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BORROWING TO SAVE?
THE IMPACT OF AUTOMATIC ENROLLMENT ON DEBT

John Beshears
James J. Choi
David Laibson
Brigitte C. Madrian
William L. Skimmyhorn

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ABSTRACT

Does automatic enrollment into a retirement plan increase borrowing outside the plan? We study a natural experiment created when the U.S. Army began automatically enrolling newly hired civilian employees into the Thrift Savings Plan. Four years after hire, automatic enrollment causes no significant change in credit scores (point estimate 0.001 standard deviations) or debt balances excluding auto loans and first mortgages (point estimate -0.6% of annual salary). We also find no significant increase in auto loan and first mortgage balances in our main regression specification, although the estimated increases in these categories are economically and statistically significant in alternative specifications.

John Beshears
Harvard Business School
Baker Library 439
Soldiers Field
Boston, MA 02163
and NBER
jbeshears@hbs.edu

James J. Choi
Yale School of Management
165 Whitney Avenue
P.O. Box 208200
New Haven, CT 06520-8200
and NBER
james.choi@yale.edu

David Laibson
Department of Economics
Littauer M-12
Harvard University
Cambridge, MA 02138
and NBER
dlaibson@gmail.com

Brigitte C. Madrian
730 B N. Eldon Tanner Building
Brigham Young University
Provo, UT 84602-3113
and NBER
brigitte_madrian@byu.edu

William L. Skimmyhorn
Mason School of Business
The College of William & Mary
101 Ukrop Way
Williamsburg, VA 23187
bill.skimmyhorn@mason.wm.edu

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Automatically enrolling employees into defined contribution retirement savings plans has become increasingly common. In the U.S., adoption of automatic enrollment has been encouraged by legislation at the federal and state levels,[1] and by robust evidence that automatic enrollment increases both the fraction of employees who contribute to the savings plan and the average contribution rate to the plan (Madrian and Shea, 2001; Choi et al., 2002, 2004; Beshears et al., 2008). The Plan Sponsor Council of America (2018) reports that 60% of the 401(k) plans in its 2016 survey sample automatically enroll employees. The United Kingdom, New Zealand, and Turkey now have national pension schemes that mandate automatic enrollment.

Automatic enrollment is intended to increase economic security in retirement. But its effectiveness at doing so depends on how the contributions it induces are financed. The assumption among advocates of automatic enrollment has been that the incremental contributions are mostly financed by decreased consumption (e.g., Thaler, 1994; Beshears et al., 2006). However, no evidence has yet emerged that rules out the alternative possibility that the incremental contributions are funded by slower growth in other asset accounts or faster growth in debt, which would at least partially undo the intended benefit of automatic enrollment.

In this paper we are able to broaden the standard automatic enrollment analysis by studying household *liabilities* and thereby asking whether automatic enrollment affects balance sheet categories other than defined contribution plan balances. Specifically, we link individual employee payroll records to their credit reports to measure the degree to which automatic enrollment is also associated with changes in household debts.

We study a natural experiment created by the introduction of automatic enrollment for *civilian* employees of the U.S. Army, which occurred simultaneously with the introduction of automatic enrollment for all other U.S. federal civil servants.[2] Gelman et al. (forthcoming) find that on the day before payday, the median federal government employee in their sample has liquid assets (checking plus savings account balances) that can cover only five days of spending.[3]

[1] At the federal level, the Pension Protection Act of 2006 encourages employers to use automatic enrollment in their defined contribution savings plans. In addition, several states have set up (or are in the process of setting up) state-facilitated retirement savings plans with the requirement that employers not offering their own retirement savings plans must automatically enroll employees into the state-based plan (Georgetown University, 2018).

[2] Uniformed members of the armed forces were not automatically enrolled during our sample period.

[3] Gelman et al. (forthcoming) find that federal employees sharply reduced their debt repayments in response to the two-week delay of 40% of one paycheck caused by the 2013 federal government shutdown, even though it was known before the paycheck delay that any pay lost during the shutdown would be fully paid retroactively. Living paycheck to paycheck is not unusual; 46% of U.S. adults report that they could not come up with $400 to cover an

Therefore, if automatic enrollment does not reduce consumption, the additional contributions it induces are likely to be funded by borrowing rather than decumulation of liquid assets.

Prior to August 2010, civilian Army employees had to opt *into* contributing to the Thrift Savings Plan (TSP), the defined contribution plan of the U.S. federal government, which is similar to a 401(k) plan. Starting on August 1, 2010, only newly hired employees were automatically enrolled in the TSP at a default contribution rate of 3% of their income unless they opted out. Importantly, employees hired prior to August 1, 2010, have *never* been subject to automatic enrollment. We identify the effect of automatic enrollment by comparing savings and debt outcomes for employees hired in the year prior to the adoption of automatic enrollment to savings and debt outcomes for employees hired in the year after. (We present results from a related regression discontinuity methodology in Online Appendix A.)

Consistent with prior evidence, we find that automatic enrollment at the low 3% default contribution rate chosen by the TSP has a modest positive average effect on contributions to the TSP.[4] At 43-48 months of tenure, automatic enrollment increases cumulative employer plus employee contributions since hire by 4.1% of first-year annualized salary.

To assess the extent to which this positive impact on asset accumulation is offset by changes in household debt, we examine three measures of debt, which we call D1, D2, and D3. Our D1 measure encompasses all debt *except* auto debt and first mortgages. Credit card balances are an example of D1 debt. Because credit cards are probably used primarily to fund non-durable consumption, increases in credit card balances are most likely associated with decreases in net worth.[5]

In contrast, changes in auto and first mortgage debt have more ambiguous implications for net worth because these types of debt are used to finance the acquisition of durable assets. If automatic enrollment increases auto or first mortgage debt by $*X* because it also increases the value of the vehicle or home the household chooses to purchase by $*X* while leaving other assets unaffected, then it has had no impact on net worth because the increase in assets exactly offsets

emergency, or would have to borrow or sell something to do so (Board of Governors of the Federal Reserve System, 2016; see also Kaplan, Violante, and Weidner, 2014).

[4] According to Vanguard (2018), 3% is the most common default contribution rate in savings plans with automatic enrollment.

[5] There would be no net worth decline if credit card balances increase simultaneously with assets such as cash, perhaps because the individual is substituting away from cash purchases towards credit card purchases. However, it is not clear why automatic enrollment would have such an effect.

the increase in liabilities.[6] On the other hand, an $*X* increase in auto or first mortgage debt could be associated with an $*X* decrease in net worth if the extra debt is not due to the household acquiring a more valuable asset, but is instead due to automatic enrollment causing $*X* of other assets to be spent down. Since we do not observe non-TSP assets, we cannot distinguish between these two extreme cases, or among cases that are combinations of these two.

Auto and first mortgage debt are further differentiated from each other by the implications they have for the evolution of *future* net worth. To the extent a larger loan indicates that one has bought more of an asset that depreciates quickly relative to the repayment rate of the debt, higher debt may presage future erosion of net worth. Because vehicles depreciate more quickly than houses (which may even appreciate), increases in auto debt are more negative signals about future net worth than increases in first mortgage balances. Indeed, if an increase in auto debt is not associated with a past decline in net worth, it is likely to be associated with a future decline in net worth. We therefore construct D2—which encompasses debt that has both intermediate and high likelihoods of being associated with net worth decline—by adding auto debt to D1. Our third measure, D3, adds first mortgages to D2 and thus includes all debt reported to the credit bureau.

We find no statistically significant evidence that automatic enrollment increases D1. At 43-48 months of tenure, the point estimate of the automatic enrollment effect as a fraction of first-year salary is –0.6% (a *decrease* in debt), and the 95% confidence interval of [–2.4%, 1.2%] has a right boundary that is relatively close to zero. We also examine the impact of automatic enrollment on credit scores and financial distress as measured by debt balances in third-party collections. We find point estimates that are nearly zero with narrow standard error bands. Therefore, our evidence does not support the worst-case scenario for automatic enrollment, where it increases high-interest debt and financial distress.

In addition, in our primary regression specification, we do not find a statistically significant effect of automatic enrollment at 43-48 months of tenure on auto debt (point estimate = 1.1% of income, 95% confidence interval = [–0.1%, 2.3%]) or first mortgage debt (point estimate = 2.2% of income, 95% confidence interval = [–5.1%, 9.5%]). Consequently, we find no statistically significant effect of automatic enrollment at 43-48 months of tenure on D2 (point

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[6] In a frictionless market, assets and liabilities increase by exactly the same amount, since borrowing that increases the present value of one's liabilities by $1 provides enough financing to buy an asset worth exactly $1.

estimate = 0.5% of income, 95% confidence interval = [-1.7%, 2.8%]) or D3 (point estimate = 2.7% of income, 95% confidence interval = [-5.2%, 10.6%]). Note, however, that the confidence intervals on the first mortgage debt and D3 effects are extremely wide. Furthermore, in some alternative regression specifications, the auto and first mortgage debt effects are positive and statistically significant, and the first mortgage effect has a much larger point estimate (between 7.4% and 16.9% of income)—indicating that the standard errors alone understate the true uncertainty around these estimates.

As previously noted, we would need data on non-TSP assets in order to assess automatic enrollment's impact on (total) net worth. We do not have such data. Instead, we calculate three measures of impact on what we call *pseudo*-net worth to underscore that we observe only part of the balance sheet. The effect on PNW1 is the treatment effect of automatic enrollment on TSP savings minus the treatment effect on D1. The effect on PNW2 is the treatment effect on TSP savings minus the treatment effect on D2. And the effect on PNW3 is the treatment effect on TSP savings minus the treatment effect on D3.

We can reject the null hypotheses that the effects of automatic enrollment on PNW1 and PNW2 are equal to zero. However, we cannot reject the null hypothesis that the effect of automatic enrollment on PNW3 is equal to zero. The respective effects on PNW1, PNW2, and PNW3 at 43-48 months of tenure are 4.7% of income (95% confidence interval = [2.7%, 6.7%]), 3.6% of income (95% confidence interval = [1.2%, 6.0%]), and 1.4% of income (95% confidence interval = [-6.7%, 9.5%]). The considerable attenuation of the PNW2 and PNW3 point estimates relative to the 4.1% effect on TSP assets, the large standard errors (especially for PNW3), and the lack of data on the complete household balance sheet suggest that we probably should not be confident that automatic enrollment has an economically large positive impact on long-run wealth formation.

Our paper is related to Blumenstock, Callen, and Ghani (2018), who run a field experiment on automatic enrollment in Afghanistan. They estimate positive effects of automatic enrollment on total savings that are mostly statistically insignificant, but because they rely on self-reports from a small sample (470 employees), their standard errors are large. Our paper is also related to Chetty et al. (2014), who study how mandatory contributions to Danish retirement accounts affect total savings. They find that a one percentage point increase in these mandatory contributions results in a 0.8 percentage point increase in the total savings rate. Although mandatory contributions have similarities with automatic enrollment, these are two different kinds of policies, as demonstrated by the difference in employees' responses to them. Chetty et al. (2014) show that when an employee moves to an employer with a mandatory contribution rate that is one percentage point higher, the employee's total savings rate remains about 0.8 percentage points higher than at her previous job for the next ten years after the job change. In contrast, Choi et al. (2004) find that in their sample of automatic enrollment firms, about half of employees have opted out of the default contribution rate within two years of hire. Chetty et al. (2014) do not observe mortgage debt, an important margin of potential crowd-out studied in our paper. Choukhmane (2018) documents another margin of crowd-out: if employees are automatically enrolled in their current job's retirement savings plan, they contribute less to their next job's opt-in retirement savings plan. Our paper is also related to the long literature on whether the availability of 401(k) plans on an opt-in basis increases total savings (Poterba, Venti, and Wise, 1995, 1996; Venti and Wise, 1997; Engen, Gale, and Scholz, 1994, 1996; Engen and Gale, 2000; Benjamin, 2003; Gelber, 2011).

The remainder of the paper proceeds as follows. Section I summarizes the relevant institutional details of the TSP and the policy change that we exploit. Section II describes our data, and Section III compares the two hire cohorts that are the focus of our analysis. Section IV documents our empirical findings on the effect of automatic enrollment on TSP contributions. Section V first describes a conceptual framework for thinking about what our various debt measures might imply about net worth and then presents our empirical findings on the impact of automatic enrollment on debt. Section V analyzes each of our three sub-categories of debt—debt excluding auto loans and first mortgages, auto loans, and first mortgages—and discusses results for the cumulative debt variables (D1, D2, and D3) and pseudo-net worth variables (PNW1, PNW2, and PNW3). Section VI shows empirical results for subpopulations that, according to previous literature, were likely to experience relatively large increases in contributions from automatic enrollment. Section VII concludes. Online Appendix A presents results from an alternative estimation strategy using a regression discontinuity design, and Online Appendix B presents supplementary tables and figures.[7]

[7] A presentation available from the authors upon request contains a study of the effect of automatic enrollment on debt using natural experiments in four private-sector firms that separately introduced automatic enrollment between 2006 and 2011. As in the body of this paper, we link credit bureau records to administrative data—in this case, 401(k) data rather than payroll data. Due to small sample sizes, we are unable to estimate the effect of automatic

I. Thrift Savings Plan institutional details and the natural experiment

The institutional details of the Thrift Savings Plan are similar to many private-sector 401(k) plans. Contributions to the TSP are made on each payday. Employee contributions are made via payroll deduction. Civilian employees receive matching contributions from the government: the first 3% of their own income contributed garners a dollar-for-dollar match, and the next 2% of income contributed is matched at a 50% rate. All civilian employee accounts also receive a government contribution called the Agency Automatic (1%) Contribution equal to 1% of their income, regardless of their own contribution rate. Matching contributions are immediately vested, while Agency Automatic (1%) Contributions vest after three years of service or upon the employee's death if the employee is still employed by the government. The IRS imposes limits on the total amount that can be contributed to the TSP within a calendar year. In 2010, the maximum employee contribution was $16,500 for those younger than 50 and $22,000 for those 50 and older. These limits have gradually risen over time. Participants can invest in five index funds—a U.S. Treasury security fund, a U.S. fixed income fund, a U.S. large cap equity fund, a U.S. small cap equity fund, and an international equity fund—and five lifecycle funds, which are mixes of the five index funds based on investor time horizons.

During our sample period, participants could take out at most one general purpose loan and one primary residence loan at a time from their TSP balances while employed. Loans had to be no less than $1,000 and no more than the minimum of (1) the participant's own contributions and earnings on those contributions minus any outstanding loan balance, (2) 50% of the participant's vested account balance or $10,000, whichever is greater, minus any outstanding loan balance, and (3) $50,000 minus any outstanding loan balance.

Employed participants could also take up to one age-based withdrawal of at least $1,000 or 100% of their vested balance (whichever is lesser) once they reach age 59½, and they could take any number of withdrawals at any age if financial hardship was certified.[8] An employee

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enrollment on debt balances with precision. Since Vantage credit scores are more tightly bounded than debt balances, we can estimate credit score effects with more precision. In all four firms, we find an economically small point estimate of the effect of automatic enrollment on Vantage scores.

[8] The TSP website reads: "To be eligible, your financial need must result from at least one of the following four conditions: • Recurring negative monthly cash flow • Medical expenses (including household improvements needed for medical care) that you have not yet paid and that are not covered by insurance • Personal casualty loss(es) that you have not yet paid and that are not covered by insurance • Legal expenses (such as attorneys' fees and court costs) that you have not yet paid for separation or divorce from your spouse."

taking a hardship withdrawal could not contribute to the TSP for the six months following the withdrawal, and if the employee was younger than 59½, a tax penalty had to be paid equal to 10% of the taxable portion of the withdrawal. Hardship withdrawals could be no less than $1,000, and no employer contributions could be withdrawn. When participants left Army employment, they could keep their balances in the TSP if the balances were greater than $200. Former employees who kept their balances in the TSP could take up to one partial withdrawal if they had not previously taken an in-service age-based withdrawal. Otherwise, they could only either keep their entire balances in the TSP or withdraw their balances in full through a mix of a lump sum payment, a series of monthly payments, and a life annuity.

Beginning on August 1, 2010, the U.S. federal government implemented automatic enrollment for all new U.S. federal employees covered by the Federal Employees' Retirement System (FERS), including those in the Army. The Army is the second-largest Cabinet-level agency in the federal government, with over 215,000 civilian employees throughout our sample period (United States Office of Personnel Management, 2016). Before this change, all federal civilian employees had to opt into the TSP to make contributions. After the change, civilian employees who were newly hired or re-hired following a break in service of at least 31 calendar days were automatically enrolled into the TSP at a default employee contribution rate of 3% of income to a pre-tax account. Contributions were invested by default entirely in the U.S. Treasury security fund, although participants could reallocate existing balances and change the destination of future contribution flows to other funds at any point in time.

There were no other changes to the TSP for Army civilian personnel during the year before and the year after the implementation of automatic enrollment, but there were two later policy changes worth mentioning. First, starting in July 2012, Army civilian employees could make contributions on an after-tax basis to a Roth account in the TSP, whereas only pre-tax contributions were allowed previously.[9] Second, furloughs in the federal government reduced pay in 2013. For a period of six weeks beginning on July 8, 2013, most Army civilian employees received one less day of pay per week due to Department of Defense furloughs. Some employees—referred to as excepted employees—whose work was deemed essential continued to

(https://www.tsp.gov/PlanParticipation/LoansAndWithdrawals/inservicewithdrawals/financialHardship.html, accessed July 7, 2017)

[9] Contributions to a Roth account are not deductible from taxable income in the year of the contribution, but withdrawals from a Roth account in retirement are usually not taxed.

work on and receive pay for all regular workdays during this period. To account for the effect of the furloughs in July and August 2013, we make an adjustment to TSP contributions in those months, as detailed in Section IV. A related but separate set of furloughs was implemented in October 2013. On October 1, the federal government shut down and furloughed all of its civilian employees, although excepted employees were required to continue working without pay. On October 5, the Pentagon recalled most of its employees from furlough, and Congress passed a bill guaranteeing that all employees would be paid wages lost due to the shutdown once it ended. The shutdown ended on October 16. Because the shutdown began in the middle of the first pay period of October and ended in the middle of the second pay period of October, no regularly scheduled payday passed without paychecks being issued to all employees. However, the first paycheck in October was abnormally low, and the second paycheck was abnormally high. Gelman et al. (forthcoming) find that employees affected by the October furloughs reduced spending and delayed debt payments during the period of temporarily low income. We only observe contributions at a monthly frequency and credit reports at a biannual frequency, so we make no adjustment for the government shutdown in October 2013.

II. Data description

To measure savings in the TSP, we use employee-level administrative payroll data from the Department of Defense. The payroll data consist of monthly cross-sections from January 2007 to December 2015 of all Army employees hired or re-hired during that period of time. We observe the dollar amounts of employee and employer TSP contributions for each month in this database. We link these records to information from Army personnel data on personal characteristics (year of birth, gender, race, state of residence, education level, and any academic discipline in which that employee specialized) and employment information (most recent year and month of hire, year and month during which the employee first became TSP-eligible, creditable service time as a federal government employee, job type, and annualized pay rate).[10] For the purposes of determining whether an employee was subject to automatic enrollment, we

[10] The Office of Economic and Manpower Analysis (OEMA) merged the Department of Defense payroll data and the Department of the Army personnel data. OEMA provided the merged administrative data to a national credit bureau for matching to credit outcomes. The resulting data set was de-identified prior to use by the research team.

use the year and month during which the employee became eligible for FERS[11], which almost always corresponds to the employee's year and month of hire; for simplicity, we will hereafter refer to the year and month of FERS eligibility as the employee's "hire date." When an employee's monthly payroll records don't begin until the second calendar month of employment (which occurs for 29% of employees) or third calendar month of employment (which occurs for 0.4% of employees), we assume the employee did not contribute to the TSP in the missing month(s).[12] We drop the 0.8% of the sample that does not have a payroll record by the third month of their tenure because of concerns that their payroll data are not reliable. Beyond an employee's second month of tenure, if payroll data are missing for a month, we assume that pay and TSP contributions in the missing month were the same as in the closest preceding non-missing month.[13]

We observe only contribution flows into the TSP; we do not observe plan balances or the funds in which balances are invested. Furthermore, we do not observe withdrawals or loan transactions in the TSP. Our measure of TSP savings will be the cumulative employee plus employer contributions to date (which exclude loan repayments). This will tend to understate TSP balances to the extent that capital gains are important but overstate them to the extent that withdrawals and loans are important. Because automatically enrolled individuals had their balances invested in the Treasury security fund by default, capital gains are unlikely to be very large in the group affected by automatic enrollment. At the end of Section IV, we will show that hardship withdrawals while employed are unlikely to materially affect our results.

For the credit analysis, we use de-identified individual-level credit reports from a national credit bureau matched to the payroll and personnel data.[14] The credit data consist of biannual month-end cross-sections from June 2007 to December 2014. In each cross-section we observe

[11] If an employee converts from being ineligible for FERS to being eligible during the automatic enrollment regime, the employee would by default be enrolled in TSP upon converting.
[12] We suspect that employees who have no payroll record in their first calendar month of employment tend to be those who were hired later in the month, since under opt-in enrollment, their TSP participation rate at the end of the second and third calendar months of employment is lower than that of employees who have a payroll record in their first calendar month of employment, but then equalizes afterwards. However, we cannot directly test this hypothesis because our data on year and month of hire do not provide intra-month information.
[13] Only 1.0% of person-months beyond the second month of tenure are missing from the payroll data. The majority of gaps are only one month long. These periods of missing payroll data may be due to employees briefly becoming affiliated with a different government agency.
[14] Credit records are at the individual level, not the household level. Therefore, if two individuals married to each other are both in our Army sample, we will double-count any debts jointly held by the couple. This bias is probably small.

debt balances[15], number of accounts, and various measures of distress (e.g., late payments, delinquent accounts, bankruptcy proceedings, etc.). The debt measures are broken up by source (e.g., mortgage, bankcard, student loans, auto loans, etc.). We also observe Vantage scores—an estimate of creditworthiness calculated by the credit bureaus that ranges from 300 (least creditworthy) to 850 (most creditworthy)—for all individuals in the credit data. We assume that employees who do not match to a credit report have no debt balances.[16]

III. Comparison of pre- and post-automatic enrollment hire cohorts

To estimate the impact of automatic enrollment, we will compare the savings and credit outcomes of two hire cohorts to each other. The pre-automatic enrollment (pre-AE) cohort consists of Army civilian employees hired in the year preceding the introduction of automatic enrollment—from August 1, 2009, to July 31, 2010. The post-automatic enrollment (post-AE) cohort consists of Army civilian employees hired in the year following the introduction of automatic enrollment—from August 1, 2010, to July 31, 2011.

Table 1 compares the characteristics of these two cohorts. The post-AE cohort is somewhat lower-paid at hire; the average annualized starting salary of the post-AE cohort is roughly 2% below that of the pre-AE cohort after deflating by the average federal pay increase between 2010 and 2011. The post-AE cohort is also slightly older, less likely to be missing race information, less educated, more likely to be in an administrative or clerical position, and less likely to be in a blue collar, professional, or technical position. Although these differences are statistically significant due to the large sample size, their economic magnitudes tend to be small. We will control for these observable differences in our regression analysis. There is not a significant difference between the cohorts in the probability of having a credit report in the six months prior to hire or in the average Vantage score conditional on having a score in the six months prior to hire.

[15] Revolving debt balances show up regardless of whether they are in their grace period (and thus not accruing interest).

[16] A large student lender misreported to the credit bureau from late 2011 through the middle of 2012, causing a significant number of student loan balances to disappear from the data during that period. We flag an individual's total student loan balance in December 2011 or June 2012 as spuriously low if it is lower than both its June 2011 and December 2012 levels. We then replace flagged student loan balances with fitted values from a linear trend drawn between the individual's balances in the two nearest adjacent reliable credit reports on either side of the flagged balances.

IV. Effect of automatic enrollment on TSP contributions

The previous literature on automatic enrollment has focused on savings plan participation and contribution rates as the outcomes of interest (Madrian and Shea, 2001; Choi et al., 2002, 2004; Beshears et al., 2008). Consistent with this literature, Online Appendix Figures B1 and B2 show that automatic enrollment substantially increases savings plan participation at all levels of tenure and shifts the distribution of savings plan contribution rates away from zero and toward the automatic enrollment default contribution rate of 3%.

In this paper, our primary savings outcome of interest is cumulative contributions to the TSP. We estimate the effect of automatic enrollment on cumulative TSP contributions by comparing the pre-AE cohort to the post-AE cohort at equivalent horizons of job tenure, controlling for calendar time fixed effects. Because our payroll data are monthly, it is possible to compare contributions at every tenure month during our sample period using every employee. However, such an approach would not be comparable with our credit analysis, where we can only observe outcomes in June and December of each year. Our computation of cumulative contributions at n months of tenure therefore includes only employees hired n months before a June or December. For example, cumulative contributions at 11 months of tenure for the post-AE cohort are computed using only August 2010 hires (cumulating their contributions from August 2010 through June 2011) and February 2011 hires (cumulating their contributions from February 2011 through December 2011).

We then make two adjustments. First, at each level of tenure, we equalize across all employees the number of paydays included in the cumulative contribution calculations. Due to where calendar month boundaries fall with respect to the biweekly pay schedule, a given tenure month for one cohort might include three paydays while the same tenure month for a different cohort only includes two paydays. Thus, when a pre-AE hire has achieved n months of tenure and experienced m paydays in total (and hence has had m TSP contribution opportunities), a corresponding post-AE hire with n months of tenure may have experienced $m' \neq m$ paydays. Even within a one-month cohort, some employees were hired earlier in the calendar month or left Army employment later in the calendar month than others, and so have had a different number of paydays by the end of the measurement period.[17] We define the benchmark number of paydays

[17] As explained in Section II, our data set includes information on an individual's year and month of hire but does not include the exact date of hire. The same is true for information regarding the date an individual separated from

experienced at *n* months of tenure as the minimum number of paydays across the pre-AE and post-AE cohorts that was experienced by somebody hired at the beginning of the applicable calendar months and employed continuously until the end of the *n*th calendar month of tenure. We scale the last month's contributions of each individual to approximate how much that individual would have contributed by tenure month *n* had she experienced the benchmark number of paydays.[18]

Second, as explained in Section I, mandatory federal government furloughs reduced most employees' pay by 20% for three-quarters of the weeks in the July and August 2013 pay periods. Employees subject to furloughs who did not adjust their contribution rates would have their total contributions in July and August 2013 depressed by 15%. The furloughs occurred at different tenures for the pre- versus post-AE cohorts. We therefore inflate contributions in July and August 2013 by a factor of 100/85.[19] We do not make an adjustment for the government shutdown in October 2013 because it only shifted pay within the month of October.

Figure 1 plots the average ratio of cumulative employer plus employee TSP contributions to annualized first-year pay against tenure. Individuals who cease to appear in the payroll data and never return are dropped from the sample from their departure date onwards. Individuals who cease to appear in the payroll data and return with a different hire date or creditable service computation date are dropped from the sample from their initial departure date onwards. Attrition across the two cohorts is similar.[20] We see in Figure 1 that the post-AE cohort has higher average cumulative TSP contributions than the pre-AE cohort, with the gap between the

employment. However, we can infer the number of paychecks received in a given month by comparing salary paid in that month to annual pay. We assume that if an employee was missing a payroll record in the first month or first two months of tenure, then the employee did not have any paydays in those months.

[18] We do not make a payday adjustment in our debt analysis.

[19] Observed average contributions in July and August 2013 are approximately 10% smaller than in adjacent months, rather than 15%, because some people were exempt from or could delay the furloughs.

[20] At 12, 24, 36, and 48 months, the fractions remaining in the sample for the pre-AE versus post-AE cohorts are 91% versus 90%, 80% versus 77%, 71% versus 67%, and 64% versus 61%, respectively. Online Appendix Tables B1 and B2 show that if we keep a constant sample through all tenures, conditioning on employees who make it to 43-48 months of tenure, our results are similar. Online Appendix Tables B3 and B4 show that the results are also similar if we analyze a balanced panel including all employees who ever appear in the pre-AE cohort or the post-AE cohort, assigning zero incremental TSP contributions after an individual terminates employment. The notable exception to the overall similarity is that Online Appendix Tables B3 and B4 show a significant negative effect of automatic enrollment on debt excluding first mortgages and auto loans, particularly student loans, although the 95% confidence intervals almost always include the point estimates from the main analysis. It is possible that automatically enrolled individuals who terminate employment use withdrawals of TSP balances to repay debt.

two cohorts increasing with tenure.[21] Given the low default contribution rate of 3% of income, it is not surprising that the differences are modest. Averaging over six-month tenure windows, the difference between the pre-AE and post-AE cohort cumulative TSP contributions is 1.9%, 3.4%, 4.5%, and 5.1% of first-year annualized salary at 7-12, 19-24, 31-36, and 43-48 months of tenure, respectively.

To compute regression-adjusted estimates of the impact of automatic enrollment on TSP contributions, we do not use cumulative contributions as the regression outcome variable because we want to control for aggregate shocks that affect all contribution rates within a calendar time period. Suppose the regression outcome variable were cumulative contributions for an employee as of calendar time t. It is natural to think that this variable reflects the sum of calendar time effects going back to the employee's time of hire. At a given t, cumulative contributions for an employee from an early hire cohort therefore reflect a different set of calendar time effects than cumulative contributions for an employee from a late hire cohort. Controlling for an indicator variable for observing cumulative contributions as of t fails to capture this difference.

We address this issue by using contributions during each six-month period as the dependent variable and controlling for six-month calendar period indicators. The explanatory variables also include tenure bucket indicators, as well as tenure bucket indicators interacted with a post-AE dummy. This regression estimates the effect of automatic enrollment on contributions during each six-month tenure bucket. To obtain an estimate of the effect of automatic enrollment on *cumulative* contributions, we add up the estimated tenure-specific automatic enrollment effects from the time of hire to the tenure horizon of interest.

More specifically, to construct our regression outcome variable at n months of tenure, we look only at employees hired n months before a June or December. Taking cumulative contributions as of that June or December, we subtract cumulative contributions as of the preceding December or June. This variable captures total contributions during the six-month period leading up to and including the June or December that is the employee's nth tenure month. For example, the outcome variable at 11 months of tenure for the post-AE cohort

[21] The apparent seasonality in the series that occurs at a six-month frequency reflects differences across calendar-month hire cohorts and arises because the hires in a given calendar month appear in the graph only once every six months.

captures January-June 2011 contributions for August 2010 hires and July-December 2011 contributions for February 2011 hires.

We stack all observations through tenure month 53 into a single regression and estimate the equation

$$y_{i\tau t} = \eta_t + \sum_s [I(\tau \in T_s)(\alpha_s + \beta_s X_i + \gamma_s PostAE_i)] + \epsilon_{i\tau t}, \quad (1)$$

where $y_{i\tau t}$ is the outcome variable for person i at tenure τ and calendar time t, η_t is a calendar time effect, $I(\tau \in T_s)$ is an indicator variable for tenure τ being in tenure bucket T_s, X_i is a vector of control variables measured as of hire (log deflated salary, age, age squared, and dummies for gender, education level, job type, college major, state of residence, and race), and $PostAE_i$ is an indicator variable for being in the post-AE cohort.[22] The coefficient γ_s represents the treatment effect of automatic enrollment on the outcome variable for tenure bucket T_s. We are ultimately interested in the treatment effect of automatic enrollment on cumulative contributions as of a given tenure bucket, so we report the cumulative sum of γ_s values up to and including the γ_s for the tenure bucket of interest. These cumulative sums are what are shown in Figure 2 and the first column of Table 2.

Using this approach, we find treatment effect estimates that are somewhat smaller than those computed from the raw differences: automatic enrollment raises cumulative contributions by 0.9%, 2.0%, 3.1%, and 4.1% of first-year salary at 7-12, 19-24, 31-36, and 43-48 months of tenure, respectively. These estimates are all highly statistically significant, with *t*-statistics (using standard errors clustered at the employee level) of approximately 10. We use 43-48 months of tenure as our preferred long-run tenure bucket, rather than 49-53 months, because post-AE cohort members hired from January to July 2011 do not contribute to the estimates at 49-53 months, as they are never observed at those tenures in our credit bureau data.[23]

The second column of Table 2 shows the regression-adjusted estimate of the effect of automatic enrollment on cumulative employee contributions, which exclude the employer match and Agency Automatic (1%) Contributions. The effect on employee contributions is less than half of the effect on total contributions. One might have expected the effect on total contributions

[22] The education level, job type, college major, and race categories are those shown in Table 1. We use a single dummy variable for the 15 states and territories with fewer than 100 employees in the sample.
[23] The regression sample includes observations beyond 43-48 months of tenure for individuals hired before January 2011 because those observations are used for estimating calendar time effects during the periods when the individuals hired from January to July 2011 have 43-48 months of tenure.

to be approximately equally split between employer and employee contributions because the TSP match structure is 100% on the first 3% of income contributed and 50% on the next 2% of income contributed. Automatic enrollment at a 3% default employee contribution rate induces many employees who would otherwise contribute 0% of income to instead contribute 3% of income and earn a one-for-one match. However, automatic enrollment can also induce employees who would otherwise contribute at a high rate to instead contribute 3% of income. If automatic enrollment increases employee contribution rates among those who have a high marginal match rate and decreases employee contribution rates among those who have a low (or no) marginal match, the net result is that the increase in employer contributions is more than half of the increase in total contributions.

Although our data do not contain withdrawal information, we can estimate an upper bound on how much hardship withdrawals undo the automatic enrollment contribution effect. Such withdrawals must be at least $1,000 and require the employee to stop contributing to the TSP for at least six months afterwards. For the bounding exercise, we assume that an employee has taken a hardship withdrawal on date t equal to 100% of her employee contributions to date if the employee was contributing to the TSP on date t, has at least $1,000 of cumulative employee contributions as of date t, and stops contributing for at least six months after date t.[24] Using this approach, we find that hardship withdrawals are rare. Subtracting our imputed measure of hardship withdrawals from contributions reduces the estimated impact of automatic enrollment on TSP balances by only 0.1% of first-year income at 43-48 months of tenure.

The first two columns of Table 3 show the results from performing the same regression analysis as in the first two columns of Table 2, but with a specification that does not control for the interaction of tenure and demographics. In other words, equation (1) is modified to replace β_s with a set of coefficients β that do not vary with tenure. For the contribution outcome variables, this alternative specification makes very little difference.

[24] If an employee's streak of not contributing is right-censored by the end of our sample period, we assume that the employee has made a hardship withdrawal.

V. Effect of automatic enrollment on credit outcomes

A. Debt measures

To get a more comprehensive picture of how automatic enrollment affects the household balance sheet, we examine three measures of debt: debt excluding first mortgages and auto debt (D1), debt including auto balances but excluding first mortgage balances (D2), and total debt (D3). D1 consists of debt that is not explicitly associated with the purchase of a durable asset, and hence most likely to be used to finance non-durable and service expenditures that contemporaneously decrease net worth. D2 and D3 successively add components of debt that are decreasingly likely to be associated with net worth erosion. All three debt measures include non-derogatory balances on installment and revolving loans held by creditors who report to the credit bureau (i.e., the lender has not taken action beyond requiring the minimum payment, usually because the debt is not over 120 days overdue for installment loans, not over 180 days overdue for revolving debt, and not included in bankruptcy proceedings). Creditors such as payday lenders that do not report to the credit bureau are excluded from our debt measure. We also include derogatory debt that has been passed to an external collection agency.[25]

In order to understand what increases in auto debt and first mortgages signify about net worth, it is helpful to recall the following balance sheet equation that holds in a frictionless market upon the origination of a loan:

$$\Delta Secured\ debt = \Delta Durable\ assets + \Delta Financial\ assets. \quad (2)$$

This equation says that the present value of new debt repayments equals the value of any durable asset acquired using the loan proceeds plus the change in financial assets. Taking out a larger secured loan indicates the purchase of a more valuable asset and/or a smaller spend-down of financial assets. If not all of the loan proceeds are used to acquire an asset—for example, in a cash-out mortgage refinancing transaction, where the borrower stays in the same home but extracts equity from it by taking out a larger first mortgage while retiring the original first mortgage—the change in financial assets could be positive. In any of these scenarios, the

[25] Our debt measure excludes charge-off accounts that have not been passed to an external collection agency (these are accounts where the original creditor has given up trying to collect on the debt), debts included in bankruptcy, and accounts in repossession or foreclosure. Charged-off debts on which repayment is not being sought arguably do not decrease the debtor's net worth. Similarly, debts in bankruptcy are likely to be eliminated. Debts in repossession or foreclosure are secured debts, so to a first approximation do not affect net worth.

contemporaneous impact on net worth—the increase in assets minus the increase in liabilities—is zero, although extracted equity may subsequently be spent down.

An automatically enrolled household might purchase a more valuable durable because it feels wealthier due to its increased TSP balances. Extra TSP balances can also ease financing constraints, since they can be accessed through a TSP loan to increase a down payment, enabling the household to get a larger secured loan.[26] To take an extreme example, Federal Housing Administration mortgage loans are subject to a 96.5% loan-to-value ratio maximum, so an extra dollar available for a down payment allows the household to access 96.5/3.5 = $27.57 more financing. The larger mortgage balance does not represent any contemporaneous net worth reduction in this transaction, since each dollar of borrowed TSP balances has been transformed into a dollar of home equity, and each additional dollar of mortgage debt is offset by an additional dollar of housing asset.

Conversely, an automatically enrolled household might extract equity or spend down fewer financial assets to acquire a durable because it has fewer financial assets available. Even though the transaction itself still has no effect on net worth in this case, the larger loan signals that automatic enrollment caused the household to draw down its non-TSP financial assets in the *past*. Hence, the portion of the loan increase that is attributable to non-TSP asset spenddown should be subtracted from TSP assets when calculating the net worth effect of automatic enrollment. Because most federal employees have minimal balances in checking and savings accounts outside the TSP (Gelman et al., forthcoming) and because automatic enrollment affects the left tail of the savings distribution most powerfully (Choi et al., 2004; also see Online Appendix Figure B2), the impact of this channel may be relatively small. However, we cannot be sure because we do not observe non-TSP assets.

Taking out a larger secured loan has potential implications for *future* net worth. We first consider the case where equity is not extracted as part of the transaction. Let W_t be wealth at time t, P_t be the price of asset a at time t, κ be costs as a fraction of asset value that owners pay but renters do not (e.g., property tax on homes), and r be the interest rate. In a frictionless market, the following two strategies for getting use of a for T periods have an identical effect on wealth T periods later: renting a for T periods, or buying it and then selling it T periods later.

[26] Calls to Bank of America, Citibank, and JPMorgan Chase confirmed that loans from retirement savings plans can be used for this purpose.

Thus, if a secured loan is used to purchase an asset but the household otherwise would have rented another asset that has the same rental value as the purchased asset, there is no effect on the path of future net worth. Expressing the $T = 1$ version of the above relationship, we get

$$W_t(1+r) - rent_t = (W_t - P_t)(1+r) - \kappa P_t + P_{t+1}. \tag{3}$$

Equation (3) holds whether or not $W_t \geq P_t$. We can solve (3) for the rental rate:

$$rent_t = P_t(r+\kappa) - (P_{t+1} - P_t). \tag{4}$$

Likewise, holding fixed the asset purchased, the size of the loan used to finance the purchase has no effect on future net worth. A larger loan does obligate the household to higher future interest payments, but these are exactly offset by the greater interest income generated by the assets that did not have to be spent down due to the larger loan.

Suppose, on the other hand, that a larger secured loan is taken out to purchase a more valuable asset a', with price $P'_t > P_t$, rather than renting a. Let W'_{t+1} be wealth at $t+1$ if a' is purchased, and W_{t+1} be wealth at $t+1$ if a is rented. Assume for simplicity that the price of both a and a' will experience proportional growth g between t and $t+1$ and κ is the same for both assets. Then

$$\begin{aligned} W'_{t+1} - W_{t+1} &= (W_t - P'_t)(1+r) - \kappa P'_t + P'_t(1+g) - [W_t(1+r) - rent_t] \\ &= (P'_t - P_t)(g - r - \kappa), \end{aligned} \tag{5}$$

where we have substituted in the expression in equation (4) for $rent_t$. Equation (5) tells us that a larger secured loan erodes future net worth through interest payments that are higher by $(P'_t - P_t)r$. But a larger secured loan also affects future net worth through the differential ownership cost and price appreciation of the asset acquired, $(P'_t - P_t)(g - \kappa)$. Note that the expression for the effect of buying a more expensive asset instead of *buying* a cheaper asset is identical to the last expression in equation (5).

The price growth rate g is highly negative for vehicles; the average new car loses about 60% of its value over the first five years of its life.[27] In contrast, the Bureau of Economic Analysis estimates that a new one-to-four unit residential structure loses only 6% of its value to depreciation over the first five years of its life, and in many markets, homes experience price

[27] https://www.carfax.com/blog/car-depreciation/ (accessed November 24, 2017). Arguably, a good deal of the depreciation occurs the moment the vehicle is driven off the dealer's lot. However, there is a difference between the "hold to maturity" value of the car—the present discounted value of the service flows it provides the owner over the course of its entire useful life—and the liquidation value of the car, which is depressed by adverse selection in the used car market. The "hold to maturity" value probably does not drop much immediately after purchase, whereas the liquidation value does.

appreciation that can be expected ex ante (Case and Shiller, 1989).[28] Therefore, a debt-financed purchase of a more expensive car is likely to result in future net worth erosion, but a debt-financed purchase of a more expensive house has ambiguous effects.

Secured loans can also increase net worth through a "forced savings" channel, where the secured loan repayment schedule causes the household to accumulate equity in the asset at a faster rate than it would have otherwise saved in total. This channel is unlikely to be very effective when the asset depreciates quickly, so that little equity is accumulated over the course of the loan. Again, this implies that a larger auto loan is a more negative signal about future net worth than a larger first mortgage.

We next consider the future net worth implications of a cash-out refinancing. If all of the extracted equity is invested, then the transaction does not change the path of future net worth. If the extracted equity is all spent on non-durable goods and services, net worth falls by the full amount extracted. Across all the waves of the Survey of Consumer Finances from 1998 to 2016, respondents' stated rationale for cash-out refinancing is relatively stable: about 40% say it is for home improvement and repair, about 10% say it is for other investment, about 5% say it is to buy a home, about 5% say it is to buy a vehicle, and about 40% say it is for other purposes. Canner, Dynan, and Passmore (2002) find that only 16% of extracted dollars are used for consumer expenditures, a category that includes vehicle purchases (a durable) and educational expenses (an investment); 26% of dollars go to repaying other debts; 45% go to home improvements, real estate, or business investment; 11% go to financial investment; and 2% go to taxes. It may be appropriate to treat self-reported reasons for cash-out refinancing with skepticism, as survey respondents may wish to portray themselves as financially responsible. However, Zhou (2017) examines survey measures of expenditures (as opposed to what respondents say they will use their cash-out refinancing proceeds for) and finds that of the expenditure categories measured by the Panel Study of Income Dynamics, the positive effect of home equity extraction is highly concentrated in categories associated with housing investment. In summary, the evidence suggests that the bulk of extracted equity that does not go towards debt repayment is invested. Of course, even if a household does not immediately use the proceeds from cash-out refinancing for

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[28] We take the rate of depreciation from https://www.bea.gov/national/pdf/BEA_depreciation_rates.pdf (accessed November 24, 2017).

non-durable goods and services, it may use them to increase its purchases of non-durable goods and services in the future.

Finally, there is an additional cost to taking out a larger loan in the real world. Because of financial market frictions, expected borrowing costs per dollar of financing exceed expected lending rates of return. In other words, receiving financing worth X requires incurring a liability whose present value is $Y > X$. Consequently, even the contemporaneous impact of a secured asset purchase on net worth is negative and decreasing (i.e., becoming more negative) in the size of the loan. Mehra, Piguillem, and Prescott (2011) estimate the average spread in the U.S. economy between borrowing and lending rates to be 2.0%.

B. Econometric methodology

We wish to estimate the effect of automatic enrollment on debt and other financial outcomes, controlling for calendar time effects and other factors. When we estimated the effect of automatic enrollment on cumulative TSP contributions, we did not use cumulative TSP contributions directly as the regression outcome variable because the outcome variable would then seem inconsistent with the regression specification featuring additive calendar time effects. In contrast, when debt levels or credit scores are the outcome variable, additive calendar time effects seem to be a reasonable specification. This judgment is based on Figures 3 through 6, which plot credit outcomes for the pre-AE and post-AE cohorts in June and December of the years 2007-2014. These figures show that there are important calendar time effects for credit outcomes over this period, and they also suggest that the calendar time effects shift credit outcomes for both the pre-AE and post-AE cohorts roughly additively. Additionally, we see that at a given point in calendar time before either cohort was hired, the post-AE cohort's credit variables are often at a different level than the pre-AE cohort's, which is at least partially due to the post-AE cohort being younger than the pre-AE cohort at each calendar date.

To estimate automatic enrollment effects while controlling for calendar time effects and fixed differences across cohorts, we estimate the following equation:

$$y_{i\tau t} = \zeta_i + \eta_t + \sum_s [I(\tau \in T_s)(\alpha_s + \beta_s X_i + \gamma_s PostAE_i)] + \epsilon_{i\tau t}, \tag{6}$$

where $y_{i\tau t}$ is the credit outcome for employee *i* at tenure τ and calendar date *t*, ζ_i is the employee fixed effect, η_t is the calendar time effect, $I(\tau \in T_s)$ is an indicator variable for tenure τ being in tenure bucket T_s, X_i is a vector of control variables measured as of hire (the same variables as in equation (1)), and $PostAE_i$ is an indicator variable for employee *i* being in the post-AE cohort. We allow for negative tenure effects in case the period leading up to hire is associated with events like unemployment that affect credit variables, and we exclude the tenure bucket containing tenure months –5 to 0 (where month 0 is the last calendar month before hire) from the summation in order to avoid multicollinearity with the employee fixed effect.[29] The tenure buckets included in the summation are {≤ –18, –17 to –12, –11 to –6, 1 to 6, 7 to 12, …, 43 to 48, 49 to 53}. The coefficient α_s represents how much the credit outcome differs from its value at tenures –5 to 0 due to achieving a tenure level in bucket s under an opt-in TSP enrollment regime. The main coefficient of interest, γ_s, is the incremental effect of being in tenure bucket s under an automatic enrollment regime instead of an opt-in enrollment regime.

It is well-known that even with perfect panel data, calendar time, tenure, and cohort effects cannot be separately identified without additional identifying assumptions because the three variables are collinear (e.g., Ameriks and Zeldes, 2004). Our identifying assumption is that tenure effects and the interaction effects of tenure with demographics are constant for all tenures less than or equal to –18 months. This assumption seems reasonable, as any credit outcome changes specifically associated with a job transition are likely to be concentrated in the 18 months before hire. To see how this assumption enables us to estimate all of our coefficients, take the expectation of first differences for two pre-AE individuals who are one tenure bucket apart at date *t*:

$$E(\Delta y_{i\tau t}) = (\alpha_s + \beta_s X_i - \alpha_{s-1} - \beta_{s-1} X_i) + (\eta_t - \eta_{t-1}) \tag{7}$$

$$E\left(\Delta y_{i',\tau-1,t}\right) = (\alpha_{s-1} + \beta_{s-1} X_{i'} - \alpha_{s-2} - \beta_{s-2} X_{i'}) + (\eta_t - \eta_{t-1}). \tag{8}$$

Taking the difference between (7) and (8) eliminates the calendar time effects:

$$\begin{aligned} E(\Delta y_{i\tau t}) - E\left(\Delta y_{i',\tau-1,t}\right) = {} & (\alpha_s + \beta_s X_i - \alpha_{s-1} - \beta_{s-1} X_i) \\ & -(\alpha_{s-1} + \beta_{s-1} X_{i'} - \alpha_{s-2} - \beta_{s-2} X_{i'}). \end{aligned} \tag{9}$$

For τ sufficiently negative, $\alpha_{s-1} - \alpha_{s-2} = \beta_{s-1} - \beta_{s-2} = 0$, allowing us to identify $(\alpha_s + \beta_s X_i - \alpha_{s-1} - \beta_{s-1} X_i)$. Normalizing the tenure effect α_{s-1} and the interaction effects of tenure with demographics β_{s-1} at a certain tenure bucket to be zero[30], we obtain an estimate for $\alpha_s + \beta_s X_i$. Repeating this procedure using another individual in tenure bucket s at date *t* with a

[29] We also exclude one calendar time dummy to avoid multicollinearity.
[30] The outcome variable *y* is not restricted to be unaffected by demographics in this baseline tenure bucket, since equation (6) includes an individual fixed effect.

different demographic value $X_{i''}$ gives us an estimate of $\alpha_s + \beta_s X_{i''}$, and repeating the procedure for many individuals with differing demographics provides enough variation to estimate α_s and β_s separately. We can then proceed to estimate α and β for every other higher tenure bucket using equation (9) and substituting in the previously estimated α and β for lower tenure buckets. Analogous reasoning shows how the post-AE cohort's γ_s coefficients are identified as well.

C. Empirical results

Figure 7 and the third column of Table 2 show the treatment effects of automatic enrollment on D1 at each tenure bucket.[31] Reassuringly, there is no significant effect of automatic enrollment estimated before hire, when neither cohort was subject to automatic enrollment. The pattern of no significant effect continues after hire, all the way out to 49-53 months of tenure. At 43-48 months of tenure, the point estimate of the automatic enrollment effect is –0.6% of first-year income, with a 95% confidence interval of [–2.4%, 1.2%].

We can decompose D1 into seven sub-categories: home equity lines of credit (HELOCs), non-HELOC revolving debt, other installment debt, second mortgages, student loans, accounts in external collections, and residual debt that does not belong to the other categories. Non-HELOC revolving debt consists of credit cards and personal lines of credit. Other installment debt consists almost entirely of non-mortgage/non-student/non-auto personal installment loans (both secured and unsecured) from personal finance companies, banks, and credit unions, but it also includes retail installment loans from retailers, which are usually used to finance a major purchase such as an appliance or furniture.[32] Examples of debt that falls in the residual category are charge cards such as American Express cards that must be paid in full at the end of each month.

Table 4 displays the automatic enrollment effect on each of the above components of debt. Few of the coefficients are statistically significant, and the significant coefficients are sometimes negative. The magnitude of the positive and significant coefficients is small—only 0.1% to 0.2% of first-year income for residual debt at later tenures. Note that there are 84 (non-

[31] In Table 2, the number of observations for the first two columns is less than the number of observations for the other columns because the contribution regressions do not include observations prior to an employee's hire date.
[32] There is an argument that we should exclude retail installment loans from D1 because they, like auto loans, are being used to purchase a durable good. However, furniture and appliances are much harder to sell on the secondary market than cars, so the ability to use these assets to generate cash is quite limited.

independent) hypothesis tests shown in the table, so we would expect some of these coefficients to be statistically significant at conventional levels by chance. We see no increase in debt in third-party collections, suggesting that automatic enrollment has no impact on the probability of financial distress.

Figure 8 and the fourth column of Table 2 show the effect of automatic enrollment on Vantage scores, conditional on having a Vantage score.[33] Consistent with what we saw with debt in third-party collections, we find no statistically significant effects on credit scores, and the point estimates lie between –0.3 and 1.3 points across the positive tenure spectrum. To assess the economic significance of the results, note that the standard deviation of Vantage Scores for the full sample in the six months prior to hire is 95. Therefore, the point estimates indicate an effect that is no more than 0.02 standard deviations in magnitude, with the lower end of the 95% confidence intervals reaching only –0.03 standard deviations. In sum, there is no indication that automatic enrollment creates any meaningful change in creditworthiness.

In contrast to the effect on D1, the point estimates indicate that automatic enrollment might create an economically meaningful increase in auto debt that rises with tenure, but none of the estimates are statistically distinguishable from zero. These results are shown in Figure 9 and the fifth column of Table 2. At 43-48 months of tenure, the effect on auto debt is 1.1% of first-year income, with a 95% confidence interval of [–0.1%, 2.3%]. The estimated effects of automatic enrollment on first-mortgage debt, shown in Figure 10 and the last column of Table 2, are not statistically significant, but they are also less precisely estimated than the auto debt effects because of the large variance in first mortgage balances. The first mortgage effect at 43-48 months of tenure is 2.2% of first-year income, with a 95% confidence interval of [–5.1%, 9.5%].[34] As discussed previously, any increase in auto debt likely represents net worth reductions

[33] The regression with Vantage score as the outcome variable excludes observations with missing Vantage scores. Vantage score is missing either because it could not be calculated for an individual's credit file or because the individual was not successfully matched to a credit file. Individuals who were not successfully matched to a credit file are assigned zero debt for the other regressions in Table 2 with credit outcome variables, so those regression samples have more observations than the sample for the Vantage score regression.

[34] In untabulated linear probability regressions, we find that automatic enrollment does not have a significant effect at 43-48 months of tenure on the probability of having any auto debt (point estimate = 0.9 percentage points, 95% confidence interval = [–1.0 pp, 2.7 pp]) or on the probability of having a first mortgage (point estimate = –0.2 percentage points, 95% confidence interval = [–1.7 pp, 1.3 pp]). Automatic enrollment could increase the extensive margin of auto loans and first mortgages if it eases down-payment constraints by making more TSP balances available for a TSP loan.

that have already occurred or will occur in the future, while higher first mortgage balances have an ambiguous effect on future net worth.

Table 5 adds the auto loan and first mortgage effects to the D1 effects to produce automatic enrollment effects on D2 and D3. At 43-48 months of tenure, automatic enrollment does not significantly increase D2 (point estimate = 0.5% of first-year income, 95% confidence interval = [–1.7%, 2.8%]) or D3 (point estimate = 2.7% of first-year income, 95% confidence interval = [–5.2%, 10.6%]).

To gauge the net impact of automatic enrollment, we calculate the difference between its effect on cumulative total TSP contributions and its effect on D1, D2, or D3.[35] We call these differences the "pseudo-net worth" effects, or PNW1, PNW2, and PNW3, respectively, to emphasize that they miss changes in assets outside the TSP that would be necessary to measure true net worth changes. In addition, since we do not have information on employees' current and future marginal tax rates, the measures do not adjust for the fact that TSP contributions were made with before-tax dollars (at least until Roth contributions became available in July 2012) and debts must be paid mostly with after-tax dollars.

We see in the fourth column of Table 5 that the automatic enrollment effect on PNW1 is positive and significant from 7-12 months of tenure onwards. The point estimates indicate that automatic enrollment raises PNW1 by 1.4%, 2.9%, 3.6%, and 4.7% of first-year salary at 7-12, 19-24, 31-36, and 43-48 months of tenure, respectively. The effect of automatic enrollment on cumulative contributions minus D2 is also positive and statistically significant. The fifth column of Table 5 shows that the point estimate for the PNW2 effect at 43-48 months of tenure is 3.6% of first-year salary. The final column of Table 5 shows that the point estimates for the effect of automatic enrollment on PNW3 are closer to zero and are not statistically significant, indicating that the increase in D3 caused by automatic enrollment may be quantitatively important relative to the increase in cumulative total contributions.

[35] We compute standard errors of these differences by bootstrap. For each bootstrap sample, we sample at the employee level and put the sampled employee's entire available history into the contribution regression and the debt regression. We then compute the difference between the estimated treatment effect on contributions and the estimated treatment effect on debt at all the positive tenure buckets. Standard errors are based on 1,000 bootstrap samples. We generate confidence sets that are robust to skewed bootstrap distributions. For our pseudo-net worth statistic $\hat{\theta}$, we generate the $2\alpha\%$ confidence set $[2\hat{\theta} - \hat{\theta}^*_{1-\alpha}, 2\hat{\theta} - \hat{\theta}^*_{\alpha}]$, where $\hat{\theta}^*_{\alpha}$ represents the αth quantile of the bootstrap distribution of $\hat{\theta}$. We obtain p-values in the usual way: if 0 is not contained in the 5% (1%) confidence set of $\hat{\theta}$, then we say that $\hat{\theta}$ is significant at the 5% (1%) level.

An earlier version of this paper presented estimates only from a specification that does not control for the interaction of tenure and demographics. These results are found in Table 3 and Online Appendix Tables B5 and B6. Under this alternative specification, automatic enrollment still does not have significant effects on D1 (point estimate at 43-48 months = 0.9% of income, 95% confidence interval = [–0.9%, 2.7%]) and credit scores (point estimate at 43-48 months = 0.2, 95% confidence interval = [–2.1, 2.6]), but it does have a positive and significant effect on auto debt and a positive and marginally significant effect on first mortgage debt. At 43-48 months, the point estimates indicate an increase in auto debt of 2.0% of income (95% confidence interval = [0.8%, 3.2%]) and an increase in first mortgage debt of 7.4% of income (95% confidence interval = [–0.03%, 14.7%]). Consequently, the positive effect on PNW2 loses significance, and the point estimate for the effect on PNW3 is negative but not significant. The point estimates of the treatment effects on debt categories from this alternative specification are generally within the 95% confidence intervals of the main specification, but also generally larger in magnitude. Although we prefer our main specification because there is no strong prior reason to believe there to be no interaction between tenure and demographics, we acknowledge that the statistical significance and point estimates of the auto and first mortgage debt effects are sensitive to the regression specification. In other words, the standard errors of our main specification's estimates understate the true uncertainty in our estimates.

We have also explored the extent to which automatic enrollment increases cash-out mortgage refinancing. We do not directly observe cash-out mortgage refinancing, so we create a proxy for this activity. We deem an individual to have executed a cash-out mortgage refinancing if, between two consecutive credit file observations, three conditions are met: (1) the individual's first mortgage balance increased by more than 10% of first-year income, (2) the individual's number of first mortgage accounts did not change, and (3) the individual's residential ZIP code did not change according to personnel records. This definition is imperfect because, among other reasons, an individual may be erroneously coded as having executed a cash-out mortgage refinancing if she sold her house and purchased a new one with a larger first mortgage within the same ZIP code. Nonetheless, the definition will capture many cash-out refinancing transactions successfully.

We only observe residential ZIP codes for individuals during their employment with the Army, so we construct the cash-out variable for employees starting at 7-12 months of tenure (comparing their ZIP code at that time to their ZIP code at 1-6 months of tenure) and continuing until they terminate employment. We run a linear probability regression with the cash-out indicator as the outcome variable, using a modified version of the specification in equation (6). Because we observe the outcome variable starting at 7-12 months of tenure, we must assume that tenure effects and the interaction effects of tenure with demographics are constant for all tenures less than or equal to 18 months (instead of –18 months). The untabulated results suggest that automatic enrollment does not increase cash-out refinancing activity. At most tenure levels, the point estimate for the effect of automatic enrollment is slightly negative, and at no tenure horizon does the point estimate exceed 0.1 percentage points with a standard error of 0.2 percentage points.

In addition, we have examined whether our results are sensitive to controlling for local variation in house prices. Based on ZIP codes at the time of hire, we match employees to Zillow's database of historical median home prices by ZIP code,[36] and we divide the median home price at each point in time by the employee's starting annual salary. In untabulated regressions, we find that the results in Table 2 are qualitatively unchanged when we augment our set of control variables to include (1) the level of the local house price variable at the time the outcome variable is measured; (2) the mean annual percentage change in the local house price variable over the previous three years; (3) the cumulative percentage change in the local house price variable over the previous three years; or (4) the cumulative percentage change in the local house price variable since the beginning of the sample period.

VI. Automatic enrollment effects on subpopulations

In this section, we analyze how automatic enrollment affects subpopulations that are likely to have especially large treatment effects on TSP contributions. Madrian and Shea (2001) find that in their sample, automatic enrollment has the largest contribution effects on those with low incomes, the young, blacks, and Hispanics. Therefore, we estimate treatment effects for

[36] The Zillow estimates of median home prices are based on single family residences, condominiums, and housing cooperatives. We drop individuals who are not successfully matched to the Zillow database. Valid ZIP codes at hire are available for 94% of pre-AE and 94% of post-AE employees. The Zillow data do not cover all ZIP codes; among employees with valid ZIP codes at hire, 71% of the pre-AE cohort and 77% of the post-AE cohort are successfully matched.

these groups in our sample, as well as for those who have only a high school education and those whose credit score at baseline is below 620 (approximately the bottom quintile of our sample).

For brevity, we focus on effects at 43-48 months of tenure.[37] The first row in Table 6 shows that indeed, these subpopulations' contributions respond especially strongly to automatic enrollment. Whereas the effect on the overall sample is 4.1% of first-year salary, the point estimates for the subpopulations in Table 6 range from 4.2% for employees less than 30 years old to 7.5% for those with a starting annualized salary less than $34,000 or those with baseline credit score below 620.

In the fifth row, we see that automatic enrollment does not have a statistically significant effect on D1 for any subpopulation. However, our estimates are imprecise, and one of the positive point estimates—3.8% for those with low credit scores—is large. The automatic enrollment effect on PNW1 is positive for all groups and statistically significant for four out of six groups. The significant effects have point estimates that are larger than the 4.7% effect on PNW1 found for the entire sample, but their 95% confidence intervals all contain 4.7%.

Lack of statistical power also plagues our PNW2 and PNW3 estimates. Only blacks exhibit a statistically significant increase in PNW2, and no group exhibits a significant effect on PNW3. However, every group's 95% confidence interval for the automatic enrollment effect on PNW2 or PNW3 includes the corresponding point estimate for the entire sample (3.6% for PNW2 and 1.4% for PNW3). More precisely estimated are effects on Vantage scores, which are insignificant and small in magnitude for all groups.

VII. Conclusion

Automatic enrollment in the TSP at a 3% of income default contribution rate is successful at increasing contributions to the TSP. At 43-48 months of tenure, this policy raises cumulative contributions to the TSP by 4.1% of first-year annualized salary. We find that little of this accumulation is offset by increased debt excluding first mortgages and auto debt, and there is no impact on credit scores or debt in third-party collections. Our primary regression specification also finds no statistically significant increases in auto and first mortgage debt. However, alternative specifications do find statistically significant increases, and the confidence intervals

[37] Online Appendix Table B7 shows the results at 43-48 months of tenure using the alternative regression specification that does not control for interactions between tenure and demographics.

on our estimates of the first mortgage debt effect are wide, so automatic enrollment may raise these latter two types of debt.

The implications of higher secured debt balances for net worth depend on what is happening to non-TSP assets on the household balance sheet, which we do not observe. Larger samples and a more complete balance sheet perspective will be necessary before we fully understand the impact of automatic enrollment.

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Table 1. Comparison of pre- and post-automatic enrollment hire cohorts

	Pre-AE (Aug ’09 – Jul ’10 hires)	Post-AE (Aug ’10 – Jul ’11 hires)	Difference	*p*-value of difference
Avg. starting salary	$56,418	$55,825	-593	0.009
Avg. deflated starting salary	$56,963	$55,825	-1138	0.000
Avg. age at hire	39.7	39.9	0.2	0.013
Male	61.2%	61.5%	0.3%	0.411
White	53.2%	56.9%	3.8%	0.000
Black	11.4%	12.2%	0.7%	0.007
Hispanic	4.0%	4.2%	0.2%	0.315
Asian	3.6%	3.5%	-0.1%	0.643
Native American	1.0%	1.0%	0.0%	0.791
Missing race	26.8%	22.2%	-4.6%	0.000
High school only	42.0%	47.1%	5.1%	0.000
Some college, no degree	13.1%	12.2%	-0.9%	0.001
Associate degree	5.4%	4.9%	-0.5%	0.012
Bachelor’s degree	21.9%	18.5%	-3.3%	0.000
Graduate degree	16.6%	16.2%	-0.4%	0.222
Unknown education	1.0%	1.0%	0.0%	0.979
STEM major | college	29.6%	25.4%	-4.2%	0.000
Business major | college	28.5%	27.6%	-0.9%	0.130
Other major | college	41.9%	47.0%	5.1%	0.000
Administrative position	31.0%	31.6%	0.7%	0.089
Blue collar position	10.1%	9.1%	-1.0%	0.000
Clerical position	6.9%	8.0%	1.1%	0.000
Professional position	23.8%	20.9%	-2.9%	0.000
Technical position	20.5%	18.4%	-2.1%	0.000
Other position	7.7%	12.0%	4.3%	0.000
Has credit report in six months before hire	83.0%	83.2%	0.1%	0.645
Avg. Vantage Score in six months before hire, conditional on having Vantage Score	686.4	687.4	1.0	0.246
# of obs. (*N*)	32,072	26,802		

Table 2. The effect of automatic enrollment on cumulative TSP contributions and debt components

Each column reports regression-adjusted effects of automatic enrollment on the dependent variable in the column heading. The contribution regressions are estimated according to equation (1), and the credit regressions are estimated according to equation (6). The coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. All dependent variables except for Vantage credit score are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in each regression.

	Cumulative total TSP contributions	Cumulative employee TSP contributions	Debt excluding auto, first mortgage (D1)	Vantage credit score	Auto debt	First mortgage debt
Tenure	--	--	0.003	-0.5	-0.001	0.024
≤ -18			(0.006)	(0.8)	(0.003)	(0.020)
Tenure	--	--	-0.003	0.0	-0.001	-0.007
-17 to -12			(0.004)	(0.6)	(0.003)	(0.016)
Tenure	--	--	-0.003	-0.1	0.000	-0.010
-11 to -6			(0.003)	(0.4)	(0.002)	(0.011)
Tenure	0.004**	0.001**	-0.001	0.3	0.001	0.021
1 to 6	(0.000)	(0.000)	(0.003)	(0.5)	(0.002)	(0.012)
Tenure	0.009**	0.003**	-0.005	0.2	0.001	0.004
7 to 12	(0.001)	(0.001)	(0.004)	(0.7)	(0.003)	(0.018)
Tenure	0.014**	0.004**	-0.006	0.5	0.004	0.011
13 to 18	(0.001)	(0.001)	(0.005)	(0.8)	(0.004)	(0.023)
Tenure	0.020**	0.007**	-0.009	0.3	0.004	-0.003
19 to 24	(0.002)	(0.001)	(0.006)	(0.9)	(0.004)	(0.026)
Tenure	0.026**	0.009**	-0.006	0.1	0.005	0.004
25 to 30	(0.002)	(0.002)	(0.007)	(1.0)	(0.005)	(0.029)
Tenure	0.031**	0.011**	-0.005	-0.3	0.009	0.019
31 to 36	(0.003)	(0.002)	(0.008)	(1.1)	(0.005)	(0.032)
Tenure	0.036**	0.012**	-0.003	0.3	0.009	0.019
37 to 42	(0.003)	(0.003)	(0.008)	(1.1)	(0.006)	(0.035)
Tenure	0.041**	0.014**	-0.006	0.1	0.011	0.022
43 to 48	(0.004)	(0.003)	(0.009)	(1.2)	(0.006)	(0.037)
Tenure	0.045**	0.016**	-0.013	1.3	0.007	0.025
49 to 53	(0.005)	(0.004)	(0.010)	(1.4)	(0.007)	(0.043)
# of obs. (*N*)	427,624	427,624	809,385	670,225	809,385	809,385

* Significant at 5% level. ** Significant at 1% level.

Table 3. The effect of automatic enrollment on cumulative TSP contributions and debt components: Alternative specification
Each column reports regression-adjusted effects of automatic enrollment on the dependent variable in the column heading. The contribution regressions are estimated according to equation (1), and the credit regressions are estimated according to equation (6), except both sets of regressions omit the variables controlling for the interaction of demographics with tenure. The coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. All dependent variables except for Vantage credit score are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in each regression.

	Cumulative total TSP contributions	Cumulative employee TSP contributions	Debt excluding auto, first mortgage (D1)	Vantage credit score	Auto debt	First mortgage debt
Tenure	--	--	0.002	-0.5	0.000	0.008
≤ -18			(0.006)	(0.8)	(0.003)	(0.020)
Tenure	--	--	-0.005	-0.1	-0.001	-0.016
-17 to -12			(0.004)	(0.6)	(0.003)	(0.016)
Tenure	--	--	-0.005	-0.1	0.000	-0.016
-11 to -6			(0.003)	(0.4)	(0.002)	(0.011)
Tenure	0.005**	0.002**	0.001	0.2	0.001	0.022
1 to 6	(0.000)	(0.000)	(0.003)	(0.5)	(0.002)	(0.012)
Tenure	0.010**	0.004**	-0.002	0.3	0.002	0.014
7 to 12	(0.001)	(0.001)	(0.004)	(0.7)	(0.003)	(0.018)
Tenure	0.015**	0.005**	-0.002	0.6	0.006	0.027
13 to 18	(0.001)	(0.001)	(0.005)	(0.8)	(0.004)	(0.023)
Tenure	0.020**	0.007**	-0.004	0.4	0.006	0.015
19 to 24	(0.002)	(0.001)	(0.006)	(0.9)	(0.004)	(0.026)
Tenure	0.026**	0.009**	0.001	0.1	0.010	0.029
25 to 30	(0.002)	(0.002)	(0.007)	(1.0)	(0.005)	(0.029)
Tenure	0.031**	0.011**	0.004	-0.1	0.015**	0.050
31 to 36	(0.003)	(0.002)	(0.008)	(1.1)	(0.005)	(0.032)
Tenure	0.036**	0.012**	0.007	0.5	0.016**	0.054
37 to 42	(0.003)	(0.003)	(0.008)	(1.1)	(0.006)	(0.035)
Tenure	0.041**	0.014**	0.009	0.2	0.020**	0.074
43 to 48	(0.004)	(0.003)	(0.009)	(1.2)	(0.006)	(0.038)
Tenure	0.045**	0.015**	0.003	1.4	0.017*	0.094*
49 to 53	(0.005)	(0.004)	(0.010)	(1.4)	(0.007)	(0.043)
# of obs. (*N*)	427,624	427,624	809,385	670,225	809,385	809,385

* Significant at 5% level. ** Significant at 1% level.

Table 4. The effect of automatic enrollment on D1 subcomponents

Each column reports coefficients from a regression estimated according to equation (6) whose dependent variable is in the column heading. All dependent variables are normalized by first-year annualized salary. The coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in each regression.

	HELOC revolving	Non-HELOC revolving	Other installment loans	Second mortgages	Student loans	External collections	Residual debt
Tenure ≤ -18	0.005 (0.003)	0.002 (0.002)	-0.003 (0.003)	0.004 (0.003)	-0.004 (0.003)	-0.001 (0.001)	0.001 (0.001)
Tenure -17 to -12	0.001 (0.002)	0.000 (0.001)	-0.003 (0.002)	0.000 (0.002)	-0.001 (0.002)	-0.001 (0.001)	0.000 (0.000)
Tenure -11 to -6	0.000 (0.001)	0.001 (0.001)	-0.003 (0.002)	-0.001 (0.001)	0.000 (0.001)	-0.001* (0.000)	0.000 (0.000)
Tenure 1 to 6	0.000 (0.001)	0.000 (0.001)	-0.003 (0.002)	0.002 (0.001)	0.001 (0.001)	0.000 (0.000)	0.000 (0.000)
Tenure 7 to 12	0.001 (0.002)	-0.001 (0.001)	-0.006* (0.003)	0.002 (0.002)	-0.001 (0.002)	0.000 (0.001)	0.001 (0.000)
Tenure 13 to 18	0.000 (0.002)	0.001 (0.002)	-0.006 (0.003)	0.002 (0.002)	-0.002 (0.002)	0.000 (0.001)	0.001 (0.001)
Tenure 19 to 24	-0.001 (0.002)	0.001 (0.002)	-0.006 (0.004)	0.000 (0.003)	-0.005 (0.003)	0.000 (0.001)	0.001 (0.001)
Tenure 25 to 30	-0.001 (0.003)	0.002 (0.003)	-0.005 (0.004)	0.002 (0.003)	-0.004 (0.003)	0.000 (0.001)	0.001* (0.001)
Tenure 31 to 36	-0.003 (0.003)	0.003 (0.003)	-0.003 (0.004)	0.002 (0.003)	-0.004 (0.004)	-0.001 (0.001)	0.001* (0.001)
Tenure 37 to 42	-0.001 (0.003)	0.003 (0.003)	-0.005 (0.004)	0.003 (0.004)	-0.003 (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 43 to 48	-0.003 (0.004)	0.003 (0.003)	-0.004 (0.004)	0.002 (0.004)	-0.004 (0.005)	-0.001 (0.001)	0.002* (0.001)
Tenure 49 to 53	-0.002 (0.004)	0.000 (0.004)	-0.009 (0.005)	0.004 (0.004)	-0.008 (0.006)	0.000 (0.001)	0.001 (0.001)
# of obs. (*N*)	809,385	809,385	809,385	809,385	809,385	809,385	809,385

* Significant at 5% level. ** Significant at 1% level.

Table 5. The effect of automatic enrollment on debt and pseudo-net worth

The first three columns report coefficients from regressions estimated according to equation (6), where the dependent variable is in the column heading. D1 is debt excluding auto loans and first mortgages, D2 is auto loans plus D1, and D3 is first mortgages plus D2. The coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. The next three columns report treatment effect estimates on three measures of net wealth accumulation that are obtained by subtracting treatment effects on debt from the treatment effects on cumulative total TSP contributions reported in Table 2: PNW1, PNW2, and PNW3 subtract D1, D2, or D3, respectively. All dependent variables are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in the debt regressions. The PNW1-PNW3 results are derived from the 809,385 person-months used in the debt regressions and the 427,624 person-months used in the contribution regressions.

	D1	D2	D3	PNW1	PNW2	PNW3
Tenure	0.003	0.003	0.027	--	--	--
≤ -18	(0.006)	(0.007)	(0.022)			
Tenure	-0.003	-0.004	-0.011	--	--	--
-17 to -12	(0.004)	(0.005)	(0.017)			
Tenure	-0.003	-0.003	-0.013	--	--	--
-11 to -6	(0.003)	(0.003)	(0.012)			
Tenure	-0.001	0.000	0.022	0.005	0.004	-0.018
1 to 6	(0.003)	(0.004)	(0.013)	(0.003)	(0.004)	(0.013)
Tenure	-0.005	-0.003	0.001	0.014**	0.013*	0.008
7 to 12	(0.004)	(0.006)	(0.019)	(0.004)	(0.005)	(0.018)
Tenure	-0.006	-0.001	0.010	0.020**	0.015*	0.004
13 to 18	(0.005)	(0.007)	(0.024)	(0.005)	(0.007)	(0.024)
Tenure	-0.009	-0.005	-0.008	0.029**	0.025**	0.028
19 to 24	(0.006)	(0.008)	(0.027)	(0.006)	(0.008)	(0.027)
Tenure	-0.006	-0.001	0.003	0.032**	0.027**	0.023
25 to 30	(0.007)	(0.009)	(0.031)	(0.007)	(0.009)	(0.031)
Tenure	-0.005	0.004	0.023	0.036**	0.027*	0.008
31 to 36	(0.008)	(0.010)	(0.034)	(0.008)	(0.010)	(0.035)
Tenure	-0.003	0.006	0.025	0.039**	0.030**	0.011
37 to 42	(0.008)	(0.011)	(0.037)	(0.009)	(0.011)	(0.038)
Tenure	-0.006	0.005	0.027	0.047**	0.036**	0.014
43 to 48	(0.009)	(0.011)	(0.040)	(0.010)	(0.012)	(0.042)
Tenure	-0.013	-0.006	0.019	0.059**	0.051**	0.027
49 to 53	(0.010)	(0.013)	(0.046)	(0.012)	(0.014)	(0.047)
# of obs. (*N*)	809,385	809,385	809,385	--	--	--

* Significant at 5% level. ** Significant at 1% level.

Table 6. The effect of automatic enrollment on subpopulations at 43-48 months of tenure

Each cell except those in the rows labeled PNW1-PNW3 contains an estimate from its own separate regression representing the treatment effect of automatic enrollment on the variable indicated in the row label at 43-48 months of tenure for the group in the column header. The contribution regressions are estimated according to equation (1), and the credit regressions are estimated according to equation (6). The cells in the PNW1-PNW3 rows show the difference between the automatic enrollment effect on cumulative total TSP contributions and its effect on D1-D3, respectively. D1 is debt excluding auto loans and first mortgages, D2 is auto loans plus D1, and D3 is first mortgages plus D2. All dependent variables except for Vantage credit score are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses.

	Salary < $34K	Age < 30	High school only	Baseline Vantage < 620	Black	Hispanic
Cumulative total TSP contributions	0.075**	0.042**	0.056**	0.075**	0.067**	0.058**
	(0.009)	(0.008)	(0.006)	(0.007)	(0.012)	(0.020)
Cumulative employee TSP contributions	0.029**	0.014*	0.021**	0.034**	0.026**	0.029
	(0.007)	(0.006)	(0.005)	(0.005)	(0.009)	(0.016)
Auto debt	0.052*	0.026	0.026*	0.034	-0.008	-0.015
	(0.023)	(0.014)	(0.011)	(0.018)	(0.022)	(0.036)
First mortgage debt	0.137	-0.080	0.117*	-0.012	0.010	0.146
	(0.117)	(0.086)	(0.059)	(0.091)	(0.122)	(0.195)
D1 (debt excl. auto and first mortgages)	0.004	-0.021	0.006	0.038	-0.026	-0.010
	(0.031)	(0.017)	(0.014)	(0.029)	(0.033)	(0.047)
D2	0.057	0.005	0.033	0.073*	-0.034	-0.025
	(0.039)	(0.023)	(0.018)	(0.035)	(0.041)	(0.059)
D3	0.194	-0.075	0.150*	0.060	-0.024	0.121
	(0.129)	(0.091)	(0.064)	(0.104)	(0.136)	(0.211)
PNW1	0.070*	0.063**	0.049**	0.037	0.094**	0.067
	(0.032)	(0.020)	(0.016)	(0.030)	(0.036)	(0.050)
PNW2	0.018	0.037	0.023	0.002	0.101*	0.082
	(0.042)	(0.025)	(0.020)	(0.037)	(0.043)	(0.061)
PNW3	-0.119	0.116	-0.094	0.015	0.092	-0.064
	(0.126)	(0.089)	(0.064)	(0.103)	(0.140)	(0.217)
Vantage credit score	3.8	-1.9	0.2	4.8	-1.6	6.5
	(3.4)	(2.9)	(1.9)	(3.1)	(4.1)	(7.8)
# of employees at 43-48 months	5,882	7,358	15,576	6,572	4,009	1,448

* Significant at 5% level. ** Significant at 1% level.

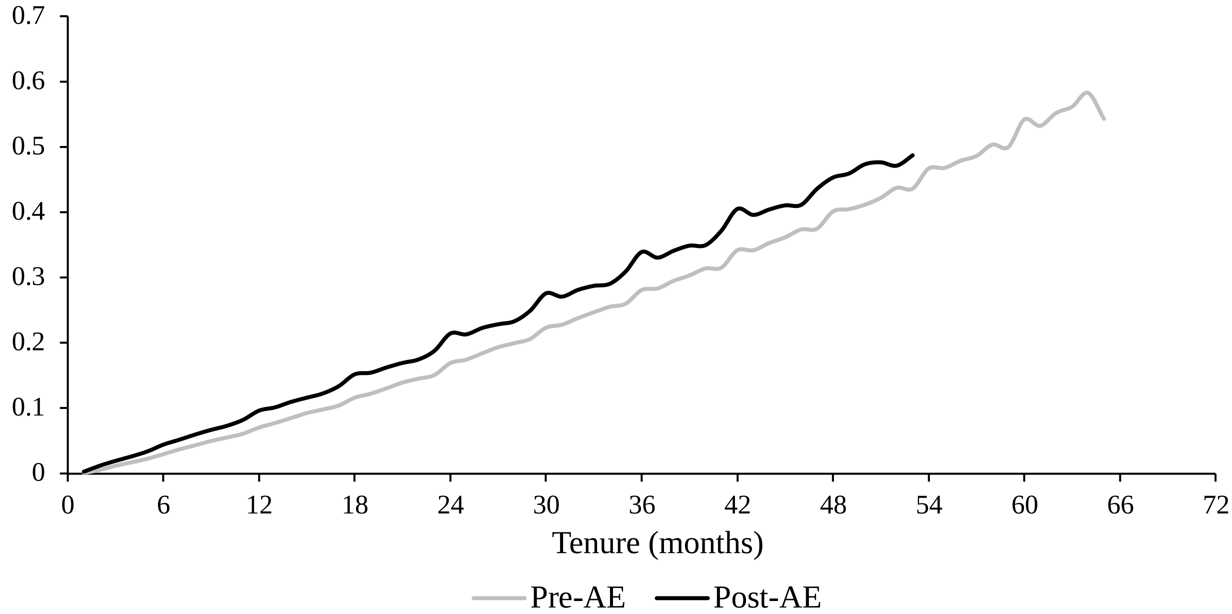

Figure 1. The ratio of cumulative total TSP contributions to annualized first-year pay. Every point in the graphed series corresponds to individuals who reached the tenure level indicated on the horizontal axis in a June or a December. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The sample at each tenure level consists of all civilians employed by the Army at that time, excluding re-hires.

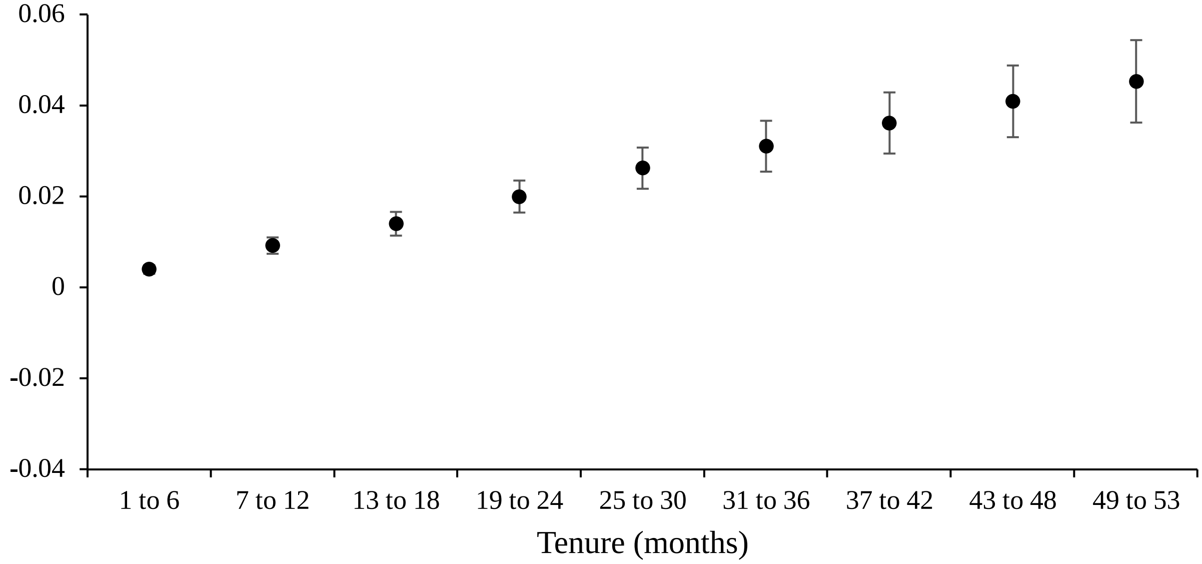

Figure 2. The effect of automatic enrollment on cumulative total TSP contributions to annualized first-year pay ratio. The estimates come from the regression in column 1 of Table 2. Point estimates and 95% confidence intervals are shown.

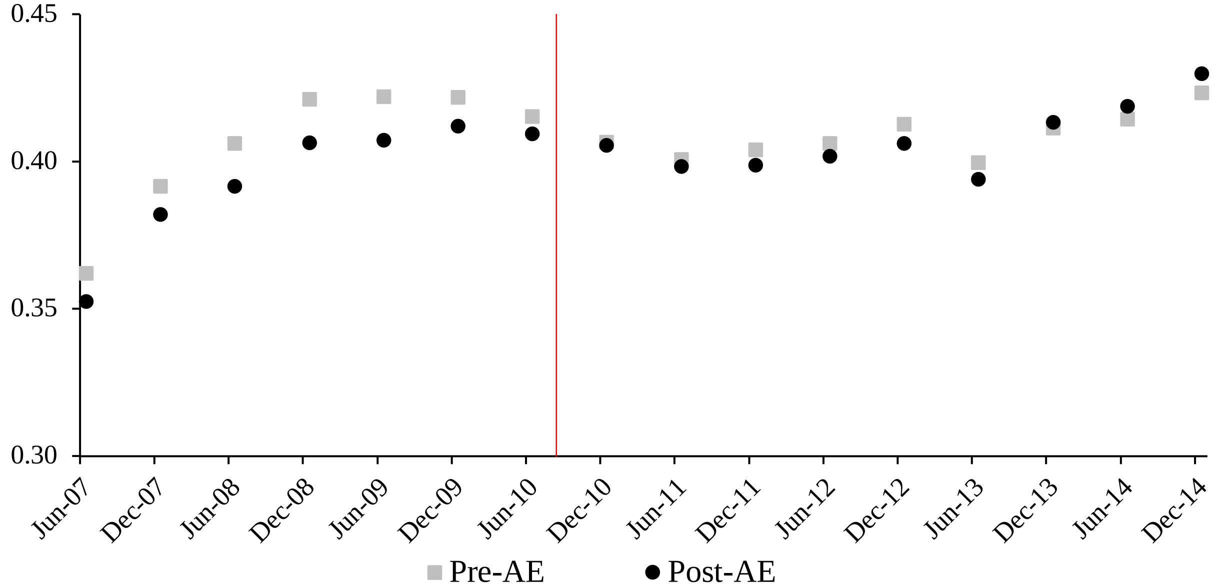

Figure 3. Debt balance excluding auto debt and first mortgages normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

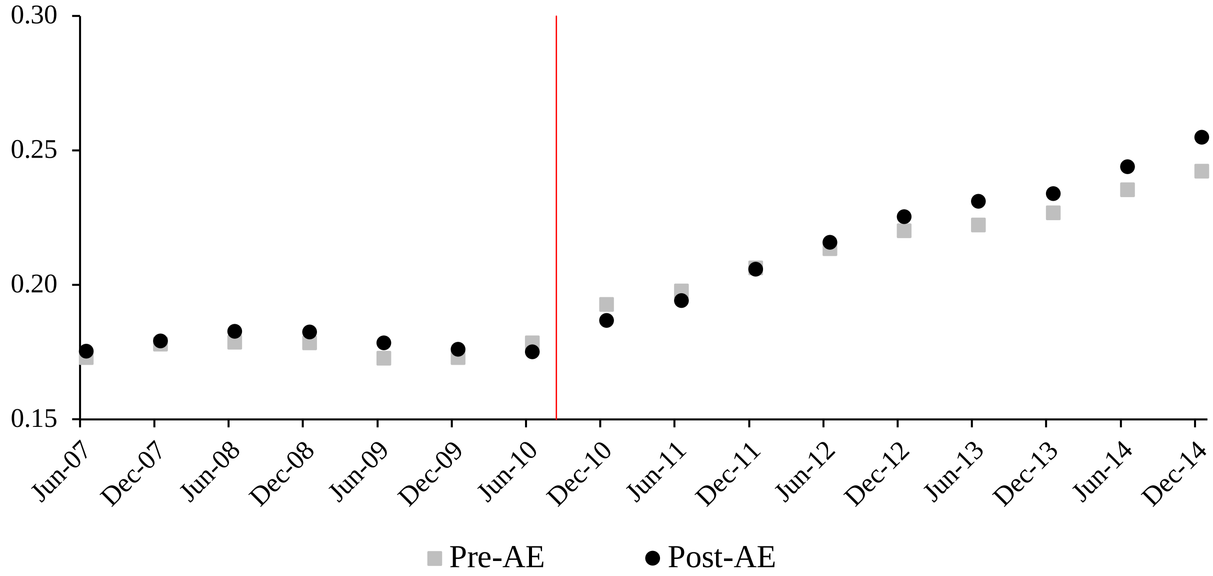

Figure 4. Auto loan and lease balance normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

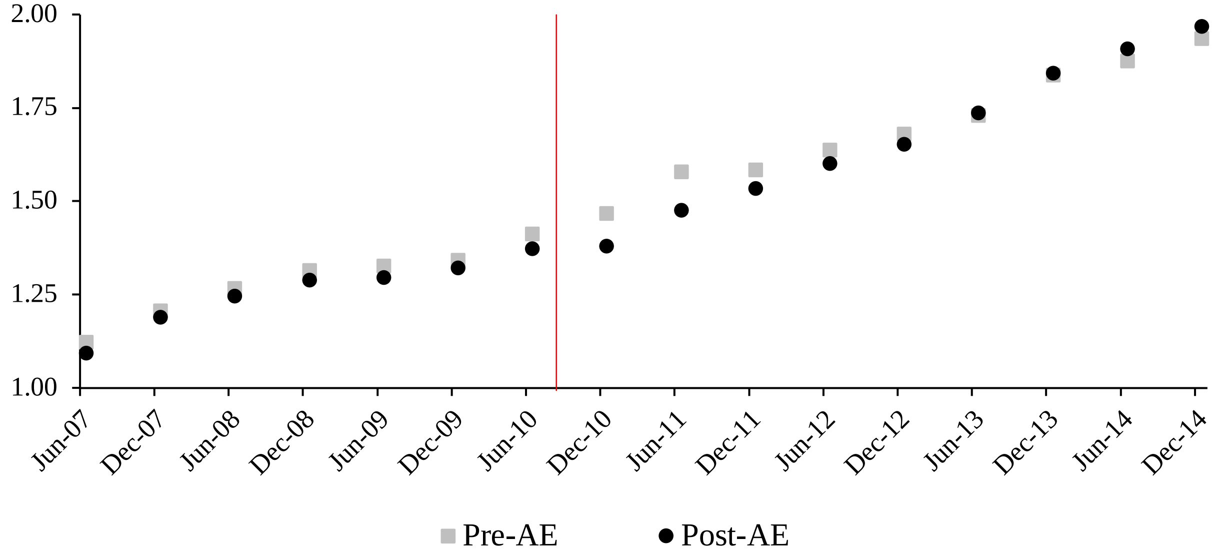

Figure 5. First mortgage balance normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

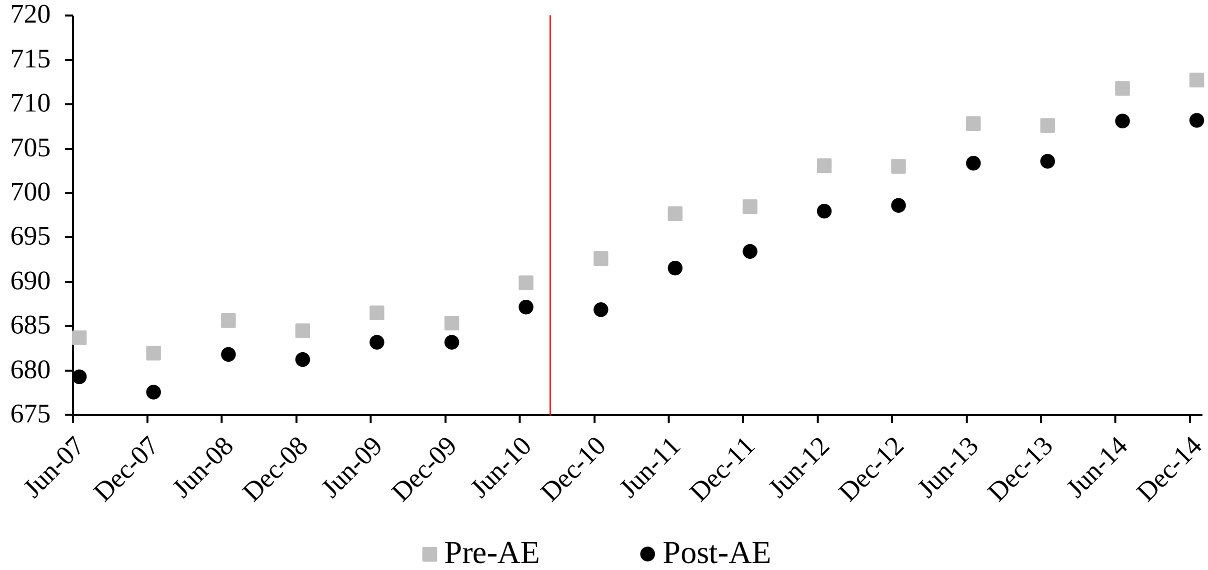

Figure 6. Vantage score at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

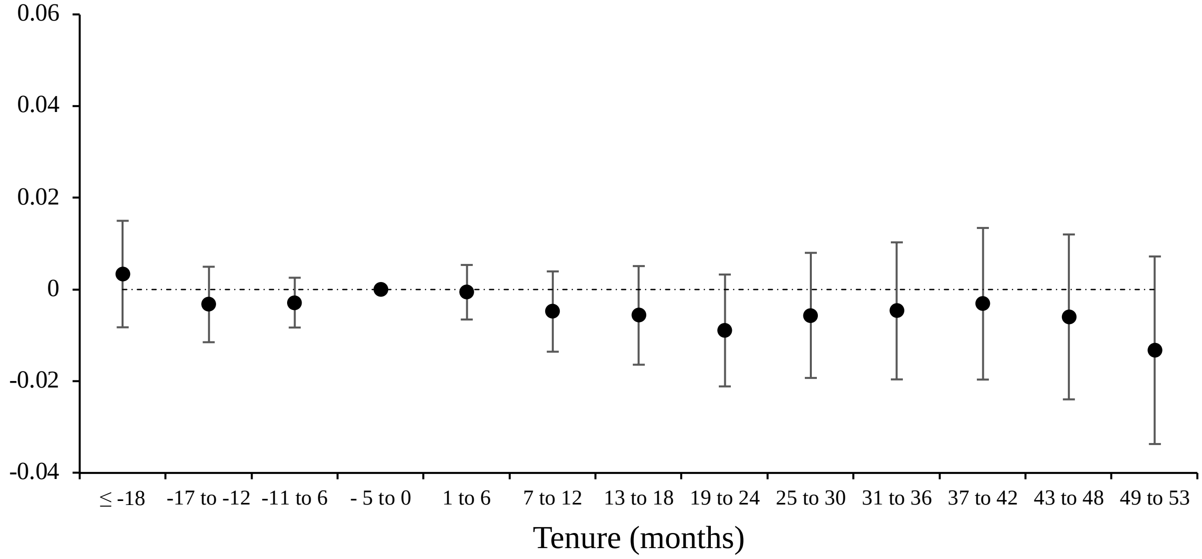

Figure 7. The effect of automatic enrollment on debt balance excluding first mortgages and auto loans normalized by annualized first-year pay (D1). The estimates come from the regression in column 3 of Table 2. Point estimates and 95% confidence intervals are shown.

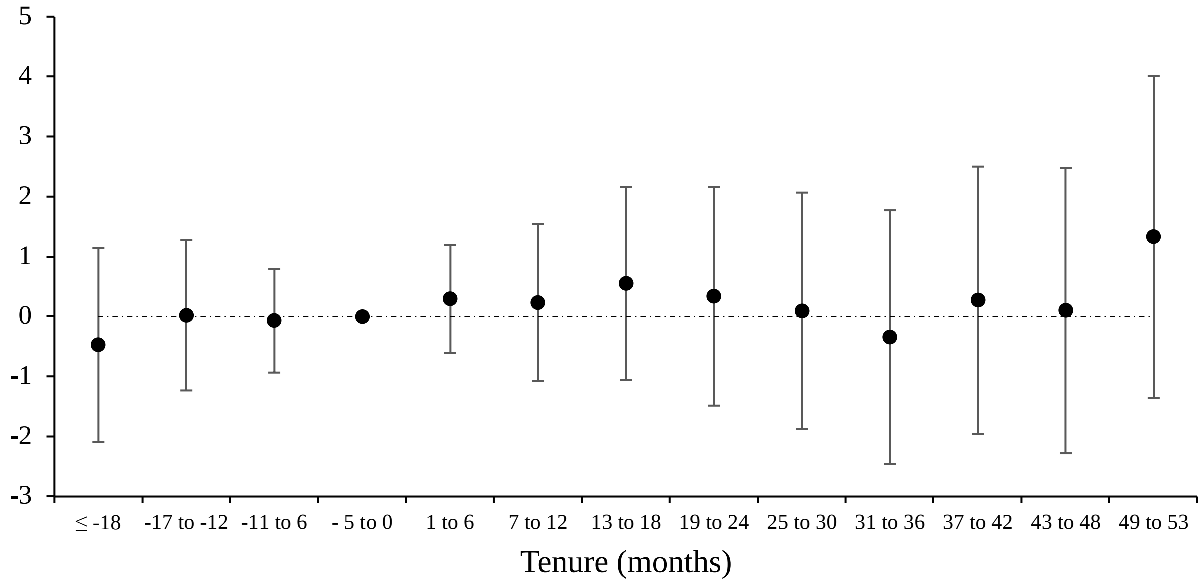

Figure 8. The effect of automatic enrollment on Vantage score. The estimates come from the regression in column 4 of Table 2. Point estimates and 95% confidence intervals are shown.

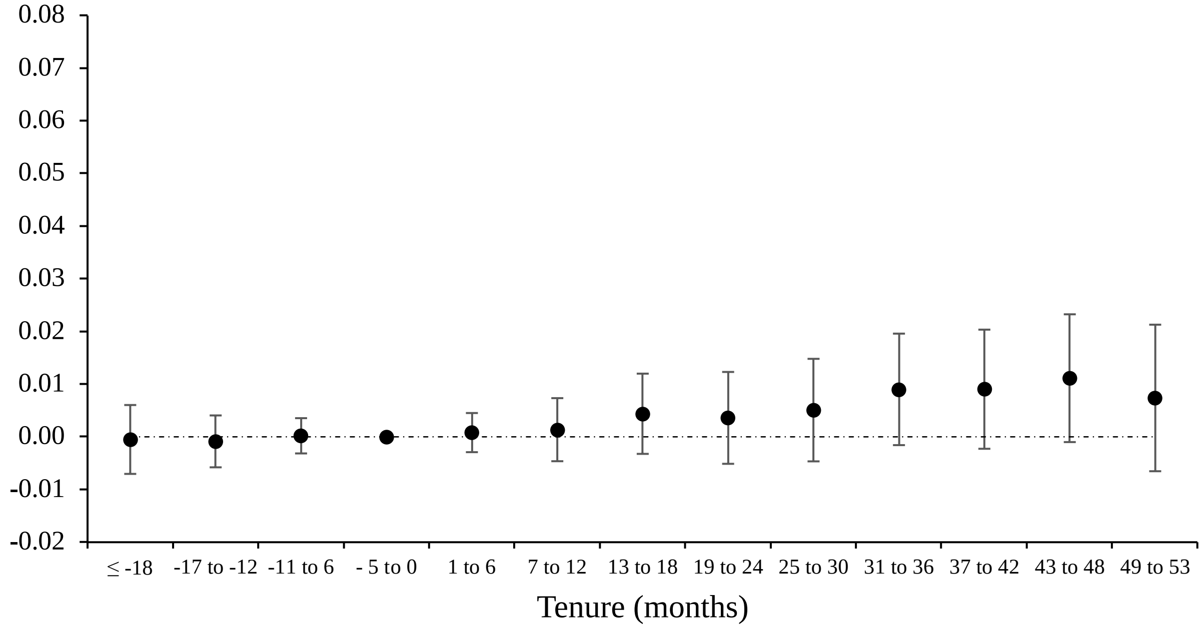

Figure 9. The effect of automatic enrollment on auto loan balance normalized by annualized first-year pay. The estimates come from the regression in column 5 of Table 2. Point estimates and 95% confidence intervals are shown.

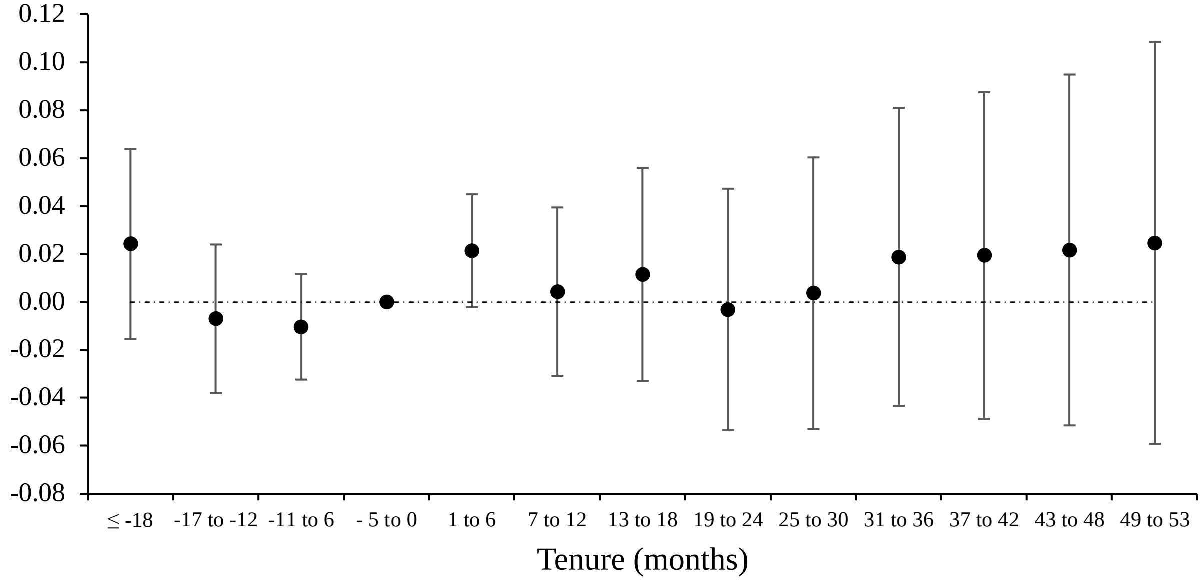

Figure 10. The effect of automatic enrollment on first mortgage balance normalized by annualized first-year pay. The estimates come from the regression in column 6 of Table 2. Point estimates and 95% confidence intervals are shown.