit file are explained by missing or invalid postal code entries, missing or invalid name entries, and the restriction to a balanced panel. Missing postal code entries may be due, for example, to migrant workers from the European Union who temporarily reside in the UK and provide a non-UK postal address to Nest. Invalid name entries may be due to keying errors or complex name variants.

[12]Experian restricted the sample to a balanced panel as part of their data-sharing agreement for this project.

[13]Some individuals are only eligible for early staging dates or later staging dates because of their birth date. For example, males aged 64 years and 11 months in June 2015 would only be eligible for auto-enrollment if they were employees of a firm with a June 2015 staging date; they would be too old for auto-enrollment at firms with later staging dates. Individuals turning 22 in March 2017 would only be eligible for auto-enrollment if they were an employee of a firm with an April 2017 staging date; they would be too young for auto-enrollment at earlier staging dates. Without imposing our birth date filter, our analysis sample would have birth dates that are imbalanced across staging dates—earlier average birth dates in early staging dates and later average birth dates in later staging dates.

number of firms to 93% of the starting sample. Hence, our baseline sample includes 649,747 employees across 161,707 firms.[14] The sample selection step that causes the biggest loss of employees is the dropping of employees whose birth date does not make them eligible to be auto-enrolled at every staging date, which excludes 4% of the initial employee sample. The sample selection step that causes the biggest loss of employers is the dropping of pre-2000 PAYE numbers, which excludes 4% of the initial employer sample.

3.2 Variables

From the Nest member records, we obtain each individual's age and gender. We also obtain individual-level monthly observations of pensionable pay, employer and employee contributions, tax relief, and accumulated pension balances. We obtain the address of the employee from the employer records.

From the Experian credit file, we obtain each individual's credit score together with measures of debt, income, and financial distress. We use Experian's UK general purpose banking and finance credit score that is provided to lenders for credit approval decisions. The debt measures are total mortgage debt, monthly mortgage payment due, total vehicle loan debt, and total unsecured debt, which is sub-divided into revolving debt and non-revolving debt. The income measure is Experian's estimate of the individual's gross annual income.[15] The measures of financial distress are a flag for whether the individual filed for bankruptcy within the previous six years and a flag for whether the consumer entered default within the previous six years.[16]

3.3 Summary Statistics

Summary statistics for the baseline sample as of November 2017 are presented in Table 2.[17] The average age of an employee is 43 years, and 41% of the sample are female. The average monthly contribution to the Nest pension is £30, and the median contribution is £22. A credit score is present for 95% of individuals, with a mean score of 935 and a standard deviation of 184. Mean income in the sample is £35,916, and median income is £29,889, which is close to the median employee earnings in the UK population.[18] Among the 98.6% of individuals in the sample for

[14] The effects of these sample selection steps on sample size are shown in Table A4.

[15] Experian estimates income using data from credit applications, such as mortgage applications, for which applicants are required to provide evidence of their income, supplemented with data on flows through the individual's current accounts in each calendar month.

[16] Default is defined as being six months behind on payments due for at least one credit product.

[17] Summary statistics for the November 2018 credit file data are shown in Appendix Table A7.

[18] Median employee earnings in the UK in 2018 were £29,588. Source: Annual Survey of Hours and Earnings 2018. https://www.ons.gov.uk/employmentandlabourmarket/peopleinwork/earningsandworkinghours/

whom we have bankruptcy and delinquency information, 1.4% had filed for bankruptcy within the past six years, and 16% had a default within the past six years.

The bottom portion of the table provides summary statistics for secured and unsecured debts. When individuals do not hold a debt product, their balance is set to £0. Approximately one third of individuals have a positive mortgage balance, with a mean value of £52,095 and mean monthly payment of £305. Eleven percent of the sample hold a vehicle loan, with a mean debt outstanding of £1,253. Overall, 67% of the sample hold an unsecured debt product, with a mean total balance of £3,836, while the median is much lower at only £482. The mean revolving debt balance is £1,630, while the mean non-revolving debt balance is higher at £2,206.

3.4 Compliance with Staging Dates

An assumption of our empirical approach is that firms actually started enrolling employees on or close to the staging date they were assigned. The only staging date data we have for all the firms in our baseline sample is the staging date the firm reports to Nest when registering. We can assess the accuracy of these self-reported dates for approximately one-third of the 161,707 employers in our baseline sample who voluntarily reported their PAYE codes to Nest, which allows us to identify their TPR-assigned staging date. We refer to this set of employers as the "TPR-matched sample."[19]

Using this TPR-matched sample, we find that 90% of employers have self-reported staging dates that are the same as their TPR-assigned staging date. Figure A1 illustrates the cumulative density function of the difference between firm-reported and TPR-assigned staging dates. There is an approximately even split between early and late staging among the small percentage of firms with a firm-reported staging date that is different from their assigned staging date. Figure A2 shows that over 99% of the members in our sample are enrolled within three months of their firm-reported staging date. Employees that are enrolled significantly after the firm-reported staging date are those that left the employer within the three-month opt-out window, but were auto-enrolled when they subsequently rejoined the firm.

On the above bases, we have confidence that firms' self-reported staging dates are largely accurate[20], and that the randomization of assigned staging date gives rise to exogenous varia-

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[19]Table A8 shows that the TPR-matched sample and the baseline sample are extremely similar in their observable characteristics.

[20]Recall too that firms could apply to move their staging date earlier than their TPR-assigned date, so some of the self-reported staging dates that come before the TPR-assigned date may be accurate.

tion in the timing of employee exposure to automatic enrollment. Our main estimates therefore include all firms in the baseline sample, using the self-reported staging date as a proxy for the TPR-assigned date, the exogenous forcing variable for how long employees at the firm had been automatically enrolled in a pension.

3.5 Nest Contributions Prior to the Staging Date

Individuals who are employed at a sample firm on its staging date may have previously been auto-enrolled at a firm that had more than 30 employees in April 2012, and hence are not in our sample of firms. This would cause us to underestimate the effect of automatic enrollment because for some of the time during which we consider these employees to be in the untreated control group, they are actually being subjected to the treatment.

We can calculate what percentage of employees in our sample already have a positive Nest balance on the staging date we attribute to them, which indicates that they were enrolled prior to their staging date at the sample firm. Even among employees assigned to the latest staging date, April 2017, only 3.5% have a Nest balance above £50 coming into that staging date, which is not much higher than the 1.4% of employees with the earliest staging date, June 2015, who have such a pre-existing balance. Therefore, the staging dates in our data appear to correspond closely to when employees are first exposed to automatic enrollment.

3.6 Tests of Employee Characteristic Balance Across Staging Dates

Randomization in when firms introduced automatic enrollment should give rise to balance in employee characteristics across the staging dates. But employees motivated to receive employer pension contributions for additional months could potentially opt to join firms that auto-enroll earlier, causing employees with early staging dates to systematically differ from employees with later staging dates.

We conduct three sets of tests for balance of employee characteristics across staging dates. First, we test for balance in frequencies—whether the fraction of the 649,747 baseline sample employees assigned to each staging date equals the fraction of final two PAYE digits that are assigned to that staging date. Figure 1 shows that these two fractions almost perfectly match each other at each staging date; the best-fit trendline for the relationship between the proportion of employees to the proportion of final two PAYE digits across staging dates has a slope of 1.02 with a standard error of 0.02. Earlier staging dates have a smaller number of PAYE codes associated with them, so the fact that the trendline has a slope slightly greater than 1 indicates that there is no hint of a systematic movement of employees to firms with earlier staging dates.

Second, we test for balance of birth date and gender, which are unaffected by automatic enrollment. We find that these variables are balanced across all the staging dates; the p-values for tests of joint equality of means are 0.309 for age (measured as of November 2017) and 0.102 for fraction female.[21]

Third, we test for balance of characteristics found in the credit files. The earliest credit file data we have is for November 2016. Employees at firms that had already implemented automatic enrollment before the credit file date would have been under treatment for different durations across cohorts as of November 2016; hence, they are not suitable for balance tests on credit file variables, which are potentially affected by automatic enrollment.

Therefore, we test for balance of credit file variables only among employees in the firms that had not staged before November 2016—those whose firm-reported staging dates are the first of January, February, and April 2017.[22] Results from this balance test are shown in Table 3. The first three columns show the mean values of the variables for the three cohorts. The number of observations in each cell is reported in parentheses; these vary between cells due to different enrollment cohort sizes and data availability. The fourth column shows p-values for tests of joint equality of means across the three staging dates. There are no statistically significant differences in the means.

People employed by more than one sample company on these companies' staging dates (or up to three months after) create another potential complication. For the purposes of our analysis, we assign individuals to the earliest staging date applicable to them. This creates the potential for selection that becomes more stringent for later assigned staging dates, since employees will only be assigned to a staging date if there is no earlier staging date that could be assigned to them. For example, people who change jobs less frequently are more likely to be assigned to a late staging date. In practice, this bias is tiny. The fraction of employees who could have been assigned to a January, February, or April 2017 staging date—the last three in our sample—because they were employed at such a company at the right time, and are actually assigned by us to that staging date is 99.2%, 99.1%, and 98.6%, respectively.

[21] Results are shown in Table A6.

[22] Balance tests on the baseline sample implicitly assume there is balance in the sample selection across staging dates. To check this, we calculate the rate at which employees are dropped due to unmatched credit file data. We find statistical equivalence of drop rates across staging dates, which are reported in Table A5.

4 Econometric Model

We observe outcomes on three dates: November 2016, November 2017, and November 2018. Let Y_{it} be the outcome for individual i that is observed at month t. The regressor of interest is the number of months that have elapsed between i's staging date and observation, $m_{it} = \min\{0, t - j_i\}$, where j_i is the staging date of the firm in which i is employed. We will refer to this variable as "months post-enrollment" as a shorthand, since the majority of employees are enrolled on their staging date. Table 4 shows the distribution of m_{it} for each t in the sample.

We estimate two econometric models. First, we flexibly estimate the effect of time since enrollment using a set of months-post-enrollment dummies, controlling for age, gender, and observation date. The regression we use is

$$Y_{it} = \sum_{m=1}^{M} \lambda_m \mu_{mit} + \delta_t + X_i' \beta + \epsilon_{it}. \tag{1}$$

Our coefficients of interest λ_m multiply the dummies μ_{mit} for whether the number of months individual i is post-enrollment as of time t is equal to m. M is the number of unique values of m_{it} that are in our sample, δ_t is a calendar date of observation fixed effect, X_i is a vector of individual covariates (dummies for one-year-wide age groups measured as of November 2017 and a gender dummy), and ϵ_{it} is the residual term.

Second, we impose a linear functional form on the effect of m_{it} to generate a summary measure of automatic enrollment's effect over time:

$$Y_{it} = \gamma m_{it} + \delta_t + X_i' \beta + \epsilon_{it} \tag{2}$$

In both regressions, we cluster standard errors at the employer level. Because of the calendar date fixed effect controls, the regressions rely on cross-sectional variation for identification of λ_m and γ, combining estimates from the 2016, 2017, and 2018 cross-sections. At each of the three observation dates, the value of m_{it} for an individual is determined exogenously, allowing for consistent identification of the automatic enrollment effect. Effects arising from the fact that all individuals in the November 2018 cross-section have been subject to a higher minimum contribution rate since April 2018 should be absorbed into the November 2018 calendar date fixed effect.

We estimate the models on the employees who do not opt out from the pension scheme within the one-month opt-out window (individuals who opt out are not observed in the Nest data nor the credit data). Thus, the coefficient estimates from Equations (1) and (2) are the average treatment effect on the treated, comparing those enrolled exogenously earlier versus exogenously later.[23] While our previous balance tests rejected differences across the staging-date cohorts in the characteristics of those who chose to be treated, we do observe a small decline in the opt-out rate from early to late staging-date cohorts (see Table A3). This decline might indicate that there are some differences in characteristics of those who chose to be treated between the earlier versus later cohorts. These differences are likely to be economically small; 84% of the first staging-date cohort and 87% of the final staging-date cohort accepted automatic enrollment and thus enter our regression sample.

5 Results

We estimate Equations (1) and (2) using the baseline sample of 649,747 individuals. The panel is balanced, providing a total of 1,949,241 observations. We group our outcome variables into debt outcomes (unsecured debt, mortgage debt, and vehicle debt), creditworthiness outcomes (bankruptcy, default, and credit score), and savings outcomes (cumulative Nest pension contributions). For debt outcomes, we study both the level of balances and whether the individual has a positive balance.

5.1 Debt

5.1.1 Unsecured Debt

Figure 2 displays treatment effect estimates from Equation (1) for unsecured debt and its subcategories of revolving and non-revolving debt. In each plot, the only cohort whose treatment contributes to the estimation of the rightmost coefficient at 41 months post-enrollment is the small pilot cohort that staged in June 2015, which widens the confidence interval around that coefficient's point estimate. Panel A indicates that total unsecured debt increases approximately linearly with months post-enrollment. This is driven by increases in non-revolving debt (Panel C), with mostly insignificant increases seen for revolving debt (Panel B). Panels D to F show little discernible change in the probability of having any unsecured debt, revolving unsecured debt, or

[23]"Being treated" here is subjection to staging without opting out within the one-month window, without regard to whether one remains at the default contribution rate thereafter. This is slightly different from being automatically enrolled because there can be a small difference between the staging date and the autoenrollment date, and because our estimation sample includes a small number of opt-in enrollers.

non-revolving unsecured debt. This indicates that the increase in non-revolving debt is driven by the intensive margin: people who are already borrowing increase their debt levels with months post-enrollment.

To provide a summary measure of these responses, Table 5 shows estimates of Equation (2) for the set of outcomes shown in Figure 2.[24] The coefficient on total unsecured debt balances in Column (1) is positive and statistically significant at the 1% level. The coefficient value implies a £7.17 increase in total unsecured debt per month post-enrollment, which is 1.5% of the median unsecured debt balance in the sample (Table 2). In Columns (2) and (3), estimates for revolving and non-revolving debt show a larger coefficient value for non-revolving debt. The coefficient for non-revolving debt, which is statistically significant, implies a £5.56 increase in non-revolving debt per month post-enrollment. Estimates in Columns (4) - (6) show no significant effects on the likelihood of holding any unsecured debt, revolving unsecured debt, or non-revolving unsecured debt, consistent with Figure 2.

The components of non-revolving unsecured debt are unsecured loans (i.e., uncollateralized installment loans), bank overdrafts (which are classified as a non-revolving unsecured debt product in the UK), sales agreements, and other products. Appendix Table A25 shows that unsecured loans account for 62% of the increase in non-revolving unsecured debt, while bank overdrafts account for 23%. The increase in unsecured loans is the only component increase that is statistically significant.

Choukhmane and Palmer (2024) find that increases in the minimum allowable contribution rate increases credit card balances by £0.79 for every £1 reduction in take-home pay. Our estimates for automatic enrollment's treatment effects on the components of unsecured revolving debt are shown in Appendix Table A26. Neither of the components of total revolving unsecured debt show statistically significant effects. In particular, the insignificant point estimate of a £1.77 increase in credit card balances per month post-enrollment is only 11-14% of the monthly increase in employee contributions that we estimate in section 5.3.

5.1.2 Secured Debt

Figure 3 shows coefficient estimates from Equation (1) for secured debt outcomes, with Panel A showing treatment effects for mortgage balances, Panel B treatment effects for the monthly mort-

[24] Estimates of Equation (2) for unsecured and secured debt using only the November 2016, November 2017, or November 2018 data are shown in Appendix Tables A19 - A24.

gage payment due, and Panel C treatment effects for vehicle debt balances.[25] Mortgage debt balances increase approximately linearly with months post-enrollment, but there is no clear evidence from the figures that monthly mortgage payments or vehicle debt balances increase.[26] Panels D and E of Figure 3 show treatment effects for the probability of holding some mortgage debt and holding some vehicle debt. The probability of having a mortgage increases with months post-enrollment, but such an increase is not seen for vehicle debt except for the June 2015 pilot cohort at 41 months.[27]

Table 6 shows estimates of Equation (2) for the set of outcomes shown in Figure 3. Column 1 indicates an increase in mortgage balances of £120 per month post-enrollment. Column 2 shows a £0.26 per month increase in monthly mortgage payments, though this is statistically insignificant. Consistent with Figure 3, there is no statistically significant effect on vehicle debt balances in Column 3. Columns 4 and 5, in which the outcome variable is a binary indicator for whether the individual holds non-zero mortgage or vehicle debt, respectively, shows a positive and statistically significant effect only on holding mortgage debt. The probability of having a mortgage increases by 0.047 percentage points per month, or a cumulative 2 percentage points at 41 months, from a baseline prevalence of 38%.

The observed increase in mortgage balances, with a less detectable increase in mortgage payments, is consistent with both being predominantly driven by the estimated treatment effect on individuals taking on new mortgages. Among individuals whose employer did not stage before November 2016, who did not yet have a mortgage on November 2016, and who obtained a mortgage by November 2017, the mean balance of their new mortgage is £159,362, with a mean monthly payment of £716. Taking these new mortgages to be representative of the new mortgages induced by auto-enrollment, a 0.046 percentage point per month increase in new mortgage origination would be associated with a £159,362 $\times$ 0.00046 = £73 per month increase in per capita mortgage balances and a £716 $\times$ 0.00046 = £0.33 per month increase in monthly per capita mortgage payments.

[25]Tables reporting estimates of Equation (1) for each of the outcome variables shown in Figure 3 onwards are shown in Appendix Tables A10 - A12

[26]Increases in mortgage balances emerge approximately six months after enrollment and subsequently increase approximately linearly. This delay is consistent with the time required to purchase a home. In the UK, mortgage offers are typically valid for six months; hence, an individual who responded to being auto-enrolled by seeking and obtaining a mortgage offer might expect to complete the purchase and assume mortgage debt over the next six-month period.

[27]We identify individuals as having a mortgage or vehicle debt by the presence of a non-zero balance in their credit file.

5.2 Default and Credit Score

Figure 4 shows treatment effect estimates from Equation (1) for three creditworthiness outcomes: an indicator for defaulting on a debt within the previous six years, an indicator for declaring bankruptcy within the previous six years, and credit score. Surprisingly, given the increases in debt induced by automatic enrollment, defaults decline and credit scores increase with months post-enrollment, while there is no significant effect on bankruptcy filings.

Table 7 displays estimates of Equation (2) for these three outcomes. The coefficient estimates indicate a significant 0.04 percentage point decrease in the default rate per month, and a statistically insignificant 0.0008 percentage point decrease in the bankruptcy rate per month. As a point of comparison, in the baseline sample in November 2017, the prevalence of default is 16.2% and the bankruptcy rate is 1.4%. The regression estimates imply that 41 months of automatic enrollment reduces defaults by $0.04 \times 41 = 1.6$ percentage points (9.8% of the baseline rate) and bankruptcies by $0.0008 \times 41 = 0.03$ percentage points (2.3% of the baseline rate). These modest effects on creditworthiness are consistent with the statistically significant effect estimated on credit score, which increases by 0.3 points (equaling 0.002 standard deviations) per month, or 0.07 standard deviations over 41 months.

Overall, we estimate modest cumulative improvements in creditworthiness due to automatic enrollment. Although some of the improvement in credit scores could be a mechanical response to the increase in mortgage holdings, the decrease in the propensity to be in default suggests that there is a real, albeit small, decrease in financial distress.

5.3 Pension Contributions

To evaluate the size of the effects on debt balances, we estimate regressions where the dependent variable is the cumulative contributions to the member's pension account (the sum of employee contributions, employer contributions, and tax relief).[28] A complication arises because individuals who leave their original staging firm are likely to move to a firm that does not use Nest as its pension provider, causing us to lose sight of their pension contributions after the move. Approximately 1.4% of remaining employees leave their original staging firm each month.

The black series in Figure 5 displays treatment effect estimates from Equation (1) where contributions are set to zero after individuals depart their original staging firm. Individuals are highly

[28] We include all contributions from June 2015 onwards, even if they occur before the employee's staging date and even if they are due to opt-in enrollment.

likely to move to another firm with automatic enrollment, so the black series underestimates pension contributions by an amount that increases with months post-enrollment as more people in our sample are no longer with their staging firm. The gray series in Figure 5 displays treatment effect estimates from Equation (1) when individuals are dropped from the regression after they leave their original staging firm. This series implicitly assumes that leavers have the same distribution of contribution rates after their departure as remainers. It probably overestimates contributions because our data do not distinguish between those who leave a firm and those who cease contributing to Nest by going through their employer instead of using the Nest website. Thus, the black and gray series constitute likely lower and upper bounds on automatic enrollment's cumulative contribution effect. Both series are close to linear in months post-enrollment, but the black series indicates that cumulative contributions increase at a slowly diminishing rate with months post-enrollment, whereas the gray series shows a slightly convex relationship.

We take two approaches to summarizing the cumulative contribution effects when estimating Equation (2). The first sets an individual's post-departure contributions to zero while using only the first 12 months post-enrollment for the estimation, since the size of the negative bias is relatively small in this region. The second uses all the available months post-enrollment but drops individuals from the regression after they depart their staging firm. Panels B and C of Table 8 show a per-month increase in cumulative contributions of £33-£39, of which £17-£20 are from the employer, £13-£16 are from the employee, and £3-£4 are from tax relief. (For completeness, Panel A shows estimates from setting post-departure contributions to zero and using all available months.)

Thus, 18 - 22% of the increase in total pension contributions induced by automatic enrollment is offset by increases in unsecured debt (£7.17 divided by £32.60 or £39.10). Alternatively, we can calculate the percentage of the reduction in take-home pay caused by automatic enrollment that is financed by increases in unsecured borrowing: 36-43% (£7.17 divided by £16.57 or £19.73). The extent to which the increases in mortgage origination caused by automatic enrollment (resulting in increased mortgage balances of £120 per month) crowds out net savings is unclear, since taking out a new mortgage is accompanied by the acquisition of an asset (the home).

5.4 Heterogeneity in Treatment Effects by Income, Credit Score, and Age

We might expect that the effects we estimate would vary with individuals' income, credit score, and age. Individuals with higher incomes are possibly less prone to exhibit a debt response, as they are more likely to have significant financial assets; in the absence of a spending reduction to finance new pension saving, their margin of adjustment would be reduced asset accumulation elsewhere. Individuals who have no access to credit due to a very poor credit score cannot increase their debt in response to automatic enrollment. On the other hand, those with low credit scores may tend to be less financially literate and attentive, which could cause the debt effect to be negatively related to credit scores. Finally, in the UK, pension funds can be withdrawn from age 55, which could increase older individuals' willingness to borrow in anticipation of being able to access their pension balances in the near future. Then again, older individuals are more likely to have significant financial assets, which would work in the opposite direction.

To investigate these treatment effect heterogeneities, we estimate variants of Equation (2) in which months post-enrollment is interacted with a dummy for having above-median income, credit score, or age as of November 2016. We also interact this dummy variable with the year, gender, and age fixed effects, which is necessary for the coefficient on the above-median dummy interaction with months post-enrollment to equal the difference between the treatment effect estimates when the unmodified Equation (2) specification is run separately on each half of the sample. We provide results for the three outcomes that seem most significantly affected by autoenrollment: non-revolving unsecured debt balances, whether the individual holds a mortgage, and pension contributions.[29]

Table 9 presents the results for non-revolving unsecured debt balances. Column (1) shows a marginally significant positive treatment interaction with the high-income dummy. Column (2) shows a significant negative coefficient on the treatment interaction with the high-credit-score dummy and a positive coefficient on the uninteracted months post-enrollment variable of almost the same magnitude as the interaction coefficient. This result indicates that the positive effect of months post-enrollment on non-revolving unsecured debt is absent for those with high credit scores. Column (3) shows a coefficient on the treatment interaction with age that is not statistically significant. Overall, these results indicate that the positive response of non-revolving unsecured debt is stronger among individuals with lower credit scores and higher income. The income interaction is surprising but should not be over-interpreted, as its statistical significance is weak.

Analogous results for the probability of holding a mortgage are shown in Table 10. The income and credit score interaction coefficients in Columns (1) and (2) are not statistically significant. The

[29]In Appendix Tables A13 - A18, we show results that interact the treatment dummy with percentile of income, credit score, or age; these are largely consistent with the binary interaction regressions.

coefficients in Column (3) indicate that the response in mortgage holding is higher among those of below-median age, and there is no mortgage response among those older than the median age.

Finally, treatment interactions for pension contributions are shown in Table 11. Here, the results indicate that the positive enrollment effect on contributions is higher for individuals with higher incomes, credit scores, and age. The increase in the effect on the pound sterling value of contributions with higher income arises mechanically from the default contribution rate being a fixed percent of qualifying earnings, and both age and credit score are positively correlated with income.

6 Discussion

Our empirical setting offers a natural experiment which allows us to precisely estimate effects on debt and pension contributions. Our setting does not lend itself well to testing between explanations for why these effects arise. We discuss a set of possible explanations here.

Automatic enrollment increasing unsecured debt might occur for at least three reasons. First, households could be choosing to increase their current consumption because of a wealth effect; all else being equal, individuals working at firms with earlier staging dates will receive more employer contributions and tax relief, and thus enjoy higher lifetime resources. Second, households might be incurring expenses associated with moving into a new home, since automatic enrollment increases the likelihood of having a mortgage. Third, households may be failing to reduce their spending to sufficiently finance their pension contributions due to inattention and inertia in their financial decision making. We should note that we do not observe non-pension savings and spending decisions by employees; automatic enrollment could potentially also lead to increases in non-pension assets, which would offset the increases in debt we observe.[30]

Automatic enrollment increasing the probability of having a mortgage might be explained by at least three mechanisms on employee demand. First, employees might interpret the offer of a pension by the firm as indicative of job security, which in turn causes households to take on expenditure commitments, such as mortgage payments (Chetty and Szeidl, 2007). It is, however, not clear why a household should interpret the employer's introduction of a legally mandated pension as signaling a reduction in unemployment risk. Second, the wealth effect arising from the commencement of the employer minimum pension contribution might induce households to

[30]Households commonly hold liquid assets and debt simultaneously (Morrison, 1998; Gross and Souleles, 2002).

purchase a home.[31] Third, employees may have a sequential view of how their personal finances should be managed. Beginning a pension contribution may prompt them to move on to the next stage of their economic life by purchasing a home. Indeed, the popular "how to" personal finance literature includes examples of authors recommending a sequential approach to personal finances whereby securing a pension precedes purchasing a home.[32]

On the supply side, mortgage providers may have taken the existence of a pension as a positive indication of creditworthiness in pre-autoenrollment models. Payslips, which are collected as proof of self-reported income and affordability, do often contain information about monthly employee deductions for pensions saving. However, many providers report not using information about pension contributions in affordability checks, and where they do use it, they typically interpret pension contributions as reducing the affordability of mortgage repayments. This would lead to a decrease in lending, not the increase we observe. Alternatively, lenders might become more willing to extend credit because they anticipate that pension assets will become available to service the debt. The fact that pension balances are not withdrawable until age 55, and the mortgage effects are weaker among older employees, argues against this mechanism.

The accumulation of unsecured debt and increased probability of having a mortgage may be related via a cross-selling channel. Individuals who are induced by automatic enrollment to originate an installment loan because their spending level is sticky might be targeted by the lender for mortgage marketing. Conversely, individuals induced by automatic enrollment to apply for a mortgage because they have a sequential approach to their personal finances might be cross-sold unsecured personal loans to fund home refurbishment or consumer durables. In our credit data, the probability of moving from not holding a mortgage to holding a mortgage between years is higher among those whose unsecured debt balances increased between years, compared with those whose unsecured debt balances did not increase.[33]

The effects we observe on credit score and the probability of default might occur via a number of channels. Mechanistically, credit score models tend to reward servicing a higher level of debt,

[31] Whether the magnitude of the response is sensible under standard economic models is a question for future investigation.

[32] For example, Suze Orman in her popular guide *The Money Book for the Young, Fabulous & Broke* writes, "I think that after you've maxed out on your company match in your 401(k), and after you have your credit card debt either paid off or declining, it's smart to focus on buying a home" (Orman, 2005). Other examples of popular authors suggested sequential approaches to personal finances include Ramsey (2002, 2013) and Warren and Tyagi (2006).

[33] Between the 2016 and 2017 credit file waves, the probability of moving from not holding a mortgage to holding a mortgage was 3.81% among individuals whose unsecured loan balances increased, and 2.51% among individuals whose unsecured loan balances did not increase (p-value of difference = 0.0001). Between 2017 and 2018, the equivalent percentages are 3.55% and 2.32% respectively (p-value of difference = 0.0001).

as this is evidence of reliability in repayment behavior. The modest decrease in defaults might be related to credit limit increases induced by improved credit scores. Alternatively, the reduction in default may occur because auto-enrolled households have a new margin through which they can obtain liquidity in an emergency: reducing their pension contributions. Each month, at least 0.095% of individuals who were contributing in the prior month and are still employed at the staging firm cease making pension contributions.[34] Auto-enrollment reduces the likelihood of an individual having defaulted on a debt any time in the last six years by 0.04 percentage points per month, so it is not quantitatively implausible that contribution cessations are responsible for a significant portion of the decrease in defaults. A more psychological explanation is that the sense of having attained a new stage of personal financial maturity may induce automatically enrolled individuals to put more effort into avoiding defaulting on debts.

The observation period in our study extends to 41 months post-enrollment for the earliest-enrolled cohort. Hence, we estimate short-to-medium-term effects on debt. We cannot draw inferences regarding the long-term impact of automatic enrollment on household indebtedness over the life-course until retirement. Within the range of our data, the borrowing effects appear to be nearly linear in time under enrollment, with little sign of leveling off, but of course debt balances cannot increase linearly forever. It may be the case that the long-term effects of automatic enrollment on debt differ from the short-term effects.

In tentatively assessing what the long-term impact of automatic enrollment might be, it is worth keeping in mind what the expected returns to pension assets are relative to consumer borrowing interest rates. In the five years ending in June 2023, the annualized return of the Nest 2040 Retirement Date Fund (which says it targets a return that is 3% above inflation) was 5.7%. In June 2018, examples of average non-revolving interest rates were 7.8% for a £5,000 personal loan, 3.8% for a £10,000 personal loan, 19.7% for an overdraft, and 4.0% for a two-year fixed-rate mortgage with a 95% loan-to-value ratio.[35] To the extent that automatic enrollment increases borrowing on more expensive forms of non-revolving unsecured debt, its positive effects on net worth arising from the employer contribution and tax relief will tend to erode over time due to the wedge between asset returns and the debt-servicing cost.

[34] This 0.095% does not include those who cease contributing via a request submitted through their employer, since we cannot distinguish these individuals from those who leave employment at the staging firm.

[35] Pension fund returns are drawn from https://www.nestpensions.org.uk/schemeweb/nest/investing-your-pension/fund-choices/compare-fund-performance.html. Interest rates are from the Bank of England (https://www.bankofengland.co.uk/statistics/visual-summaries/quoted-household-interest-rates).

7 Conclusion

In this paper, we examine whether automatic enrollment into retirement savings increases debt. We exploit a large-scale randomized roll-out of automatic enrollment among employers with 2–29 employees in the United Kingdom using linked pension and credit file data. We find that the additional pension savings generated through automatic enrollment are partially offset by increases in unsecured debt, particularly non-revolving unsecured debt such as overdrafts and personal loans. An additional month of enrollment yields on average a £33 - £39 contribution to the pension, while causing a £7 increase in unsecured debt. Automatic enrollment also increases the likelihood of the individual holding a mortgage, resulting in a £120 per month increase in mortgage balances. For unsecured debt, the increase in debt balances occurs at the intensive margin, with no effect seen on the likelihood of the individual borrowing on an unsecured debt product. Surprisingly, creditworthiness steadily increases with time under automatic enrollment; the prevalence of defaults on debt decreases and credit scores increase. Overall, our study shows that automatic enrollment has complex effects across different facets of the household balance sheet.

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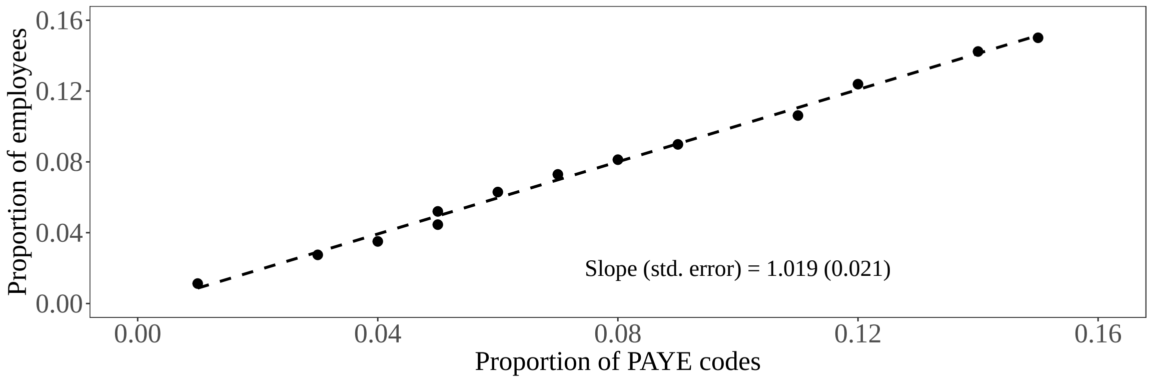
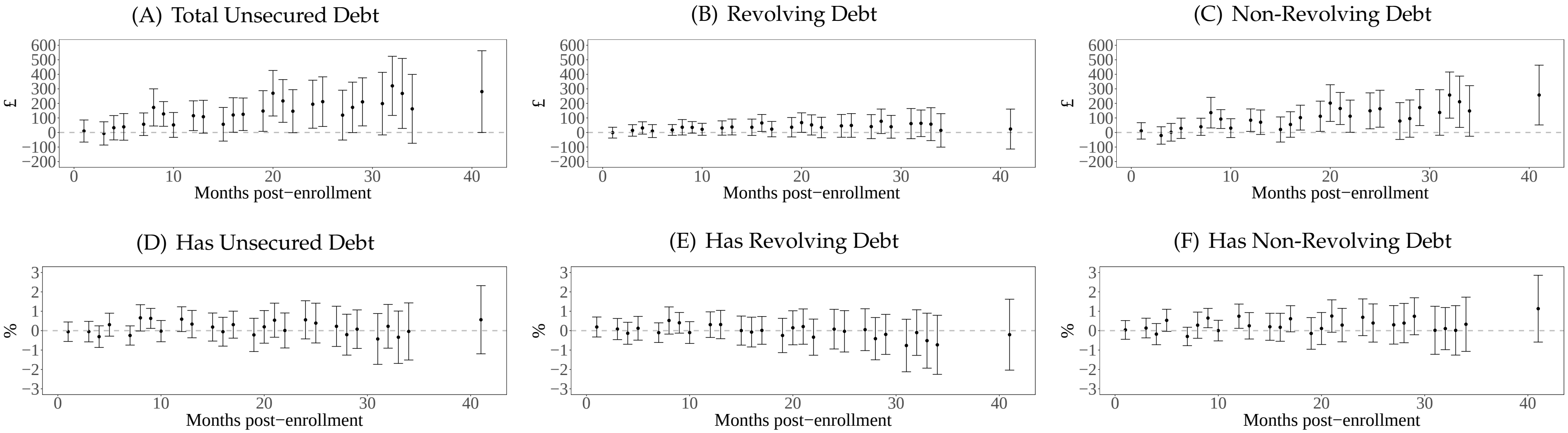
Figure 1: Proportion of Employees vs. Proportion of PAYE Codes, by Staging Date



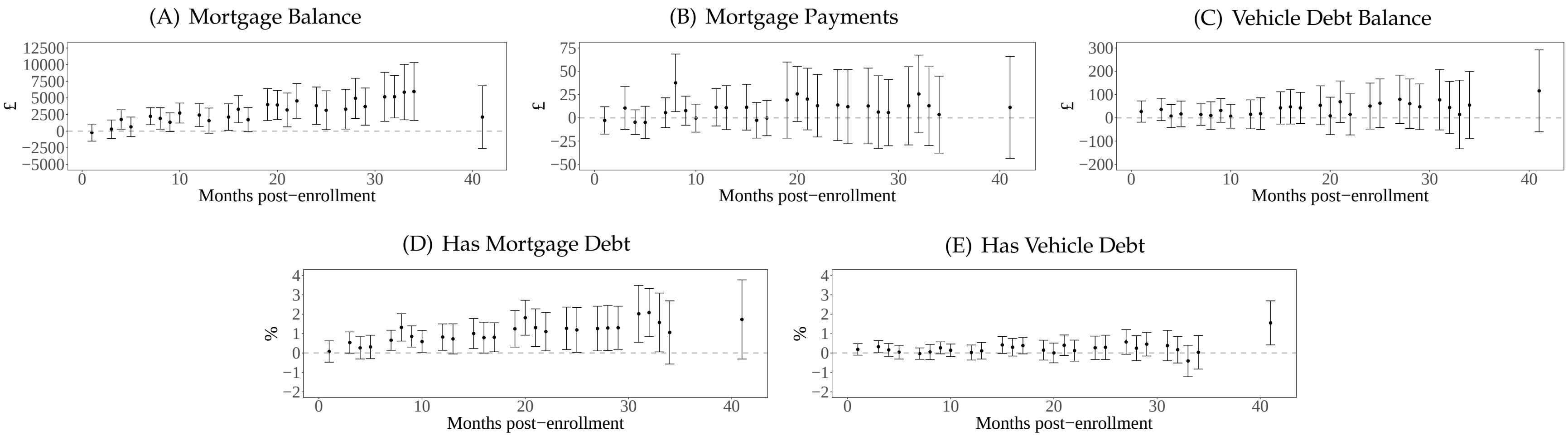
Notes: Each dot on the plot represents a staging date, with the y-axis showing the proportion of the baseline sample's employees assigned to that staging date, and the x-axis showing the proportion of final two PAYE digits assigned to that staging date. The dashed line is the best-fit trendline.

Figure 2: Automatic Enrollment Treatment Effects on Unsecured Debt



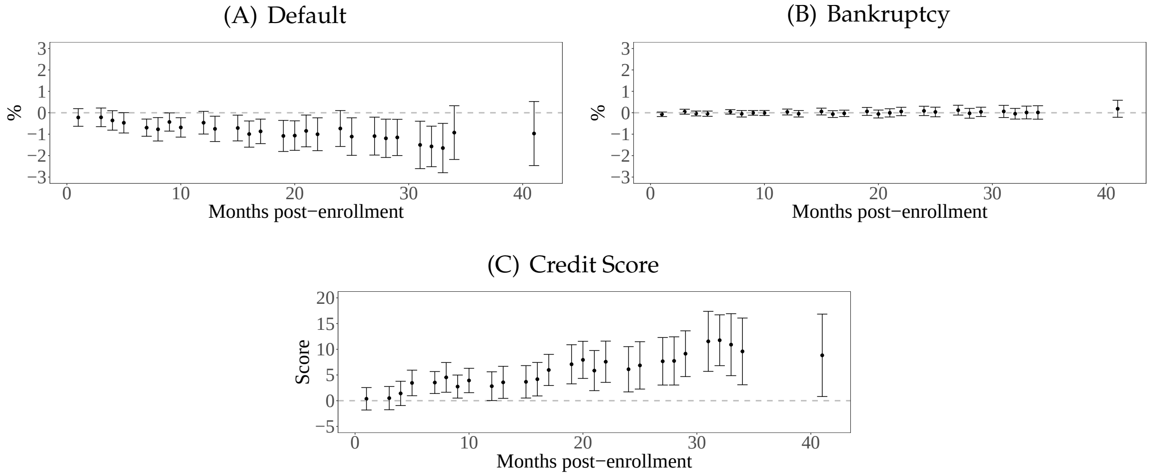
Notes: These graphs plot coefficients and 95% confidence intervals from estimates of Equation (1), where the dependent variable is in each graph's title. The rightmost coefficient is estimated from the June 2015 test trial cohort, which includes only a small number of employers. The estimation sample is the baseline sample. Coefficient and standard error values are reported in Table A9.

Figure 3: Automatic Enrollment Treatment Effects on Secured Debt



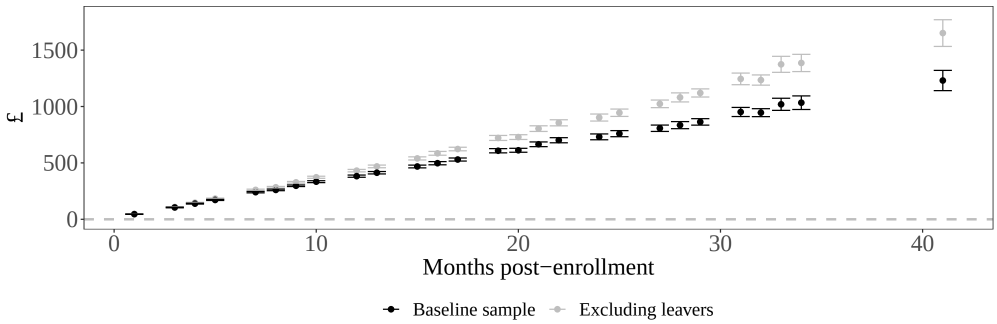
Notes: These graphs plot coefficients and 95% confidence intervals from estimates of Equation (1), where the dependent variable is in each graph's title. The rightmost coefficient is estimated from the June 2015 test trial cohort, which includes only a small number of employers. The estimation sample is the baseline sample. Coefficient and standard error values are reported in Table A10.

Figure 4: Automatic Enrollment Treatment Effects on Default, Bankruptcy, Credit Score



Notes: These graphs plot coefficients and 95% confidence intervals from estimates of Equation (1), where the dependent variable is an indicator for having defaulted on a debt in the previous six years, an indicator for having declared bankruptcy in the previous six years, or credit score. The rightmost coefficient is estimated from the June 2015 test trial cohort, which includes only a small number of employers. The estimation sample is the baseline sample. Coefficient and standard error values are reported in Table A11.

Figure 5: Automatic Enrollment Treatment Effects on Cumulative Pension Contributions



Notes: This graph plots coefficients and 95% confidence intervals from estimates of Equation (1), where the dependent variable is cumulative Nest pension contributions. The rightmost coefficient is estimated from the June 2015 test trial cohort, which includes only a small number of employers. The estimation sample is either the baseline sample, with contributions set to zero after individuals leave their original staging firm, or the baseline sample excluding individuals once they leave their original staging firm. Coefficient and standard error values are reported in Table A12.

Table 1: Staging Dates for Employers with 2-29 Employees

Final two PAYE digits	Staging date
92*	Jun 1, 2015
02-04	Jan 1, 2016
00, 05-07	Feb 1, 2016
01, 08-11	Mar 1, 2016
12-16	Apr 1, 2016
17-22	Jun 1, 2016
23-29	Jul 1, 2016
30-37	Aug 1, 2016
38-46	Oct 1, 2016
47-57	Nov 1, 2016
58-69	Jan 1, 2017
70-83	Feb 1, 2017
84-91, 93-99	Apr 1, 2017

Notes: This table shows how the final two digits of the employer's PAYE code map to its TPR-assigned staging date.
* A small test trial cohort of firms staged early in June 2015.

Table 2: Baseline Sample Summary Statistics

	Miss (%)	>0 (%)	Mean	SD	p25	p50	p75	p90
Age	0.0		42.5	11.3	32.3	42.2	51.8	58.1
Female (%)	0.0		40.9					
Monthly contribution (£)	0.0	78.0	29.58	79.54	6.47	22.01	38.47	59.46
Credit score	4.8		934.8	183.5	849.0	1,004.0	1,044.0	1,101.0
Income (£)	1.4	98.6	35,916	17,680	24,849	29,889	41,400	56,250
Bankrupt (%)	1.4		1.4					
Default (%)	1.4		16.2					
Secured Debt								
Mortgage (£)	0.0	37.5	52,095	118,622	0.00	0.00	74,645	162,432
Mortgage payments (£)	0.0	37.6	305	1,981	0.00	0.00	444	843
Vehicle debt (£)	0.0	11.3	1,253	5,249	0.00	0.00	0.00	2,339
Unsecured Debt								
Total (£)	0.0	67.2	3,836	8,217	0.00	482	4,004	12,195
of which...								
Revolving (£)	0.0	55.4	1,630	4,006	0.00	67.00	1,380	4,757
Non-revolving (£)	0.0	39.5	2,206	6,443	0.00	0.00	919	7,749
of which...								
Overdraft (£)	0.0	14.6	193	2,900	0.00	0.00	0.00	201
Unsecured loans (£)	0.0	26.3	1,507	4,306	0.00	0.00	91.00	5,668
Sales agreements (£)	0.0	5.6	82.70	712	0.00	0.00	0.00	0.00
Other (£)	0.0	4.2	423	3,483	0.00	0.00	0.00	0.00
Overdraft CLU (%)	0.0		7.6	26.5	0.0	0.0	0.0	25.0
Revolving CLU (%)	0.0		18.2	31.0	0.0	0.0	24.0	75.0
N=649,747								

Notes: This table shows summary statistics as of November 2017 for individuals in the baseline sample. CLU denotes credit limit utilization. "Miss" refers to the percentage of observations for which a value is not provided in the data. The next column records the percentage of non-missing observations with a value greater than zero. Subsequent columns show the mean, standard deviation (SD), 25th percentile value (p25), median (p50), 75th percentile value (p75), and 90th percentile value (p90) calculated from all non-missing observations.

Table 3: Employee Characteristic Balance Tests

	Mean			Equality of means
	Jan-2017	Feb-2017	Apr-2017	p-value
Age (Years)	41.4 (N=80,156)	41.5 (N=91,832)	41.3 (N=96,455)	0.087
Female (%)	40.8 (N=80,156)	40.3 (N=91,832)	41.2 (N=96,455)	0.184
Credit Score	930 (N=76,063)	929 (N=87,245)	928 (N=91,293)	0.624
Income (£)	35,300 (N=79,159)	35,200 (N=90,636)	35,300 (N=95,202)	0.751
Bankrupt (%)	1.38 (N=79,159)	1.43 (N=90,636)	1.44 (N=95,202)	0.878
Default (%)	16.5 (N=79,159)	16.9 (N=90,636)	16.7 (N=95,202)	0.434
Mortgage (£)	49,100 (N=80,156)	48,300 (N=91,832)	49,000 (N=96,455)	0.701
Mortgage payments (£)	293 (N=80,156)	293 (N=91,832)	281 (N=96,455)	0.704
Vehicle debt (£)	1,100 (N=80,156)	1,120 (N=91,832)	1,090 (N=96,455)	0.756
Unsecured Debt (£) *of which*	3,490 (N=80,156)	3,600 (N=91,832)	3,530 (N=96,455)	0.369
Revolving (£)	1,510 (N=80,156)	1,510 (N=91,832)	1,510 (N=96,455)	1.000
Non Revolving (£)	1,990 (N=80,156)	2,090 (N=91,832)	2,020 (N=96,455)	0.201

Notes: This table presents balance tests for means of individual characteristics across individuals with firm staging dates of January, February, and April 2017. Columns (1) to (3) show the means for the January, February, and April 2017 cohorts. N is the number of observations used to calculate the corresponding mean. Column (4) shows the *p*-values for tests of joint equality of the three cohort means, using robust standard errors clustered by employer. The means come from Experian's November 2016 credit data. Monetary values are in November 2016 pounds (£).

Table 4: Months Post-Enrollment Counts in Each Data Extract

Months post-enrollment	Year of observation		
	2016	2017	2018
0	337,184	0	0
1	58,470	0	0
3	52,923	0	0
4	47,619	0	0
5	41,179	0	0
7	34,190	96,455	0
8	29,335	0	0
9	23,166	91,832	0
10	18,152	80,156	0
12	0	68,741	0
13	0	58,470	0
15	0	52,923	0
16	0	47,619	0
17	7,529	41,179	0
19	0	34,190	96,455
20	0	29,335	0
21	0	23,166	91,832
22	0	18,152	80,156
24	0	0	68,741
25	0	0	58,470
27	0	0	52,923
28	0	0	47,619
29	0	7,529	41,179
31	0	0	34,190
32	0	0	29,335
33	0	0	23,166
34	0	0	18,152
41	0	0	7,529

Notes: This table shows the number of individuals that have each value of months post-enrollment at each date of observation.

Table 5: Treatment Effects on Unsecured Debt

	Debt balance			Has debt balance x 100		
	Total (£) (1)	Revolving (£) (2)	Non-revolving (£) (3)	Total (4)	Revolving (5)	Non-revolving (6)
Months post-enrollment	7.172***	1.616	5.556***	0.0025	-0.0109	0.0163
	(2.545)	(1.196)	(1.907)	(0.0148)	(0.0154)	(0.0141)
Observations	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241
Year fixed effects	✓	✓	✓	✓	✓	✓
Gender fixed effects	✓	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for unsecured debt outcomes, controlling for observation year, gender, and age fixed effects. In Columns (1) - (3), the dependent variable is debt balances (total unsecured, revolving unsecured, or non-revolving unsecured). In Columns (4) - (6), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table 6: Treatment Effects on Secured Debt

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£) (1)	Payments (£) (2)	Balance (£) (3)	Payments (4)	Balance (5)
Months post-enrollment	120.1***	0.2586	1.446	0.0474***	0.0117
	(42.46)	(0.4716)	(1.471)	(0.0169)	(0.0091)
Observations	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241
Year fixed effects	✓	✓	✓	✓	✓
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table 7: Treatment Effects on
Bankruptcy, Default, Credit Score

	Default x 100 (1)	Bankruptcy x 100 (2)	Credit score (3)
Months post-enrollment	-0.0388***	-0.0008	0.3121***
	(0.0127)	(0.0032)	(0.0669)
Observations	1,922,571	1,922,571	1,855,316
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for creditworthiness outcomes, controlling for observation year, gender, and age fixed effects. The dependent variable is 100 multiplied by a binary indicator for filing for bankruptcy within the past six years, 100 multiplied by a binary indicator for defaulting on a debt within the past six years, or credit score. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table 8: Treatment Effects on Cumulative Pension Contributions

A. ALL

	Total (£) (1)	Employer (£) (2)	Employee (£) (3)	Tax relief (£) (4)
Months post-enrollment	29.32***	14.95***	11.53***	2.870***
	(0.4712)	(0.2963)	(0.1990)	(0.0499)
Observations	1,949,241	1,949,241	1,949,241	1,949,241
Year fixed effects	✓	✓	✓	✓
Gender fixed effects	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓

B. MONTHS POST-ENROLLMENT UP TO 12

	Total (£) (1)	Employer (£) (2)	Employee (£) (3)	Tax relief (£) (4)
Months post-enrollment	32.60***	16.57***	12.83***	3.198***
	(0.2523)	(0.1794)	(0.0949)	(0.0237)
Observations	979,402	979,402	979,402	979,402
Year fixed effects	✓	✓	✓	✓
Gender fixed effects	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓

C. EXCLUDING LEAVERS AS THEY GO

	Total (£) (1)	Employer (£) (2)	Employee (£) (3)	Tax relief (£) (4)
Months post-enrollment	39.10***	19.73***	15.54***	3.873***
	(0.5431)	(0.3487)	(0.2322)	(0.0581)
Observations	1,567,635	1,567,635	1,567,635	1,567,635
Year fixed effects	✓	✓	✓	✓
Gender fixed effects	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for cumulative Nest pension contributions, controlling for observation year, gender, and age fixed effects. The dependent variable is cumulative total contributions, cumulative employer contributions, cumulative employee contributions, or cumulative tax relief deposited into the pension. The sample is either the baseline sample (Panel A), the baseline sample up to 12 months post-enrollment (Panel B), or the baseline sample excluding individuals once they leave their original staging firm (Panel C). Incremental contributions of employees in the baseline sample are set to zero after they leave their original staging firm. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table 9: Interactions: Non-Revolving Unsecured Debt

	Unsecured debt balances		
	Non-revolving (£)		
	(1)	(2)	(3)
Months post-enrollment	1.592	12.32***	5.150**
	(1.515)	(3.346)	(2.221)
Months post-enrollment × (Income > median)	6.081*		
	(3.571)		
Months post-enrollment × (Credit Score > median)		-11.80***	
		(3.736)	
Months post-enrollment × (Age > median)			0.0102
			(3.615)
Observations	1,922,646	1,850,127	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	
(Income > median)-Year fixed effects	✓		
(Income > median)-Gender fixed effects	✓		
(Income > median)-Age fixed effects	✓		
(Credit Score > median)-Year fixed effects		✓	
(Credit Score > median)-Gender fixed effects		✓	
(Credit Score > median)-Age fixed effects		✓	
(Age > median)-Year fixed effects			✓
(Age > median)-Gender fixed effects			✓

Notes: This table shows regression estimates of Equation (2) modified to include a binary indicator for having above-median income, credit score, or age, and the interaction of that indicator with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of the above-median dummy with these fixed effects. The dependent variable is non-revolving unsecured debt balances. The sample is the baseline sample. Standard errors are robust and clustered at employer level. *Significant at 10% level; **5% level; ***1% level.

Table 10: Interactions: Has Mortgage Debt

	Has mortgage x 100		
	(1)	(2)	(3)
Months post-enrollment	0.0321**	0.0403**	0.0789***
	(0.0148)	(0.0199)	(0.0228)
Months post-enrollment × (Income > median)	-0.0171		
	(0.0237)		
Months post-enrollment × (Credit Score > median)		-0.0266	
		(0.0260)	
Months post-enrollment × (Age > median)			-0.0893***
			(0.0286)
Observations	1,922,646	1,850,127	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	
(Income > median)-Year fixed effects	✓		
(Income > median)-Gender fixed effects	✓		
(Income > median)-Age fixed effects	✓		
(Credit Score > median)-Year fixed effects		✓	
(Credit Score > median)-Gender fixed effects		✓	
(Credit Score > median)-Age fixed effects		✓	
(Age > median)-Year fixed effects			✓
(Age > median)-Gender fixed effects			✓

Notes: This table shows regression estimates of Equation (2) modified to include a binary indicator for having above-median income, credit score, or age, and the interaction of that indicator with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of the above-median dummy with these fixed effects. The dependent variable is 100 multiplied by a binary indicator for having mortgage debt. The sample is the baseline sample. Standard errors are robust and clustered at employer level. *Significant at 10% level; **5% level; ***1% level.

Table 11: Interactions: Cumulative Pension Contributions

A. MONTHS POST-ENROLLMENT UP TO 12

	Cumulative contributions (£)		
	(1)	(2)	(3)
Months post-enrollment	27.27***	29.17***	30.46***
	(0.2131)	(0.2203)	(0.2650)
Months post-enrollment × (Income > median)	10.70***		
	(0.3236)		
Months post-enrollment × (Credit Score > median)		7.539***	
		(0.3147)	
Months post-enrollment × (Age > median)			4.403***
			(0.3212)
Observations	966,294	929,472	979,402
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	
(Income > median)-Year fixed effects	✓		
(Income > median)-Gender fixed effects	✓		
(Income > median)-Age fixed effects	✓		
(Credit Score > median)-Year fixed effects		✓	
(Credit Score > median)-Gender fixed effects		✓	
(Credit Score > median)-Age fixed effects		✓	
(Age > median)-Year fixed effects			✓
(Age > median)-Gender fixed effects			✓

B. EXCLUDING LEAVERS AS THEY GO

	(1)	(2)	(3)
Months post-enrollment	33.05***	35.71***	37.85***
	(0.5039)	(0.5146)	(0.6203)
Months post-enrollment × (Income > median)	11.66***		
	(0.7372)		
Months post-enrollment × (Credit Score > median)		7.259***	
		(0.7014)	
Months post-enrollment × (Age > median)			2.849***
			(0.7641)
Observations	1,545,783	1,493,512	1,566,094
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	
(Income > median)-Year fixed effects	✓		
(Income > median)-Gender fixed effects	✓		
(Income > median)-Age fixed effects	✓		
(Credit Score > median)-Year fixed effects		✓	
(Credit Score > median)-Gender fixed effects		✓	
(Credit Score > median)-Age fixed effects		✓	
(Age > median)-Year fixed effects			✓
(Age > median)-Gender fixed effects			✓

Notes: This table shows regression estimates of Equation (2) modified to include a binary indicator for having above-median income, credit score, or age, and the interaction of that indicator with months post-enrollment. The regressions also control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and their interaction with the above-median dummy. The dependent variable is cumulative Nest contributions. The sample is the baseline sample up to 12 months post-enrollment, with contributions set to zero after employees leave their original staging firm (Panel A), or the baseline sample excluding individuals once they leave their original staging firm (Panel B). Standard errors are robust and clustered at employer level. *Significant at 10% level; **5% level; ***1% level.

A Appendix Materials

A.1 Assignment of Employer PAYE Reference Numbers

Our empirical design exploits the staggered rollout of automatic enrollment across firms. Staging dates were assigned to firms based upon the final two digits of their PAYE reference number. These numbers are assigned to firms in a quasi-random manner, which we explain in this section.[36]

Employer Pay-As-You-Earn (PAYE) references are a unique reference for each firm in the UK used by His Majesty's Revenue and Customs (HMRC) to identify an employer for the purposes of employment reporting and compliance. References are combinations of numbers and letters assigned at firm birth.[37]

Since April 2000, PAYE references take the format of 3 numbers, followed by two letters, followed by 5 numbers, e.g. 123/AB45678. The first 3 numbers correspond to a HMRC office number. The remaining letters are assigned in sequence by incrementing the first letter until it reaches a value Z, at which point the final number is incremented and the initial letter is reset back to A, and so on. For example, the first firm to register is assigned AA00001, the next firm BA00001, and so on, until the series reaches ZA00001, at which point the series continues to AA00002, BA00002, etc. Only 20 of the 26 letters in the alphabet are used. The last two digits therefore increment after each 20 firms register. When the final number reaches 99999, the second letter is incremented, the first letter goes back to A, and the final number is reset to 00001. Hence, the sequence would be ZA99999, AB00001, BB00001...

Staging dates were assigned to firms based upon the final two digits of this PAYE reference. If the number of registrations were low, there could potentially be economically meaningful correlations between PAYE digits and seasonalities.[38] Given the very large number of newly registered employers in any given year (in 2016, approximately 414,000, or 1,636 per day), the last two digits change approximately 81 times per day.[39] Because the system cycles through the complete set of

[36]We are grateful to Jonathan Cribb and Carl Emmerson from the Institute for Fiscal Studies for sharing detailed information regarding the allocation of employer PAYE reference numbers which is also available in Cribb and Emmerson (2021).

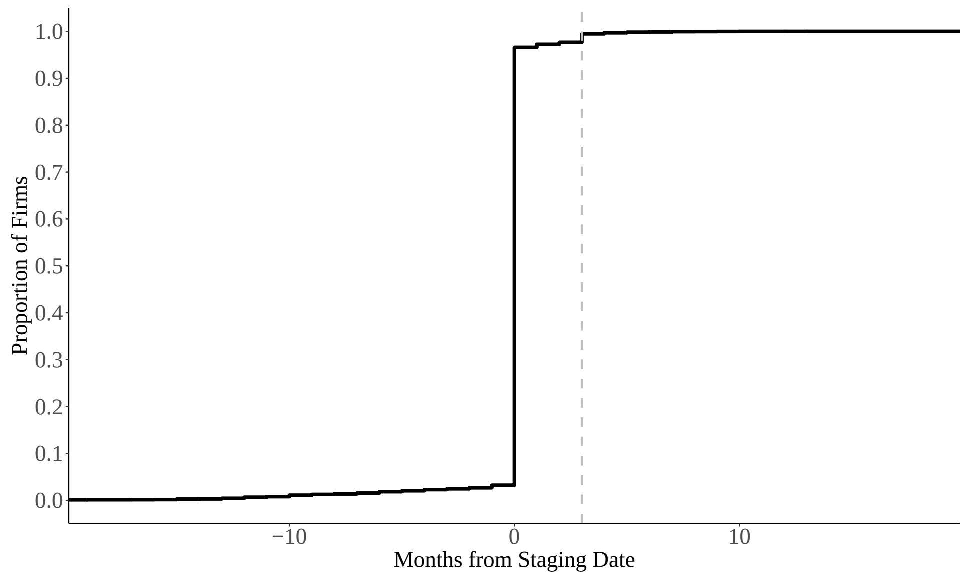
[37]For the purposes of PAYE, the term employer means any person who pays earnings, that is, "an individual or business which employs workers." See here: https://www.gov.uk/hmrc-internal-manuals/paye-manual/paye055#IDAQIS5H

[38]For example, if only 20 firms registered each month, then the last two digits would correlate with months of the year. There may be important month-on-month differences in registering firm characteristics, and hence the last two digits would not be orthogonal to relevant variables.

[39]Dividing 414,000 employers by 253 business days provides this average of 1,636 newly-registered employers per day. With the final two digits changing every 20 registrations, this implies on average, a given day would see the use of 81 unique two-number endings to the PAYE code.

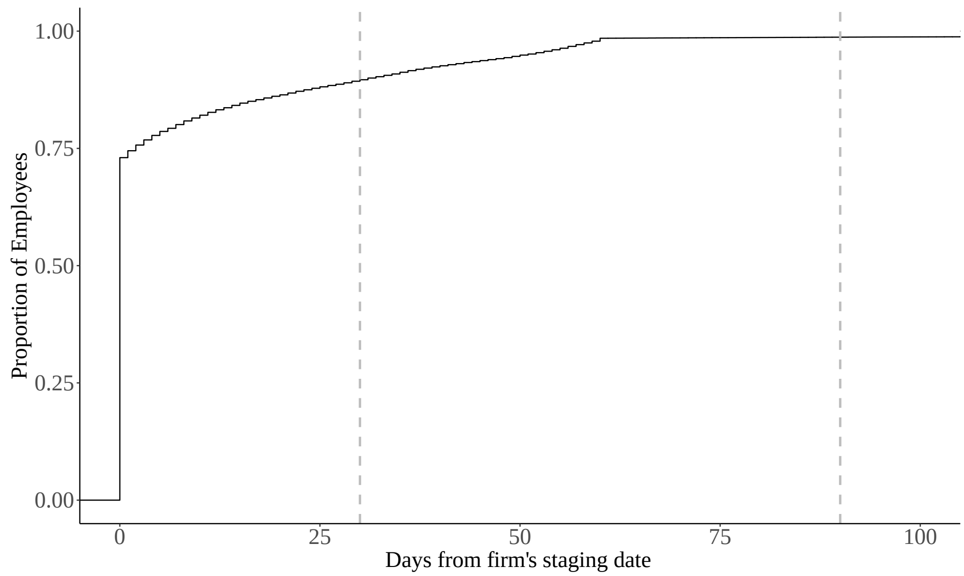
last two digits of the employer PAYE reference almost daily, we are confident that the assignment of the last two digits generates no economically relevant sequence patterns in the data. Therefore, we conclude that these digits are as good as randomly assigned, and hence regard the TPR's assignment of employers to staging dates based on the last two digits of employer PAYE references to be as-good-as-random.

Figure A1: Firm-Reported Staging Date vs TPR-Assigned Staging Date



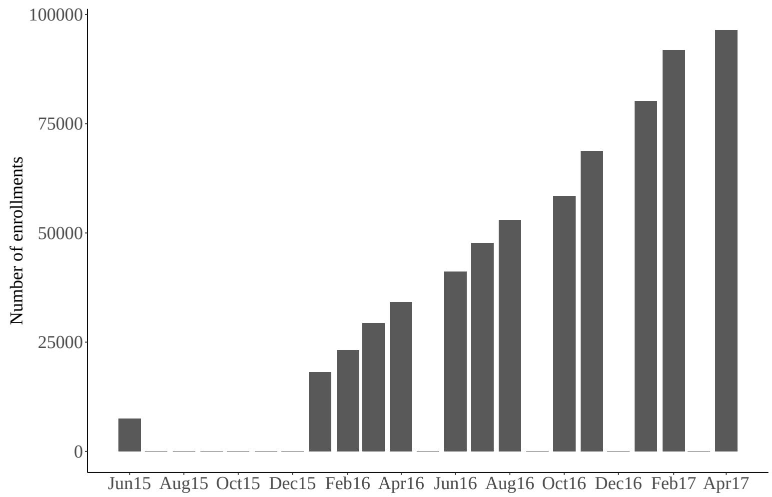
Notes: This figure illustrates the cumulative density function of the time difference (in months) between the firm's self-reported staging date and its TPR-assigned staging date as provided by TPR. A negative value on the x-axis indicates the firm staged early. The dashed vertical line indicates the end of the three-month window within which firms were obliged to enroll all eligible employees. The sample is the TPR-matched sample, for which summary statistics are shown in Table A8.

Figure A2: Member Enrollment Date vs Firm-Reported Staging Date



Notes: This figure illustrates the cumulative density function of the time difference (in days) between the firm's self-reported staging date and the date on which its employees were enrolled into the Nest scheme. A value of zero indicates the employee was enrolled on the staging date. A positive value indicates the employee was enrolled after the staging date. Dashed vertical lines mark 30 and 90 days after the staging date. Firms were obliged to enroll all their employees by 90 days after the staging date. The sample is the baseline sample.

Figure A3: Enrollments by Calendar Month



Notes: This figure illustrates the number of employees enrolled in each staging date cohort.

Table A1: Staging Dates for Employers with 30+ Employees

Number of employees	Staging date
120,000 or more	Oct 1, 2012
50,000 - 119,999	Nov 1, 2012
30,000 - 49,999	Jan 1, 2013
20,000 - 29,999	Feb 1, 2013
10,000 - 19,999	Mar 1, 2013
6,000 - 9,999	Apr 1, 2013
4,100 - 5,999	May 1, 2013
4,000 - 4,099	Jun 1, 2013
3,000 - 3,999	Jul 1, 2013
2,000 - 2,999	Aug 1, 2013
1,250 - 1,999	Sep 1, 2013
800 - 1,249	Oct 1, 2013
500 - 799	Nov 1, 2013
350 - 499	Jan 1, 2014
250 - 349	Feb 1, 2014
160 - 249	Apr 1, 2014
90 - 159	May 1, 2014
62 - 89	Jul 1, 2014
61	Aug 1, 2014
60	Oct 1, 2014
59	Nov 1, 2014
58	Jan 1, 2015
54 - 57	Mar 1, 2015
50 - 53	Apr 1, 2015
40 - 49	Aug 1, 2015
30 - 39	Oct 1, 2015

Notes: This table shows how the number of employees at the firm on April 1, 2012 maps to its staging date.

Table A2: Opt-Out Rates by Age

Age band	Opt-out rate (%)	Number of optouts
Under 30	7.62	14,761
30-39	10.13	20,139
40-49	14.34	29,506
50-59	20.01	38,311
60 and above	31.41	15,680

Notes: Opt-out computations use only the universe of employees eligible for automatic enrollment who were auto-enrolled, "voluntarily" enrolled before or up to three months after their staging date, or opted out. (Other enrollment types are typically for workers who are not eligible for auto-enrollment because of their age or income.) For individuals who worked for more than one sample firm, we consider only their choice for their earliest-staging firm. If an individual worked for more than one sample firm with the same staging date, we randomly select one of those firms for the purposes of this computation. Member age is measured as of November 2016.

Table A3: Opt-Out Rates by Staging Date

Staging date	Opt-out rate (%)
June 2015	15.95
January 2016	14.90
February 2016	15.48
March 2016	15.64
April 2016	15.04
June 2016	14.20
July 2016	13.74
August 2016	13.80
October 2016	13.91
November 2016	13.92
January 2017	14.48
February 2017	13.96
April 2017	12.98

Notes: Opt-out computations use only the universe of employees eligible for automatic enrollment who were auto-enrolled, "voluntarily" enrolled before or up to three months after their staging date, or opted out. (Other enrollment types are typically for workers who are not eligible for auto-enrollment because of their age or income.) For individuals who worked for more than one sample firm, we consider only their choice for their earliest-staging firm. If an individual worked for more than one sample firm with the same staging date, we randomly select one of those firms for the purposes of this computation.

Table A4: Sample Selection

Step	Employees	Employees	Firms	Firms
	(N)	(%)	(N)	(%)
Starting sample of Nest employee records	712,818	100	173,570	100
1. Drop if firm PAYE reference pre-2000	707,472	99.25	167,219	96.34
2. Keep if matched credit file	686,359	96.29	165,559	95.38
3. Drop if not TPR-approved staging date	677,816	95.09	163,721	94.33
4. Keep those who are age eligible	649,747	91.15	161,707	93.17
Baseline sample	649,747		161,707	

Notes: The table shows the steps in our sample selection and how they affect our sample size. Step 4 keeps only those individuals who, because of their birth date, are eligible to be auto-enrolled at any firm in the sample (for men, those aged under 65 on the final staging date; for women, those aged under 63 years and 9 months on the final staging date; for men and women, those aged at least 22 on the first staging date). The starting sample size of Nest employee records is used as the denominator when computing percentages. The baseline sample is the main sample used in our analysis.

Table A5: Credit File Non-Matching Rate Balance Test

Staging date	Unmatched (%)	N
June 2015	3.05	7,894
January 2016	2.84	19,177
February 2016	3.28	24,587
March 2016	3.20	31,207
April 2016	3.00	36,364
June 2016	3.11	44,054
July 2016	2.89	50,917
August 2016	2.86	56,698
October 2016	2.97	62,795
November 2016	2.87	74,103
January 2017	2.93	86,522
February 2017	3.06	99,532
April 2017	2.94	104,821
	p-value = 0.882	

Notes: The table shows the proportion of individuals for whom a credit file match was not achieved in Step 2 of Table A4, by staging date. Individuals whose employer-reported staging date is not a TPR-approved staging date listed in Table 1 are excluded from the table. The *p*-value is for a test of joint equality of these unmatched percentages across all the staging dates.

Table A6: Age and Gender Balance Test

Staging date	Age	Female (%)	N
June 2015	42.55	41.03	7,529
January 2016	42.51	41.48	18,152
February 2016	42.52	41.10	23,166
March 2016	42.54	41.06	29,335
April 2016	42.58	40.68	34,190
June 2016	42.45	41.11	41,179
July 2016	42.57	41.01	47,619
August 2016	42.50	39.87	52,923
October 2016	42.57	41.21	58,470
November 2016	42.56	41.91	68,741
January 2017	42.41	40.82	80,156
February 2017	42.48	40.34	91,832
April 2017	42.26	41.17	96,455
	p-value = 0.309	p-value = 0.102	

Notes: The table shows mean age at November 2017 and the percentage of female individuals for each staging date. The *p*-values are for tests of joint equality of mean age or percent female across all the staging dates.

Table A7: Baseline Sample Summary Statistics, November 2018 Credit File

	Miss (%)	>0 (%)	Mean	SD	p25	p50	p75	p90
Age	0.0		43.5	11.3	33.3	43.2	52.8	59.1
Female (%)	0.0		40.9					
Monthly contribution (£)	0.0	67.7	57.66	163	0.00	46.49	90.00	136
Credit score	4.6		938.2	182.0	852.0	1,007.0	1,044.0	1,101.0
Income (£)	1.4	98.6	36,644	18,201	25,217	30,313	42,399	57,835
Bankrupt (%)	1.4		1.5					
Default (%)	1.4		16.0					
Secured Debt								
Mortgage (£)	0.0	38.1	54,057	118,859	0.00	0.00	78,178	169,774
Mortgage payments (£)	0.0	38.2	309	1,933	0.00	0.00	463	860
Vehicle debt (£)	0.0	11.8	1,353	5,520	0.00	0.00	0.00	2,999
Unsecured Debt								
Total (£)	0.0	68.1	4,085	8,463	0.00	569	4,429	12,953
of which...								
Revolving (£)	0.0	56.5	1,733	4,107	0.00	90.00	1,560	5,116
Non-revolving (£)	0.0	40.5	2,352	6,620	0.00	0.00	1,079	8,292
of which...								
Overdraft (£)	0.0	13.9	167	2,584	0.00	0.00	0.00	172
Unsecured loans (£)	0.0	27.4	1,647	4,581	0.00	0.00	194	6,189
Sales agreements (£)	0.0	5.5	84.28	718	0.00	0.00	0.00	0.00
Other (£)	0.0	5.8	454	3,696	0.00	0.00	0.00	0.00
Overdraft CLU (%)	0.0		7.2	25.1	0.0	0.0	0.0	20.0
Revolving CLU (%)	0.0		18.5	30.8	0.0	0.0	25.0	75.0
N=649,747								

Notes: This table shows summary statistics as of November 2018 for individuals in the baseline sample. CLU denotes credit limit utilization. "Miss" refers to the percentage of observations for which a value is not provided in the data. The next column records the percentage of non-missing observations with a value greater than zero. Subsequent columns show the mean, standard deviation (SD), 25th percentile value (p25), median, 75th percentile value (p75), and 90th percentile value (p90) calculated from all non-missing observations.

Table A8: Summary Statistics, TPR-Matched Sample

	Miss (%)	>0 (%)	Mean	SD	p25	p50	p75	p90
Age	0.0		42.3	11.2	32.2	42.0	51.6	57.9
Female (%)	0.0		41.5					
Monthly contribution (£)	0.0	76.9	28.52	77.51	4.18	21.00	37.43	58.21
Credit score	5.3		936.4	181.8	849.0	1,004.0	1,044.0	1,101.0
Income (£)	1.5	98.5	36,271	18,321	24,867	29,913	41,730	57,485
Bankrupt (%)	1.5		1.3					
Default (%)	1.5		15.9					
Secured Debt								
Mortgage (£)	0.0	37.3	54,643	134,201	0.00	0.00	75,639	167,590
Mortgage payments (£)	0.0	37.5	320	2,824	0.00	0.00	447	864
Vehicle debt (£)	0.0	11.2	1,283	5,417	0.00	0.00	0.00	2,253
Unsecured Debt								
Total (£)	0.0	66.4	3,864	8,506	0.00	451	3,975	12,209
of which...								
Revolving (£)	0.0	54.7	1,662	4,132	0.00	58.00	1,385	4,850
Non-revolving (£)	0.0	39.0	2,201	6,698	0.00	0.00	868	7,596
of which...								
Overdraft (£)	0.0	14.5	191	3,327	0.00	0.00	0.00	207
Unsecured loans (£)	0.0	25.8	1,492	4,334	0.00	0.00	60.00	5,539
Sales agreements (£)	0.0	5.5	82.40	719	0.00	0.00	0.00	0.00
Other (£)	0.0	4.1	436	3,497	0.00	0.00	0.00	0.00
Overdraft CLU (%)	0.0		7.6	27.6	0.0	0.0	0.0	25.0
Revolving CLU (%)	0.0		18.0	30.6	0.0	0.0	24.0	74.0
N=160,175								

Notes: This table shows summary statistics as of November 2017 for individuals in the TPR-matched sample, which consists of firms that reported their PAYE number to Nest. "Miss" refers to the percentage of observations for which a value is not provided in the data. The next column records the percentage of observations with a value greater than zero. Subsequent columns show the mean, standard deviation (SD), 25th percentile value (p25), median, 75th percentile value (p75), and 90th percentile value (p90) calculated from all non-missing observations.

Table A9: Months Post-Enrollment (MPE) Treatment Effect Estimates: Unsecured Debt

	Total (£) (1)	Revolving (£) (2)	Non-revolving (£) (3)	Has debt x 100 (4)	Has revolving x 100 (5)	Has non-revolving x 100 (6)
1 MPE	10.07	-1.055	11.13	-0.0543	0.1886	0.0376
	(38.74)	(18.90)	(28.75)	(0.2561)	(0.2632)	(0.2471)
3 MPE	-6.484	14.11	-20.59	-0.0494	0.0825	0.1351
	(40.86)	(20.15)	(30.60)	(0.2696)	(0.2779)	(0.2587)
4 MPE	32.63	31.16	1.467	-0.3098	-0.1350	-0.1794
	(42.66)	(21.64)	(31.02)	(0.2844)	(0.2892)	(0.2782)
5 MPE	38.66	9.774	28.89	0.3076	0.1228	0.5326*
	(46.77)	(22.67)	(35.57)	(0.3024)	(0.3126)	(0.2908)
7 MPE	56.67	17.46	39.22	-0.2491	-0.1039	-0.2994
	(39.59)	(18.99)	(30.04)	(0.2527)	(0.2589)	(0.2423)
8 MPE	172.2***	35.93	136.3**	0.6597*	0.5239	0.2822
	(65.18)	(27.13)	(53.64)	(0.3463)	(0.3575)	(0.3442)
9 MPE	127.5***	35.12*	92.39***	0.6324**	0.4047	0.6501**
	(43.27)	(20.34)	(33.03)	(0.2630)	(0.2719)	(0.2536)
10 MPE	51.91	21.88	30.03	-0.0239	-0.0994	0.0028
	(43.60)	(20.97)	(32.97)	(0.2797)	(0.2868)	(0.2707)
12 MPE	115.3**	30.74	84.54**	0.5911*	0.3090	0.7451**
	(52.28)	(25.11)	(39.48)	(0.3258)	(0.3373)	(0.3188)
13 MPE	108.3*	37.72	70.54	0.3372	0.3145	0.2501
	(57.62)	(27.81)	(42.96)	(0.3595)	(0.3721)	(0.3475)
15 MPE	56.42	35.92	20.49	0.1858	0.0038	0.1997
	(58.99)	(28.67)	(43.97)	(0.3692)	(0.3826)	(0.3557)
16 MPE	120.4**	65.47**	54.90	-0.0552	-0.0735	0.1741
	(60.53)	(29.85)	(44.79)	(0.3796)	(0.3916)	(0.3686)
17 MPE	125.1**	23.05	102.1**	0.3092	0.0140	0.6138*
	(57.08)	(26.93)	(43.63)	(0.3550)	(0.3657)	(0.3437)
19 MPE	147.6**	36.09	111.5**	-0.2199	-0.2486	-0.1436
	(71.35)	(34.09)	(53.03)	(0.4381)	(0.4512)	(0.4166)
20 MPE	270.0***	67.91**	202.1***	0.1956	0.1467	0.1114
	(79.92)	(34.21)	(64.41)	(0.4293)	(0.4478)	(0.4209)
21 MPE	216.9***	51.93	165.0***	0.5387	0.2095	0.7532*
	(75.05)	(35.43)	(56.21)	(0.4481)	(0.4643)	(0.4261)
22 MPE	146.3*	34.35	112.0**	0.0085	-0.3364	0.2870
	(75.57)	(35.85)	(56.46)	(0.4606)	(0.4759)	(0.4400)
24 MPE	194.4**	45.63	148.8**	0.5578	0.0772	0.6898
	(84.22)	(40.02)	(63.06)	(0.5024)	(0.5217)	(0.4830)
25 MPE	212.0**	48.53	163.4**	0.3897	-0.0343	0.3935
	(87.17)	(41.62)	(64.72)	(0.5233)	(0.5442)	(0.5024)
27 MPE	119.3	40.36	78.89	0.2225	0.0503	0.3005
	(87.46)	(42.14)	(64.61)	(0.5287)	(0.5511)	(0.5068)
28 MPE	172.7*	77.09*	95.58	-0.2065	-0.4134	0.3910
	(88.46)	(42.96)	(65.38)	(0.5385)	(0.5563)	(0.5173)
29 MPE	210.6**	39.29	171.3***	0.0757	-0.1930	0.7419
	(84.31)	(40.02)	(63.27)	(0.5068)	(0.5266)	(0.4869)
31 MPE	198.5*	61.02	137.5*	-0.4286	-0.7665	0.0177
	(109.8)	(52.96)	(79.85)	(0.6685)	(0.6910)	(0.6319)
32 MPE	320.6***	63.23	257.4***	0.2242	-0.0992	0.1060
	(103.7)	(46.58)	(80.88)	(0.5747)	(0.6000)	(0.5546)
33 MPE	268.6**	57.32	211.3**	-0.3394	-0.5165	0.0144
	(122.7)	(57.75)	(90.03)	(0.6896)	(0.7229)	(0.6508)
34 MPE	162.5	14.39	148.1*	-0.0404	-0.7313	0.3296
	(120.8)	(58.68)	(88.69)	(0.7514)	(0.7771)	(0.7133)
41 MPE	281.1**	23.57	257.5**	0.5630	-0.2076	1.134
	(143.4)	(70.08)	(104.9)	(0.8972)	(0.9336)	(0.8796)
Observations	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241
Gender fixed effects	✓	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (1) for unsecured debt outcomes, controlling for observation year, gender, and age fixed effects. Age fixed effect coefficients are not shown. In Columns (1) - (3), the dependent variable is debt balances (total unsecured, revolving unsecured, or non-revolving unsecured). In Columns (4) - (6), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A10: Months Post-Enrollment (MPE) Treatment Effect Estimates: Secured Debt

	Mortgage balance (£) (1)	Mortgage payments (£) (2)	Vehicle finance (£) (3)	Has mortgage x 100 (4)	Has vehicle finance x 100 (5)
1 MPE	-222.2	-2.767	26.98	0.0750	0.1840
	(655.2)	(7.486)	(23.26)	(0.2802)	(0.1506)
3 MPE	292.2	10.51	36.23	0.5377*	0.3234**
	(705.8)	(11.73)	(24.42)	(0.2793)	(0.1595)
4 MPE	1,752.6**	-4.658	7.430	0.2625	0.1586
	(741.1)	(6.734)	(25.31)	(0.2922)	(0.1673)
5 MPE	642.7	-4.910	16.78	0.3109	0.0425
	(751.1)	(8.842)	(28.18)	(0.3088)	(0.1818)
7 MPE	2,234.1***	5.519	14.02	0.6571**	-0.0313
	(651.3)	(8.164)	(23.52)	(0.2638)	(0.1488)
8 MPE	1,913.3**	37.53**	9.864	1.319***	0.0504
	(823.2)	(15.77)	(30.09)	(0.3595)	(0.2015)
9 MPE	1,337.8*	7.663	31.71	0.8510***	0.2640*
	(717.7)	(7.972)	(25.95)	(0.2784)	(0.1573)
10 MPE	2,718.5***	-0.3145	6.902	0.5885**	0.1380
	(766.8)	(7.674)	(26.11)	(0.2931)	(0.1657)
12 MPE	2,412.3***	11.30	14.96	0.8200**	0.0239
	(869.3)	(10.23)	(31.70)	(0.3469)	(0.1981)
13 MPE	1,579.6	10.93	18.02	0.7261*	0.1117
	(964.7)	(12.05)	(34.75)	(0.3963)	(0.2173)
15 MPE	2,113.1**	11.40	42.62	1.004**	0.4144*
	(1,017.5)	(12.54)	(35.35)	(0.3956)	(0.2238)
16 MPE	3,292.8***	-2.630	47.10	0.7903*	0.2978
	(1,042.7)	(9.747)	(37.59)	(0.4065)	(0.2319)
17 MPE	1,723.2*	-0.2444	42.51	0.8084**	0.3821*
	(923.4)	(9.673)	(34.22)	(0.3815)	(0.2185)
19 MPE	3,991.7***	19.07	53.73	1.248***	0.1477
	(1,225.1)	(20.84)	(42.68)	(0.4807)	(0.2616)
20 MPE	3,930.0***	25.72*	8.337	1.817***	0.0028
	(1,119.4)	(15.09)	(41.06)	(0.4593)	(0.2598)
21 MPE	3,184.8**	20.18	69.02	1.305***	0.3986
	(1,297.5)	(16.92)	(45.59)	(0.4938)	(0.2704)
22 MPE	4,542.0***	13.04	14.71	1.103**	0.1222
	(1,333.0)	(17.15)	(45.01)	(0.5063)	(0.2762)
24 MPE	3,831.8***	13.73	50.56	1.273**	0.2690
	(1,430.4)	(19.45)	(50.40)	(0.5579)	(0.3067)
25 MPE	3,140.8**	11.93	62.84	1.188**	0.2916
	(1,489.6)	(20.29)	(53.17)	(0.5893)	(0.3193)
27 MPE	3,302.6**	12.73	79.52	1.261**	0.5692*
	(1,524.8)	(20.74)	(53.12)	(0.5878)	(0.3233)
28 MPE	4,932.3***	6.222	60.71	1.289**	0.2459
	(1,540.8)	(19.86)	(54.15)	(0.5966)	(0.3285)
29 MPE	3,690.4***	5.649	47.18	1.301**	0.4554
	(1,426.2)	(18.18)	(50.13)	(0.5669)	(0.3119)
31 MPE	5,157.6***	12.86	77.26	2.018***	0.3819
	(1,878.8)	(21.42)	(65.97)	(0.7466)	(0.3999)
32 MPE	5,178.4***	25.61	44.34	2.083***	0.1694
	(1,630.6)	(21.31)	(57.23)	(0.6340)	(0.3505)
33 MPE	5,867.8***	12.93	14.45	1.573**	-0.4121
	(2,126.5)	(21.77)	(75.22)	(0.7739)	(0.4142)
34 MPE	5,955.9***	3.486	54.65	1.058	0.0347
	(2,220.6)	(21.08)	(73.52)	(0.8310)	(0.4411)
41 MPE	2,114.2	11.22	115.9	1.727*	1.552***
	(2,400.1)	(27.92)	(89.74)	(1.041)	(0.5792)
Observations	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (1) for secured debt outcomes, controlling for observation year, gender, and age fixed effects. Age fixed effect coefficients are not shown. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A11: Months Post-Enrollment (MPE) Treatment Effect Estimates: Creditworthiness

	Bankrupt x 100 (1)	Default x 100 (2)	Credit score (3)
1 MPE	-0.0712	-0.2199	0.3712
	(0.0536)	(0.2104)	(1.113)
3 MPE	0.0480	-0.2130	0.5035
	(0.0588)	(0.2220)	(1.151)
4 MPE	-0.0330	-0.3588	1.420
	(0.0585)	(0.2313)	(1.208)
5 MPE	-0.0436	-0.4687*	3.458***
	(0.0628)	(0.2422)	(1.268)
7 MPE	0.0411	-0.6960***	3.533***
	(0.0539)	(0.2050)	(1.089)
8 MPE	-0.0462	-0.7712***	4.537***
	(0.0774)	(0.2787)	(1.474)
9 MPE	-0.0040	-0.4313**	2.747**
	(0.0565)	(0.2169)	(1.143)
10 MPE	-0.0092	-0.6850***	3.935***
	(0.0600)	(0.2306)	(1.209)
12 MPE	0.0356	-0.4637*	2.823**
	(0.0700)	(0.2699)	(1.430)
13 MPE	-0.0460	-0.7518**	3.573**
	(0.0775)	(0.3010)	(1.590)
15 MPE	0.0568	-0.7127**	3.670**
	(0.0807)	(0.3067)	(1.610)
16 MPE	-0.0617	-0.9947***	4.185**
	(0.0804)	(0.3122)	(1.661)
17 MPE	-0.0277	-0.8686***	5.979***
	(0.0756)	(0.2919)	(1.545)
19 MPE	0.0627	-1.082***	7.087***
	(0.0949)	(0.3698)	(1.941)
20 MPE	-0.0579	-1.066***	7.943***
	(0.0940)	(0.3514)	(1.841)
21 MPE	-0.0055	-0.8483**	5.857***
	(0.0975)	(0.3786)	(1.990)
22 MPE	0.0593	-1.003**	7.579***
	(0.0998)	(0.3906)	(2.048)
24 MPE	0.0867	-0.7347*	6.116***
	(0.1106)	(0.4278)	(2.246)
25 MPE	0.0355	-1.113**	6.870***
	(0.1146)	(0.4470)	(2.351)
27 MPE	0.1208	-1.090**	7.666***
	(0.1168)	(0.4510)	(2.359)
28 MPE	-0.0254	-1.193***	7.736***
	(0.1170)	(0.4557)	(2.393)
29 MPE	0.0420	-1.152***	9.142***
	(0.1105)	(0.4309)	(2.271)
31 MPE	0.0619	-1.501***	11.54***
	(0.1445)	(0.5642)	(2.977)
32 MPE	-0.0456	-1.572***	11.76***
	(0.1272)	(0.4824)	(2.522)
33 MPE	0.0121	-1.647***	10.90***
	(0.1520)	(0.5874)	(3.075)
34 MPE	0.0133	-0.9263	9.591***
	(0.1598)	(0.6396)	(3.309)
41 MPE	0.1858	-0.9696	8.828**
	(0.2026)	(0.7626)	(4.090)
Observations	1,922,571	1,922,571	1,855,316
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓

Notes: This table shows regression estimates of Equation (1) for creditworthiness outcomes, controlling for observation year, gender, and age fixed effects. Age fixed effect coefficients are not shown. The dependent variable is 100 multiplied by a binary indicator for filing for bankruptcy within the past six years, 100 multiplied by a binary indicator for defaulting on a debt within the past six years, or credit score. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A12: Months Post-Enrollment (MPE) Treatment Effect Estimates: Contributions

	Cumulative contributions (£) (1)	(2)
1 MPE	45.63***	46.48***
	(0.9770)	(0.9559)
3 MPE	104.7***	108.8***
	(1.458)	(1.480)
4 MPE	138.8***	146.6***
	(1.868)	(1.921)
5 MPE	171.5***	183.0***
	(2.351)	(2.466)
7 MPE	240.4***	260.8***
	(3.072)	(3.316)
8 MPE	260.5***	283.9***
	(3.499)	(3.757)
9 MPE	297.4***	327.7***
	(3.574)	(3.864)
10 MPE	333.6***	372.7***
	(4.291)	(4.712)
12 MPE	382.4***	431.4***
	(5.009)	(5.569)
13 MPE	412.9***	468.9***
	(5.433)	(6.061)
15 MPE	468.2***	539.9***
	(6.260)	(7.038)
16 MPE	497.3***	585.0***
	(7.214)	(8.576)
17 MPE	530.0***	623.1***
	(6.928)	(8.027)
19 MPE	607.8***	721.2***
	(9.568)	(11.29)
20 MPE	611.9***	728.8***
	(8.887)	(10.76)
21 MPE	665.0***	803.9***
	(10.65)	(12.79)
22 MPE	701.0***	855.5***
	(11.56)	(13.76)
24 MPE	730.9***	902.2***
	(13.21)	(15.89)
25 MPE	759.3***	944.8***
	(13.78)	(16.63)
27 MPE	807.2***	1,023.6***
	(14.40)	(17.19)
28 MPE	834.8***	1,080.8***
	(16.03)	(20.46)
29 MPE	863.5***	1,120.2***
	(14.63)	(18.38)
31 MPE	951.6***	1,244.9***
	(20.60)	(26.32)
32 MPE	945.5***	1,234.6***
	(18.00)	(23.12)
33 MPE	1,019.5***	1,374.0***
	(27.40)	(36.21)
34 MPE	1,033.8***	1,386.1***
	(30.74)	(38.96)
41 MPE	1,230.5***	1,651.2***
	(45.76)	(60.41)
Observations	1,949,241	1,567,635
Gender fixed effects	✓	✓
Age fixed effects	✓	✓
Year fixed effects	✓	✓

Notes: This table shows regression estimates of Equation (1) for cumulative Nest pension contributions, controlling for observation year, gender, and age fixed effects. Age fixed effect coefficients are not shown. The sample in column (1) is the baseline sample and in column (2) the baseline sample excluding individuals once they leave their original staging firm. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A13: Interactions: Non-revolving Unsecured Debt

	Unsecured debt balances		
	Non-revolving (£)		
	(1)	(2)	(3)
Months post-enrollment	-3.068	15.61***	5.974*
	(2.849)	(4.341)	(3.162)
Income percentile	3,962.5***		
	(133.9)		
Months post-enrollment × Income percentile	14.62*		
	(7.707)		
Income percentile × 2017	-271.5***		
	(86.15)		
Income percentile × 2018	-582.4***		
	(179.1)		
Income percentile × Gender	-1,968.7***		
	(58.47)		
Income percentile × Age	27.18***		
	(3.227)		
Credit score percentile		174.4*	
		(101.6)	
Months post-enrollment × Credit score percentile		-18.26***	
		(5.971)	
Credit score percentile × 2017		991.9***	
		(67.92)	
Credit score percentile × 2018		1,574.7***	
		(139.9)	
Credit score percentile × Gender		-140.9***	
		(46.37)	
Credit score percentile × Age		-65.94***	
		(2.577)	
Age percentile			870.1***
			(36.11)
Months post-enrollment × Age percentile			-1.787
			(6.388)
Age percentile × 2017			-344.4***
			(72.43)
Age percentile × 2018			-661.1***
			(148.7)
Age percentile × Gender			-75.37
			(48.97)
Observations	1,922,646	1,850,127	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

Notes: This table shows regression estimates of Equation (2) modified to include the percentile of income, credit score, or age and its interaction with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of the percentile variable with these fixed effects. The dependent variable is non-revolving unsecured debt balances. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A14: Interactions: Has Mortgage Debt

	Has mortgage x 100		
	(1)	(2)	(3)
Months post-enrollment	0.0099	0.0493*	0.1375***
	(0.0177)	(0.0253)	(0.0296)
Income percentile	131.5***		
	(0.6900)		
Months post-enrollment × Income percentile	0.0111		
	(0.0336)		
Income percentile × 2017	-3.523***		
	(0.3671)		
Income percentile × 2018	-8.500***		
	(0.7701)		
Income percentile × Gender	-2.845***		
	(0.2846)		
Income percentile × Age	-0.8261***		
	(0.0159)		
Credit score percentile		85.51***	
		(0.7397)	
Months post-enrollment × Credit score percentile		-0.0584	
		(0.0433)	
Credit score percentile × 2017		2.581***	
		(0.4675)	
Credit score percentile × 2018		3.096***	
		(0.9862)	
Credit score percentile × Gender		-2.726***	
		(0.3793)	
Credit score percentile × Age		-0.8994***	
		(0.0174)	
Age percentile			26.85***
			(0.2885)
Months post-enrollment × Age percentile			-0.2093***
			(0.0477)
Age percentile × 2017			-6.603***
			(0.5141)
Age percentile × 2018			-12.60***
			(1.086)
Age percentile × Gender			-0.1013
			(0.3920)
Observations	1,922,646	1,850,127	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

Notes: This table shows regression estimates of Equation (2) modified to include the percentile of income, credit score, or age and its interaction with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of the percentile variable with these fixed effects. The dependent variable is 100 multiplied by an indicator for having mortgage debt. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A15: Interactions: Pension Contributions

A. MONTHS POST-ENROLLMENT UP TO 12

	Cumulative total pension contributions (£)		
	(1)	(2)	(3)
Months post-enrollment	21.68***	25.85***	27.60***
	(0.2542)	(0.2587)	(0.3142)
Income percentile	28.14***		
	(5.808)		
Months post-enrollment × Income percentile	22.02***		
	(0.6407)		
Income percentile × 2017	-6.532		
	(6.389)		
Income percentile × Gender	-54.83***		
	(2.677)		
Income percentile × Age	-0.0037		
	(0.1382)		
Credit score percentile		38.09***	
		(5.207)	
Months post-enrollment × Credit score percentile		14.18***	
		(0.5534)	
Credit score percentile × 2017		3.226	
		(5.947)	
Credit score percentile × Gender		-16.93***	
		(2.483)	
Credit score percentile × Age		-0.7694***	
		(0.1184)	
Age percentile			8.776***
			(1.375)
Months post-enrollment × Age percentile			10.03***
			(0.5482)
Age percentile × 2017			-13.43**
			(5.939)
Age percentile × Gender			-18.47***
			(2.508)
Observations	966,294	929,472	979,402
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

B. EXCLUDING LEAVERS AS THEY GO

	Cumulative total pension contributions (£)		
	(1)	(2)	(3)
Months post-enrollment	26.60***	32.82***	35.82***
	(0.6484)	(0.6156)	(0.7608)
Income percentile	59.76***		
	(15.99)		
Months post-enrollment × Income percentile	24.25***		
	(1.458)		
Income percentile × 2017	-35.39**		
	(14.64)		
Income percentile × 2018	60.91*		
	(31.86)		
Income percentile × Gender	-173.0***		
	(7.489)		
Income percentile × Age	0.4163		
	(0.3751)		
Credit score percentile		72.76***	
		(13.22)	
Months post-enrollment × Credit score percentile		12.86***	
		(1.209)	
Credit score percentile × 2017		-2.833	
		(12.48)	
Credit score percentile × 2018		44.44	
		(27.29)	
Credit score percentile × Gender		-43.03***	
		(6.862)	
Credit score percentile × Age		-1.352***	
		(0.2983)	
Age percentile			19.90***
			(3.613)
Months post-enrollment × Age percentile			6.663***
			(1.291)
Age percentile × 2017			-8.423
			(12.99)
Age percentile × 2018			-54.61*
			(28.36)
Age percentile × Gender			-32.07***
			(6.808)
Observations	1,545,783	1,493,512	1,566,094
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

Notes: This table shows regression estimates of Equation (2) modified to include the percentile of income, credit score, or age and its interaction with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of the percentile variable with these fixed effects. The dependent variable is cumulative total Nest pension contributions. The sample is either the baseline sample up to 12 months post-enrollment (Panel A), or the baseline sample excluding individuals once they leave their original staging firm (Panel B). Incremental contributions of employees in the baseline sample are set to zero after they leave their original staging firm. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A16: Interactions: Non-revolving Unsecured Debt

	Unsecured debt balances		
	Non-revolving (£)		
	(1)	(2)	(3)
Months post-enrollment	-53.06	29.31***	7.964
	(73.83)	(10.26)	(6.627)
log(Income)	3,280.2***		
	(138.3)		
Months post-enrollment × log(Income)	5.437		
	(7.218)		
log(Income) × 2017	26.03		
	(80.74)		
log(Income) × 2018	141.0		
	(167.5)		
log(Income) × Gender	-1,943.6***		
	(56.26)		
log(Income) × Age	22.27***		
	(3.354)		
Credit score		2.238***	
		(0.1682)	
Months post-enrollment × Credit score		-0.0240**	
		(0.0098)	
Credit score × 2017		-0.1504	
		(0.1217)	
Credit score × 2018		-0.3150	
		(0.2390)	
Credit score × Gender		-0.0965	
		(0.0791)	
Credit score × Age		-0.1046***	
		(0.0045)	
Age			20.96***
			(0.9295)
Months post-enrollment × Age			-0.0672
			(0.1626)
Age × 2017			-8.741***
			(1.839)
Age × 2018			-16.79***
			(3.782)
Age × Gender			-1.522
			(1.250)
Observations	1,922,571	1,855,316	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

Notes: This table shows regression estimates of Equation (2) modified to include the log of income, credit score, or age and its interaction with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of log income, credit score, or age with these fixed effects. The dependent variable is non-revolving unsecured debt balances. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A17: Interactions: Has Mortgage Debt

	Has mortgage x 100		
	(1)	(2)	(3)
Months post-enrollment	0.1986	0.0362	0.2702***
	(0.2267)	(0.0577)	(0.0558)
log(Income)	115.7***		
	(0.4590)		
Months post-enrollment × log(Income)	-0.0185		
	(0.0219)		
log(Income) × 2017	0.4358*		
	(0.2433)		
log(Income) × 2018	0.3985		
	(0.5035)		
log(Income) × Gender	-2.525***		
	(0.1820)		
log(Income) × Age	-1.088***		
	(0.0102)		
Credit score		0.1060***	
		(0.0011)	
Months post-enrollment × Credit score		-3.86×10^{-6}	
		(6.19×10^{-5})	
Credit score × 2017		0.0024***	
		(0.0007)	
Credit score × 2018		0.0037***	
		(0.0014)	
Credit score × Gender		-0.0045***	
		(0.0005)	
Credit score × Age		-0.0009***	
		(2.64×10^{-5})	
Age			0.6445***
			(0.0074)
Months post-enrollment × Age			-0.0055***
			(0.0012)
Age × 2017			-0.1668***
			(0.0132)
Age × 2018			-0.3176***
			(0.0279)
Age × Gender			0.0072
			(0.0101)
Observations	1,922,571	1,855,316	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

Notes: This table shows regression estimates of Equation (2) modified to include the log of income, credit score, or age and its interaction with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of log income, credit score, or age with these fixed effects. The dependent variable is 100 multiplied by an indicator for having mortgage debt. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A18: Interactions: Pension Contributions

A. MONTHS POST-ENROLLMENT UP TO 12

	Cumulative total pension contributions (£)		
	(1)	(2)	(3)
Months post-enrollment	-133.6***	11.98***	22.53***
	(5.953)	(0.5784)	(0.5786)
log(Income)	56.51***		
	(5.003)		
Months post-enrollment × log(Income)	15.99***		
	(0.5868)		
log(Income) × 2017	0.9227		
	(5.840)		
log(Income) × Gender	-40.28***		
	(2.397)		
log(Income) × Age	-0.8560***		
	(0.1142)		
Credit score		0.0295***	
		(0.0068)	
Months post-enrollment × Credit score		0.0225***	
		(0.0007)	
Credit score × 2017		0.0016	
		(0.0074)	
Credit score × Gender		-0.0288***	
		(0.0031)	
Credit score × Age		-0.0004**	
		(0.0002)	
Age			0.2557***
			(0.0368)
Months post-enrollment × Age			0.2414***
			(0.0143)
Age × 2017			-0.2916*
			(0.1553)
Age × Gender			-0.5336***
			(0.0667)
Observations	966,284	931,029	979,402
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

B. EXCLUDING LEAVERS AS THEY GO

	Cumulative total pension contributions (£)		
	(1)	(2)	(3)
Months post-enrollment	-140.9***	17.07***	31.45***
	(13.73)	(1.389)	(1.420)
log(Income)	138.7***		
	(15.76)		
Months post-enrollment × log(Income)	17.24***		
	(1.341)		
log(Income) × 2017	-14.95		
	(13.38)		
log(Income) × 2018	69.67**		
	(28.88)		
log(Income) × Gender	-125.0***		
	(6.668)		
log(Income) × Age	-1.975***		
	(0.3641)		
Credit score		0.0660***	
		(0.0171)	
Months post-enrollment × Credit score		0.0234***	
		(0.0017)	
Credit score × 2017		-0.0290*	
		(0.0169)	
Credit score × 2018		0.0354	
		(0.0371)	
Credit score × Gender		-0.0832***	
		(0.0087)	
Credit score × Age		-0.0009**	
		(0.0004)	
Age			0.7924***
			(0.0950)
Months post-enrollment × Age			0.1755***
			(0.0336)
Age × 2017			-0.3364
			(0.3389)
Age × 2018			-1.750**
			(0.7386)
Age × Gender			-1.626***
			(0.1805)
Observations	1,547,224	1,497,971	1,567,635
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	

Notes: This table shows regression estimates of Equation (2) modified to include the log of income, credit score, or age and its interaction with months post-enrollment. The regressions additionally control for observation year, gender and—in Columns (1) and (2)—age fixed effects, and the interaction of log income, credit score, or age with these fixed effects. The dependent variable is cumulative total Nest pension contributions. The sample is either the baseline sample up to 12 months post-enrollment with contributions set to zero after employees leave their original staging firm (Panel A), or the baseline sample excluding individuals once they leave their original staging firm (Panel B). Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A19: Automatic Enrollment Treatment Effects on Unsecured Debt (2016 Data)

	Debt balance			Has debt balance x 100		
	Total (£)	Revolving (£)	Non-revolving (£)	Total	Revolving	Non-revolving
	(1)	(2)	(3)	(4)	(5)	(6)
Months post-enrollment	11.84***	2.637*	9.203***	0.0358*	0.0242	0.0344*
	(3.394)	(1.571)	(2.634)	(0.0209)	(0.0215)	(0.0203)
Observations	649,747	649,747	649,747	649,747	649,747	649,747
Gender fixed effects	✓	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for unsecured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2016 data. In Columns (1) - (3), the dependent variable is debt balances (total unsecured, revolving unsecured, or non-revolving unsecured). In Columns (4) - (6), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A20: Automatic Enrollment Treatment Effects on Unsecured Debt (2017 Data)

	Debt balance			Has debt balance x 100		
	Total (£) (1)	Revolving (£) (2)	Non-revolving (£) (3)	Total (4)	Revolving (5)	Non-revolving (6)
Months post-enrollment	7.890***	1.902	5.989***	0.0020	-0.0143	0.0191
	(2.544)	(1.180)	(1.964)	(0.0148)	(0.0155)	(0.0146)
Observations	649,747	649,747	649,747	649,747	649,747	649,747
Gender fixed effects	✓	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for unsecured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2017 data. In Columns (1) - (3), the dependent variable is debt balances (total unsecured, revolving unsecured, or non-revolving unsecured). In Columns (4) - (6), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A21: Automatic Enrollment Treatment Effects on Unsecured Debt (2018 Data)

	Debt balance			Has debt balance x 100		
	Total (£) (1)	Revolving (£) (2)	Non-revolving (£) (3)	Total (4)	Revolving (5)	Non-revolving (6)
Months post-enrollment	4.214 (2.598)	0.8074 (1.221)	3.406* (1.967)	-0.0123 (0.0149)	-0.0232 (0.0156)	0.0047 (0.0147)
Observations	649,747	649,747	649,747	649,747	649,747	649,747
Gender fixed effects	✓	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for unsecured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2018 data. In Columns (1) - (3), the dependent variable is debt balances (total unsecured, revolving unsecured, or non-revolving unsecured). In Columns (4) - (6), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A22: Automatic Enrollment Treatment Effects on Secured Debt (2016 Data)

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£)	Payments (£)	Balance (£)	Payments	Balance
	(1)	(2)	(3)	(4)	(5)
Months post-enrollment	188.6***	0.8817	2.360	0.0855***	0.0204*
	(53.72)	(0.6431)	(1.898)	(0.0220)	(0.0124)
Observations	649,747	649,747	649,747	649,747	649,747
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2016 data. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A23: Automatic Enrollment Treatment Effects on Secured Debt (2017 Data)

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£)	Payments (£)	Balance (£)	Payments	Balance
	(1)	(2)	(3)	(4)	(5)
Months post-enrollment	117.6***	0.6235	1.961	0.0430***	0.0147
	(40.94)	(0.5624)	(1.527)	(0.0162)	(0.0093)
Observations	649,747	649,747	649,747	649,747	649,747
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2017 data. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A24: Automatic Enrollment Treatment Effects on Secured Debt (2018 Data)

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£)	Payments (£)	Balance (£)	Payments	Balance
	(1)	(2)	(3)	(4)	(5)
Months post-enrollment	85.98**	-0.4582	0.3666	0.0323**	0.0040
	(41.59)	(0.5184)	(1.597)	(0.0164)	(0.0095)
Observations	649,747	649,747	649,747	649,747	649,747
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2018 data. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A25: Treatment Effects on Non-Revolving Unsecured Debt

	Total (£) (1)	Unsecured loans (£) (2)	Overdraft (£) (3)	Sales agreements (£) (4)	Other (£) (5)
Months post-enrollment	5.556***	3.426***	1.294	0.0955	0.7406
	(1.907)	(1.198)	(0.9832)	(0.1549)	(0.9269)
Observations	1,949,241	1,949,241	1,949,241	1,949,241	1,949,241
Year fixed effects	✓	✓	✓	✓	✓
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for the components of non-revolving unsecured debt balances, controlling for observation year, gender, and age fixed effects. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A26: Treatment Effects on Revolving Unsecured Debt

	Total (£) (1)	Credit cards (£) (2)	Other revolving (£) (3)
Months post-enrollment	1.616	1.772	-0.1563
	(1.196)	(1.149)	(0.1792)
Observations	1,949,241	1,949,241	1,949,241
Year fixed effects	✓	✓	✓
Gender fixed effects	✓	✓	✓
Age fixed effects	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for the components of revolving unsecured debt balances, controlling for observation year, gender, and age fixed effects. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A27: Automatic Enrollment Treatment Effects on Secured Debt (2016 Data Non leavers)

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£)	Payments (£)	Balance (£)	Payments	Balance
	(1)	(2)	(3)	(4)	(5)
Months post-enrollment	369.9***	1.998***	5.091**	0.2415***	0.0460***
	(55.79)	(0.7194)	(2.037)	(0.0229)	(0.0132)
Observations	620,743	620,743	620,743	620,743	620,743
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2018 data. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A28: Automatic Enrollment Treatment Effects on Secured Debt (2017 Data Non leavers)

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£)	Payments (£)	Balance (£)	Payments	Balance
	(1)	(2)	(3)	(4)	(5)
Months post-enrollment	284.2***	1.575**	3.596**	0.1596***	0.0223**
	(45.77)	(0.7245)	(1.756)	(0.0176)	(0.0105)
Observations	514,765	514,765	514,765	514,765	514,765
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2018 data. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table A29: Automatic Enrollment Treatment Effects on Secured Debt (2018 Data Non leavers)

	Mortgage		Vehicle debt	Has mortgage x 100	Has vehicle debt x 100
	Balance (£)	Payments (£)	Balance (£)	Payments	Balance
	(1)	(2)	(3)	(4)	(5)
Months post-enrollment	214.9***	0.0742	1.602	0.1143***	0.0130
	(50.79)	(0.7244)	(2.038)	(0.0189)	(0.0114)
Observations	432,127	432,127	432,127	432,127	432,127
Gender fixed effects	✓	✓	✓	✓	✓
Age fixed effects	✓	✓	✓	✓	✓

Notes: This table shows regression estimates of Equation (2) for secured debt outcomes, controlling for observation year, gender, and age fixed effects, using only the 2018 data. In Columns (1) - (3), the dependent variable is mortgage balances, monthly mortgage payment, or vehicle debt balance. In Columns (4) and (5), the dependent variable is 100 multiplied by a binary variable indicating whether the individual holds some of the indicated debt product type. The sample is the baseline sample. Standard errors are robust and clustered at the employer level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.