Borrowing to Save?
The Impact of Automatic Enrollment on Debt

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January 7, 2020

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.

This research was supported by grants from the National Institutes of Health (grants P01AG005842, P30AG034532, and R01AG021650), the Pershing Square Fund for Research on the Foundations of Human Behavior, the Smith Richardson Foundation, and the U.S. Social Security Administration (grant RRC0809840007), funded as part of the Retirement Research Consortium. We thank Brian Baugh, Brigham Frandsen, John Friedman, Ori Heffetz, Ted O'Donoghue, Daniel Reck, Jonathan Reuter, Barak Richman, Nick Roussanov, David Scharfstein, Richard Thaler, Jack VanDerhei, and audience members at the AEA Annual Meeting, BYU, Carnegie Mellon, CFPB, Cornell, the Miami Behavioral Finance Conference, MIT, NBER, NYU, the RAND Behavioral Finance Forum, SMU, Stanford, Texas Tech, UCL, University of Nebraska Lincoln, University of Pennsylvania, and Yale for helpful comments. We are grateful for the research assistance of Ross Chu, Jonathan Cohen, Justin Katz, Peter Maxted, and Charles Rafkin. Luke Gallagher from the U.S. Army Office of Economic and Manpower Analysis provided critical assistance in preparing the data. To access the data studied in this paper, the researchers entered a data use agreement that gave the U.S. Army Office of Economic and Manpower Analysis the right to review the paper prior to public release to ensure that no individuals were identifiable, that the data were correctly described, and that no policies or procedures were violated. This research was reviewed by the Harvard and NBER IRBs and determined to be "not human subjects research." Beshears, Choi, Laibson, and Madrian have received additional grant support from the TIAA Institute and the National Employment Savings Trust (NEST). They have received research data from Alight Solutions. Beshears, Choi, and Madrian are TIAA Institute Fellows. Beshears is an advisor to and equity holder in Nutmeg Saving and Investment, a robo-advice asset management company. He has received research data from Voya Financial and the Commonwealth Bank of Australia. Choi has no additional disclosures. Laibson has received additional grant support from the Russell Sage Foundation. He is a member of the Russell Sage Foundation Behavioral Economics Roundtable and a member of the Federal Reserve Bank of Philadelphia Consumer Finance Institute Academic Advisory Board. He has received research data from the Financial Conduct Authority (U.K.). He has received honoraria to give talks at events hosted by Research Affiliates, The American Council of Life Insurers, and Hartford Insurance. Madrian is a member of the Consumer Financial Protection Bureau (CFPB) Academic Research Council and a member of the Defined Contribution Institutional Investment Association (DCIIA) Academic Advisory Council. Skimmyhorn has received compensation from the Financial Industry Regulatory Authority (FINRA). See the authors' websites for a complete list of outside activities. The views expressed here are those of the authors and do not reflect the views or position of the United States Military Academy, the Department of the Army, the Department of Defense, the Social Security Administration, any agency of the federal government, Harvard, Yale, BYU, William & Mary, or the NBER.

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. Its effectiveness at doing so depends not only on whether it increases retirement plan contributions, but also on whether it induces changes in other parts of household balance sheets. The assumption among advocates of automatic enrollment has been that the incremental retirement plan contributions caused by automatic enrollment are mostly financed by decreased consumption (e.g., Thaler, 1994; Beshears et al., 2006). However, no evidence has yet emerged that rules out alternative possibilities. For example, automatic enrollment might lead to slower growth in the balances of other asset accounts, essentially shifting balances from those other accounts to retirement accounts and dampening the effect of automatic enrollment on net wealth accumulation. Alternatively, automatic enrollment might lead to faster growth in debt, which would at least partially undo the intended benefit of automatic enrollment. Such an effect would be particularly concerning if automatic enrollment increased high-interest debt balances and financial distress. That possibility is the focus of this paper.

We augment existing analyses of automatic enrollment by studying household *liabilities* and thereby asking whether automatic enrollment affects other balance sheet categories in addition to defined contribution plan balances. Specifically, we link individual employee payroll records to 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

[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).

automatic enrollment for all other U.S. federal civil servants.[2] 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 the 32,072 employees hired in the year prior to the adoption of automatic enrollment to savings and debt outcomes for the 26,802 employees hired in the year after, while controlling for calendar time fixed effects. (We present results from a related regression discontinuity methodology in Online Appendix A.) 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] Our new-hire sample's average income of about $56,000 is much lower than the Gelman et. al sample average income of $89,804, so our study sample is unlikely to have more liquidity (Kaplan, Violante, and Weidner, 2014). For the individuals in our sample who are induced by automatic enrollment to contribute more to the TSP, there is little scope for automatic enrollment to reduce balances in non-retirement liquid asset accounts, but there is likely scope for automatic enrollment to increase borrowing.

We first confirm that automatic enrollment increases contributions to the TSP. 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 and increases cumulative employee contributions since hire by 1.4% of first-year annualized salary. There is, however, considerable heterogeneity in this treatment effect. Up to 19% of employees hired after the

[2] Uniformed members of the armed forces were not automatically enrolled during our sample period, and we omit them from our analysis.

[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 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.

implementation of automatic enrollment would never have participated in the TSP within four years of hire in the absence of automatic enrollment, but do participate under automatic enrollment. If they remained at the 3% default contribution rate for four years, automatic enrollment increased their cumulative employee contributions by 12% of annual pay, generating a reduction in take-home pay that has the potential to trigger increased borrowing and financial distress.[5]

Our main results are for the effect of automatic enrollment on credit scores and debt balances excluding auto debt and first mortgages. At 43-48 months after hire, automatic enrollment's effect on credit scores is a minuscule 0.001 standard deviation *increase*, with a 95% confidence interval of [-0.02 standard deviations, 0.03 standard deviations] that is extremely tight around zero. Derogatory debt balances that have been passed to an external collection agency decrease insignificantly by 0.1% of first-year salary, with a 95% confidence interval of [-0.3%, 0.1%], a further indication that automatic enrollment does not increase financial distress. Total debt balances excluding auto and first mortgage debt fall by 0.6% of first-year salary, with a 95% confidence interval of [–2.4%, 1.2%]. In sum, our evidence does not support the hypothesis that automatic enrollment increases financial distress and costly borrowing.

Our results on auto loans and first mortgages are less conclusive for two reasons. First, because these types of debt are usually originated in order to finance the acquisition of an asset, any increases in their balances have ambiguous implications for net worth—assets typically increase along with liabilities. (We discuss in Section IX a framework for thinking about the possible net worth effects of an origination of secured debt.) Second, because of the high variance of first mortgage balances, we estimate effects on first mortgages with little precision despite our relatively large sample size. We find no significant increase in either kind of debt balance in our main regression specification, although the point estimates are positive. At 43-48 months of tenure, the point estimate of the auto debt balance effect is 1.1% of income (95% confidence interval = [–0.1%, 2.3%]), and the point estimate of the first mortgage balance effect

[5] The large positive treatment effect for these employees is offset in the estimation of the average treatment effect by the negative treatment effect for employees who would have contributed at a higher rate in the absence of automatic enrollment. This latter group of employees is less likely to be close to the margin of financial distress, so even though automatic enrollment increases their take-home pay, they may not exhibit a decrease in financial distress. Because of the latter group, we do not pursue an instrumental variables strategy that estimates the change in debt caused by an incremental dollar in TSP contributions, using automatic enrollment as the instrument. The monotonicity assumption required for interpreting an instrumental variable estimate as a local average treatment effect among compliers is not satisfied.

is 2.2% of income (95% confidence interval = [–5.1%, 9.5%]). However, the auto and first mortgage debt effects are positive and statistically significant in some alternative (non-benchmark) specifications.

Our paper is related to Blumenstock, Callen, and Ghani (2018), who conduct 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. Choukhmane (2019) documents a non-debt 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. Goda et al. (2018) find that the asset allocation default in the TSP can have unintended effects on employees' contribution rate decisions (see also Mitchell et al., 2009).[6]

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). Chetty et al. (2014) find that a one percentage point increase in mandatory contributions to Danish retirement accounts results in a 0.8 percentage point increase in the total savings rate.[7] At an economy-wide level, policies that increase retirement plan contributions may also affect other household balance sheet elements because greater household savings affect the overall financial system, for example by promoting capital market development (Scharfstein, 2018).

The remainder of the paper proceeds as follows. Section I summarizes the relevant institutional details of the TSP, and Section II describes the natural experiment we exploit. Section III describes our data, and Section IV compares the two hire cohorts that are the focus of our analysis. Section V documents our empirical findings on the effect of automatic enrollment on TSP contributions. Section VI describes the econometric methodology we use to estimate

[6] Other papers finding that nudges do not have their intended consequences include Schultz et al. (2007), Wisdom, Downs, and Loewenstein (2010), Costa and Kahn (2013), Beshears et al. (2015), Rogers and Feller (2016), Keys and Wang (2016), Goldin et al. (2017), Medina (2018), and Allcott and Kessler (2019).

[7] 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.

automatic enrollment's effect on credit outcomes. We show the main results on credit scores and debt excluding auto loans and first mortgages in Section VII. In Section VIII, we show additional results on auto loans and first mortgages. Section IX develops a framework for thinking about the implications an increase in auto or first mortgage debt could have for household net worth. Section X presents estimates of automatic enrollment's effect on contributions net of debt, and Section XI presents estimates of effects on subpopulations that are likely to have especially large treatment effects on contributions. Section XII concludes. Online Appendix A shows results from an alternative estimation strategy using a regression discontinuity design, and Online Appendix B contains supplementary tables and figures.[8]

I. Thrift Savings Plan institutional details

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.

[8] 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 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.

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.[9] An employee 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.

II. The natural experiment

On August 1, 2010, the U.S. federal government implemented automatic enrollment for all newly-hired 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), and one of the 25 largest employers in

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[9] 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."

(https://www.tsp.gov/PlanParticipation/LoansAndWithdrawals/inservicewithdrawals/financialHardship.html, accessed July 7, 2017) the U.S. (WorldAtlas, 2017). 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.[10] Second, federal government furloughs reduced pay for a period of time in 2013. For the six weeks beginning on July 8, 2013, most Army civilian employees received one fewer 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 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 V. 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

[10] 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.

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.

III. 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).[11] For the purposes of determining whether an employee was subject to automatic enrollment, we use the year and month during which the employee became eligible for FERS[12], 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).[13] 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

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[11] 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.

[12] 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.

[13] 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.

and TSP contributions in the missing month were the same as in the closest preceding non-missing month.[14]

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 V, 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 using names and Social Security numbers.[15] The credit data consist of biannual month-end cross-sections from June 2007 to December 2014. In each cross-section we observe debt balances[16], 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.). However, not all lenders report to the credit bureau. For example, we do not observe payday loans or title loans. We do 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.[17]

[14] 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.

[15] 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.

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

[17] 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. 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, and adding these controls has little effect on our main results. 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.

V. 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 Figure B1 shows that automatic enrollment substantially increases savings plan participation at all levels of tenure. Figure 1 shows that automatic enrollment both (1) shifts the distribution of savings plan contribution rates away from zero and toward the automatic enrollment default contribution rate of 3%, and (2) shifts the distribution away from higher contribution rates and toward 3%. Thus, while our analysis in this section focuses on the effect of automatic enrollment on mean contributions, it is important to note that there is significant heterogeneity in treatment effects.

In this paper, our primary savings outcome 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 levels of job tenure. Unlike almost all of the previous literature on automatic enrollment, we also control for calendar time 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.[18] We define the benchmark number of paydays 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 nth calendar month of tenure. We scale the last month's contributions of each individual to approximate how much that

[18] 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 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.

individual would have contributed by tenure month *n* had she experienced the benchmark number of paydays.[19]

Second, as explained in Section II, 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.[20] We do not make an adjustment for the government shutdown in October 2013 because it only shifted pay within the month of October.

Figure 2 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.[21] We see in Figure 2 that the post-AE cohort has higher average cumulative TSP contributions than the pre-AE cohort, with the gap between the two cohorts increasing with tenure.[22] 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.

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

[20] 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.

[21] 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.

[22] 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.

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

We stack all observations into a single regression and estimate the equation

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

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),[23] and $PostAE_i$ is an indicator variable for being in the post-AE cohort. 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 Table 2.

Our main specification additionally controls for interactions between employee demographic characteristics and tenure using the equation

$$y_{i\tau t} = \eta_t + \sum_s [I(\tau \in T_s)(\alpha_s + \beta_s X_i + \gamma_s PostAE_i)] + \epsilon_{i\tau t}, \quad (2)$$

where the only difference relative to the previous regression equation (1) is that the β coefficients on the employee characteristic control variables are allowed to vary by tenure bucket. We prefer this second regression specification to the previous specification because it more flexibly controls for demographic characteristics. In F-tests, the regression coefficients capturing the interactions between demographic characteristics and tenure are jointly highly significant ($p < 0.001$). Nonetheless, we also report results from the first specification because it was the main specification in a previous draft of this paper.

The first column of Table 2 shows the regression-adjusted cumulative contribution effect estimated from equation (1), where demographic × tenure interactions are not controlled for. We find treatment effect estimates that are somewhat smaller than those computed from the raw differences: automatic enrollment raises cumulative contributions by 1.0%, 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.[24]

[23] 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.

[24] 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.

The second column of Table 2 displays estimates of the cumulative contribution effect that additionally control for demographic × tenure interactions, in accordance with equation (2). These estimates are also shown in graphical form in Figure 3. Controlling for demographic differences at each tenure level makes almost no difference to the point estimates or the standard errors.[25]

The third and fourth columns of Table 2 show the regression-adjusted estimates of the effect of automatic enrollment on cumulative *employee* contributions, which exclude the employer match and Agency Automatic (1%) Contributions. As with total contributions, the estimates are nearly identical whether or not demographic × tenure interactions are controlled for.[26] The effect on employee contributions is less than half of the effect on total contributions; the point estimate under either specification is 1.4% of first-year salary at 43-48 months of tenure.

One might have expected the effect on total contributions 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

[25] As a robustness check, we have also estimated regressions that do not control for demographic × tenure interactions but instead control for demographic × calendar time interactions. As shown in Appendix Table B8, the results are very similar.

[26] The estimates are also nearly identical if we do not control for demographic × tenure interactions but instead control for demographic × calendar time interactions. See Appendix Table B8.

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*.[27] Using this approach, we find that hardship withdrawals are rare. Subtracting our upper-bound 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.

VI. Econometric methodology for estimating automatic enrollment effects on credit outcomes

We wish to estimate the effect of automatic enrollment on credit 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 a regression specification featuring additive calendar time effects. In contrast, when credit scores or debt levels are the outcome variable, additive calendar time effects seem to be a reasonable specification. This judgment is based on Figures 4 through 7, 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:

$$y_{i\tau t} = \zeta_i + \eta_t + \sum_s [I(\tau \in T_s)(\alpha_s + \gamma_s PostAE_i)] + \epsilon_{i\tau t}, \tag{3}$$

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, and $PostAE_i$ is an indicator variable for employee *i* being in the post-AE

[27] 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.

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.[28] 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.

Our main specification additionally controls for interactions between tenure and employee demographics:

$$y_{i\tau t} = \zeta_i + \eta_t + \sum_s [I(\tau \in T_s)(\alpha_s + \beta_s X_i + \gamma_s PostAE_i)] + \epsilon_{i\tau t}, \tag{4}$$

where X_i is a vector of control variables measured as of hire (the same variables as in contribution regression equation (2)). We prefer the specification in equation (4) because it more flexibly controls for demographic characteristics than the specification in equation (3). The additional regression coefficients in equation (4) are jointly highly significant ($p < 0.001$) in F-tests. We report results from regressions that use equation (3) for comparability with a previously circulated draft of this paper, which used equation (3) as its main specification.

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 time immediately 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:

$$E(\Delta y_{i\tau t}) = (\alpha_s + \beta_s X_i - \alpha_{s-1} - \beta_{s-1} X_i) + (\eta_t - \eta_{t-1}) \tag{5}$$

[28] We also exclude one calendar time dummy to avoid multicollinearity.

$$E(\Delta y_{i',\tau-1,t}) = (\alpha_{s-1} + \beta_{s-1}X_{i'} - \alpha_{s-2} - \beta_{s-2}X_{i'}) + (\eta_t - \eta_{t-1}). \quad (6)$$

Taking the difference between (5) and (6) eliminates the calendar time effects:

$$E(\Delta y_{i\tau t}) - E(\Delta y_{i',\tau-1,t}) = (\alpha_s + \beta_s X_i - \alpha_{s-1} - \beta_{s-1}X_i) \\ -(\alpha_{s-1} + \beta_{s-1}X_{i'} - \alpha_{s-2} - \beta_{s-2}X_{i'}). \quad (7)$$

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[29], 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 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 (7) 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.

VII. Effect of automatic enrollment on credit scores and debt excluding auto loans and first mortgages

We begin by examining automatic enrollment's effect on a summary measure of creditworthiness, the Vantage credit score. The first column of Table 3 shows the effect of automatic enrollment on Vantage scores, conditional on having a Vantage score, controlling for only person and calendar time fixed effects.[30] 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 effects continues after hire, all the way out to 49-53 months of tenure, and the point estimates lie between –0.1 and 1.4 points across the positive tenure spectrum. The second column of Table 3 and Figure 8 show that additionally controlling for demographic × tenure interactions barely moves the point estimates.[31] At 43-48 months of

[29] The outcome variable y is not restricted to be unaffected by demographics in this baseline tenure bucket, since equation (4) includes an individual fixed effect.

[30] 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 with credit outcome variables, so those regressions have more observations than the Vantage score regressions.

[31] The results are very similar if we do not control for demographic × tenure interactions but instead control for demographic × calendar time interactions. See Appendix Table B8.

tenure, automatic enrollment is estimated to increase Vantage scores by 0.1 points, with a 95% confidence interval of [-2.3, 2.5].

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 at 43-48 months that is no more than 0.001 standard deviations in magnitude, with the lower end of the 95% confidence interval reaching only –0.02 standard deviations. In sum, there is no indication that automatic enrollment creates any meaningful change in creditworthiness.

Our primary debt balance outcome of interest is debt excluding auto loans and first mortgages, normalized by annualized salary in the first year of tenure. These are debts that are typically used to purchase non-durables, services, or durables that have little resale value—and hence are associated with decreases in financial net worth. We include non-derogatory balances (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) on home equity lines of credit (HELOCs), non-HELOC revolving debt, other installment debt, second mortgages, student loans, and residual debt that does not belong to the other categories. We also include derogatory debt that has been passed to an external collection agency.[32] 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. 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. Creditors that do not report to the credit bureau, such as payday lenders, are excluded from our debt measure.

The third column of Table 3 shows the treatment effects on debt balances when we do not control for demographic × tenure interactions. As with the credit score regressions, there is no

[32] 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.

significant effect of automatic enrollment estimated before hire, which provides a measure of confidence in the validity of the empirical strategy. After hire, automatic enrollment never has a significant effect on debt balances. At 43-48 months of tenure, the point estimate of the treatment effect is 0.9% of first-year annualized salary. In the fourth column of Table 3 and in Figure 9, we see that the estimated effects remain insignificant once we additionally control for demographic × tenure interactions. Relative to the less comprehensive specification, the point estimates move in the negative direction, but the two sets of point estimates lie within each other's 95% confidence intervals at each tenure level. With the additional controls, 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%].[33]

Table 4 displays the automatic enrollment effect separately for each sub-component 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-independent) hypothesis tests shown in the table, so we would expect some of these coefficients to be statistically significant at conventional levels by chance. Importantly, we see no increase in debt in third-party collections, reinforcing the conclusion from the credit score analysis that automatic enrollment has no impact on the probability of financial distress.

VIII. Effect of automatic enrollment on auto debt and first mortgages

We analyze auto debt and first mortgages separately from other debts because increases in auto debt and first mortgages are typically associated with the acquisition of an asset. Since both assets and liabilities increase upon origination of these kinds of debt, the inference we should draw about net worth from changes in these debt balances is ambiguous. We will discuss in Section IX a framework for thinking about the implications that changes in auto and first mortgage debt have for net worth.

The first column of Table 5 shows that when we control only for calendar time and person fixed effects, there is a significant increase in auto debt balances from tenure months 31-

[33] As shown in Appendix Table B8, when we do not control for demographic × tenure interactions but instead control for demographic × calendar time interactions, the results are quite similar to the results in the fourth column of Table 3.

36 onwards. At 43-48 months of tenure, auto debt is estimated to increase by 2.0% of first-year income. However, once we additionally control for demographic × tenure interactions, there is no significant effect of automatic enrollment at any tenure level, as seen in the second column of Table 5 and Figure 10. 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%].[34]

The estimated effect of automatic enrollment on first mortgage debt, shown in the last two columns of Table 5 and Figure 11, also achieves statistical significance in the very latest tenure bucket of 49-53 months when demographic × tenure interactions are not controlled for; the point estimate is a 9.4% of income increase. However, the estimates lose significance at all tenure levels in the more comprehensive main specification. These effects are not estimated with much precision despite our large sample size because of the high variance of first mortgage balances. In the main specification, 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%].[35]

Although the inclusion or exclusion of demographic × tenure controls does affect whether the treatment effects' 95% confidence intervals include zero, note that at any given tenure level, the point estimate of one specification lies within the 95% confidence interval of the other specification's estimate. We prefer our main specification because we see no strong prior reason to restrict the interactions between tenure and demographics to be zero. Indeed, F-tests indicate that the interaction terms are jointly highly significant ($p < 0.001$).

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]).

We have also explored the extent to which automatic enrollment increases cash-out mortgage refinancing, 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. We do not directly observe cash-out mortgage refinancing, so we create a proxy for this activity. We deem an

[34] When we do not control for demographic × tenure interactions but instead control for demographic × calendar time interactions, the results are very similar to the results in the second column of Table 5. See Appendix Table B8.
[35] As with the auto debt results, the first mortgage debt results are similar whether we control for demographic × tenure interactions or control for demographic × calendar time interactions. See Appendix Table B8.

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 (4). 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 first mortgage debt 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 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

[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.

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.

IX. Implications of auto debt and first mortgages for net worth

In order to understand what changes in auto debt and first mortgages could imply 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:

$$\Delta Secured\ debt = \Delta Durable\ assets + \Delta Financial\ assets. \quad (8)$$

This equation says that, since borrowing that increases the present value of one's liabilities by $1 provides enough financing to buy an asset worth exactly $1, 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—the change in financial assets could be positive. In any of these scenarios, the 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.[37] 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

[37] Calls to Bank of America, Citibank, and JPMorgan Chase confirmed that loans from retirement savings plans can be used for this purpose. We do not have access to individual-level data on TSP loans, but publicly available sources indicate that during our sample period, the percentage of TSP participants who took a loan in a given year was approximately 10% (see the Annual Reports of the Thrift Savings Plan available at https://www.frtib.gov/ReadingRoom/index.html).

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 automatic enrollment affects the left tail of the savings distribution most powerfully (Choi et al., 2004; also see Figure 1), 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. 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

$$W_t(1+r) - rent_t = (W_t - P_t)(1+r) - \kappa P_t + P_{t+1}. \tag{9}$$

Equation (9) holds whether or not $W_t \geq P_t$. We can solve (9) for the rental rate:

$$rent_t = P_t(r+\kappa) - (P_{t+1} - P_t). \tag{10}$$

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 investment 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

$$\begin{aligned} W'_{t+1} - W_{t+1} &= (W_t - P'_t)(1+r) - \kappa P'_t + P'_t(1+g) - [W_t(1+r) - rent_t] \\ &= (P'_t - P_t)(g - r - \kappa), \end{aligned} \tag{11}$$

where we have substituted in the expression in equation (10) for $rent_t$. Equation (11) 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 (11).

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.[38] 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 appreciation that can be expected ex ante (Case and Shiller, 1989).[39] 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 mortgage refinancing. If all of the extracted equity is invested, then the transaction does not change the path of future net

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[38] 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.

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

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 in human capital); 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. Overall, 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 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%.

In summary, employees whose TSP contributions are increased by automatic enrollment are likely to have little non-TSP liquidity to begin with. Therefore, if automatic enrollment increases auto and first mortgage debt, it is probably because households are buying more valuable cars and homes, not because automatic enrollment caused households to spend down more non-TSP assets. If the former is true, then the short-run net worth implications of any increase in auto and first mortgage debt under automatic enrollment are minimal. In the long run, higher auto debt is more likely to presage net worth erosion than higher first mortgage debt.

X. Automatic enrollment effect on contributions net of debt

What is automatic enrollment's effect on contributions net of any changes in debt? Because different types of debt have different implications for net worth, we construct three measures of debt which successively add components of debt that are decreasingly likely to be associated with net worth erosion. D1 encompasses all debt balances excluding auto and first mortgage debt, representing debt that is most likely to signal net worth decreases. D2 is D1 plus auto debt. D3 is D2 plus first mortgage debt, and D3 hence includes all debt balances in our data. NET1, NET2, and NET3 are, respectively, the difference between automatic enrollment's effect on TSP contributions and its effect on D1, D2, or D3.[40, 41]

We see in Table 6 that at 43-48 months of tenure, automatic enrollment does not significantly increase D1 (point estimate = -0.6% of income, 95% confidence interval = [–2.4%, 1.2%]), 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%]). The automatic enrollment effects on NET1 and NET2 are positive and significant from 7-12 months of tenure onwards. The point estimates indicate that automatic enrollment raises NET1 by 1.4%, 2.9%, 3.6%, and 4.7% of first-year salary and NET2 by 1.3%, 2.5%, 2.7%, and 3.6% of first-year salary at 7-12, 19-24, 31-36, and 43-48 months of tenure, respectively. The final column of Table 6 shows that the point estimates for the effect of automatic enrollment on NET3 are positive beyond months 1-6 but closer to zero and statistically insignificant, indicating that the increase in D3 caused by automatic enrollment may be quantitatively important relative to the increase in cumulative total contributions.

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[40] 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.

[41] 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 intervals that are robust to skewed bootstrap distributions. For our NETn statistic $\hat{\theta}$, we generate the $100(1-2\alpha)\%$ confidence interval $[2\hat{\theta}-\hat{\theta}^*_{1-\alpha}, 2\hat{\theta}-\hat{\theta}^*_{\alpha}]$, 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 95% (99%) confidence interval of $\hat{\theta}$, then we say that $\hat{\theta}$ is significant at the 5% (1%) level.

XI. 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 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.[42] The first row in Table 7 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 7 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 baseline credit score below 620.

In the third row, we see that automatic enrollment does not have any effects on Vantage scores; the estimates are insignificant and small in magnitude for all groups. Automatic enrollment also does not have a statistically significant effect on debt excluding auto loans and first mortgages for any subpopulation. However, these estimates are imprecise, and one of the positive point estimates—3.8% for those with low credit scores—is large. For auto debt, the automatic enrollment effect is positive and significant for those with salary less than $34,000 (5.2% of first-year salary) or only a high school education (2.6% of first-year salary). The effect on first mortgages is positive and significant for those with only a high school education (11.7% of first-year salary). However, for every group, the 95% confidence interval of the auto loan or first mortgage effect includes the corresponding point estimate for the entire population (1.1% for auto debt and 2.2% for first mortgages).

The automatic enrollment effect on NET1 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 NET1 found for the entire sample, but their 95% confidence intervals all contain 4.7%. Lack of statistical power also plagues our NET2 and NET3 estimates. Only blacks

[42] 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.

exhibit a statistically significant change in NET2 (a 10.1% of first-year salary increase), and no group exhibits a significant effect on NET3 despite many point estimates that are large in magnitude. However, every group's 95% confidence interval for the effect on NET2 or NET3 includes the corresponding point estimate for the entire sample (3.6% for NET2 and 1.4% for NET3).

XII. 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. The main result of our paper is that automatic enrollment has a precisely estimated zero effect on credit scores, and also has no significant effect on debt excluding first mortgages and auto debt, or on debt in collections. In other words, we provide strong evidence against the hypothesis that automatic enrollment increases financial distress and high-interest debt balances. In our benchmark regressions, we do not find statistically significant evidence that automatic enrollment increases auto debt or first mortgages, although we sometimes find positive and significant effects on these categories of debt in alternative specifications, and we estimate the first mortgage effect with little precision. It would be valuable for further research to examine how automatic enrollment affects non-retirement assets, so that we have a comprehensive picture of effects on the total household balance sheet.

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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. Effect of automatic enrollment on cumulative TSP contributions

Each column reports regression-adjusted effects of automatic enrollment on the dependent variable in the column heading as of the tenure months in the row label, estimated according to equation (1) or (2). The 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 each regression.

	Cumulative total TSP contributions	Cumulative total TSP contributions	Cumulative employee TSP contributions	Cumulative employee TSP contributions
Tenure 1 to 6	0.005** (0.000)	0.004** (0.000)	0.002** (0.000)	0.001** (0.000)
Tenure 7 to 12	0.010** (0.001)	0.009** (0.001)	0.004** (0.001)	0.003** (0.001)
Tenure 13 to 18	0.015** (0.001)	0.014** (0.001)	0.005** (0.001)	0.004** (0.001)
Tenure 19 to 24	0.020** (0.002)	0.020** (0.002)	0.007** (0.001)	0.007** (0.001)
Tenure 25 to 30	0.026** (0.002)	0.026** (0.002)	0.009** (0.002)	0.009** (0.002)
Tenure 31 to 36	0.031** (0.003)	0.031** (0.003)	0.011** (0.002)	0.011** (0.002)
Tenure 37 to 42	0.036** (0.003)	0.036** (0.003)	0.012** (0.003)	0.012** (0.003)
Tenure 43 to 48	0.041** (0.004)	0.041** (0.004)	0.014** (0.003)	0.014** (0.003)
Tenure 49 to 53	0.045** (0.005)	0.045** (0.005)	0.015** (0.004)	0.016** (0.004)
Calendar time fixed effects	Yes	Yes	Yes	Yes
Demographic × tenure controls	No	Yes	No	Yes
# of obs. (*N*)	427,624	427,624	427,624	427,624

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

Table 3. Effect of automatic enrollment on Vantage credit scores and debt excluding auto and first mortgages

Each column reports regression-adjusted effects of automatic enrollment on the dependent variable in the column heading as of the tenure months in the row label, estimated according to either equation (3) or (4). The dependent debt variable is 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.

	Vantage credit score	Vantage credit score	Debt excluding auto, first mortgage	Debt excluding auto, first mortgage
Tenure ≤ -18	-0.5 (0.8)	-0.5 (0.8)	0.002 (0.006)	0.003 (0.006)
Tenure -17 to -12	-0.1 (0.6)	0.0 (0.6)	-0.005 (0.004)	-0.003 (0.004)
Tenure -11 to -6	-0.1 (0.4)	-0.1 (0.4)	-0.005 (0.003)	-0.003 (0.003)
Tenure 1 to 6	0.2 (0.5)	0.3 (0.5)	0.001 (0.003)	-0.001 (0.003)
Tenure 7 to 12	0.3 (0.7)	0.2 (0.7)	-0.002 (0.004)	-0.005 (0.004)
Tenure 13 to 18	0.6 (0.8)	0.5 (0.8)	-0.002 (0.005)	-0.006 (0.005)
Tenure 19 to 24	0.4 (0.9)	0.3 (0.9)	-0.004 (0.006)	-0.009 (0.006)
Tenure 25 to 30	0.1 (1.0)	0.1 (1.0)	0.001 (0.007)	-0.006 (0.007)
Tenure 31 to 36	-0.1 (1.1)	-0.3 (1.1)	0.004 (0.008)	-0.005 (0.008)
Tenure 37 to 42	0.5 (1.1)	0.3 (1.1)	0.007 (0.008)	-0.003 (0.008)
Tenure 43 to 48	0.2 (1.2)	0.1 (1.2)	0.009 (0.009)	-0.006 (0.009)
Tenure 49 to 53	1.4 (1.4)	1.3 (1.4)	0.003 (0.010)	-0.013 (0.010)
Calendar time fixed effects	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes
Demographic × tenure controls	No	Yes	No	Yes
# of obs. (*N*)	670,225	670,225	809,385	809,385

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

Table 4. Effect of automatic enrollment on debt subcomponents

Each column reports coefficients from a regression estimated according to equation (4) 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)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# 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. Effect of automatic enrollment on auto and first mortgage debt

Each column reports regression-adjusted effects of automatic enrollment on the dependent variable in the column heading as of the tenure months in the row label, estimated according to either equation (3) or (4). The 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 each regression.

	Auto debt	Auto debt	First mortgage debt	First mortgage debt
Tenure ≤ -18	0.000 (0.003)	-0.001 (0.003)	0.008 (0.020)	0.024 (0.020)
Tenure -17 to -12	-0.001 (0.003)	-0.001 (0.003)	-0.016 (0.016)	-0.007 (0.016)
Tenure -11 to -6	0.000 (0.002)	0.000 (0.002)	-0.016 (0.011)	-0.010 (0.011)
Tenure 1 to 6	0.001 (0.002)	0.001 (0.002)	0.022 (0.012)	0.021 (0.012)
Tenure 7 to 12	0.002 (0.003)	0.001 (0.003)	0.014 (0.018)	0.004 (0.018)
Tenure 13 to 18	0.006 (0.004)	0.004 (0.004)	0.027 (0.023)	0.011 (0.023)
Tenure 19 to 24	0.006 (0.004)	0.004 (0.004)	0.015 (0.026)	-0.003 (0.026)
Tenure 25 to 30	0.010 (0.005)	0.005 (0.005)	0.029 (0.029)	0.004 (0.029)
Tenure 31 to 36	0.015** (0.005)	0.009 (0.005)	0.050 (0.032)	0.019 (0.032)
Tenure 37 to 42	0.016** (0.006)	0.009 (0.006)	0.054 (0.035)	0.019 (0.035)
Tenure 43 to 48	0.020** (0.006)	0.011 (0.006)	0.074 (0.038)	0.022 (0.037)
Tenure 49 to 53	0.017* (0.007)	0.007 (0.007)	0.094* (0.043)	0.025 (0.043)
Calendar time fixed effects	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes
Demographic × tenure controls	No	Yes	No	Yes
# of obs. (*N*)	809,385	809,385	809,385	809,385

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

Table 6. Effect of automatic enrollment on debt aggregates and cumulative TSP contributions net of debt aggregates

The first three columns report coefficients from regressions estimated according to equation (4), 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 last three columns report the estimated treatment effects on cumulative TSP contributions minus the D1, D2, or D3 effect estimates, where the contribution effect estimates are taken from the second column of Table 2. 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 NET1-NET3 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	NET1	NET2	NET3
Tenure ≤ -18	0.003 (0.006)	0.003 (0.007)	0.027 (0.022)	--	--	--
Tenure -17 to -12	-0.003 (0.004)	-0.004 (0.005)	-0.011 (0.017)	--	--	--
Tenure -11 to -6	-0.003 (0.003)	-0.003 (0.003)	-0.013 (0.012)	--	--	--
Tenure 1 to 6	-0.001 (0.003)	0.000 (0.004)	0.022 (0.013)	0.005 (0.003)	0.004 (0.004)	-0.018 (0.013)
Tenure 7 to 12	-0.005 (0.004)	-0.003 (0.006)	0.001 (0.019)	0.014** (0.004)	0.013* (0.005)	0.008 (0.018)
Tenure 13 to 18	-0.006 (0.005)	-0.001 (0.007)	0.010 (0.024)	0.020** (0.005)	0.015* (0.007)	0.004 (0.024)
Tenure 19 to 24	-0.009 (0.006)	-0.005 (0.008)	-0.008 (0.027)	0.029** (0.006)	0.025** (0.008)	0.028 (0.027)
Tenure 25 to 30	-0.006 (0.007)	-0.001 (0.009)	0.003 (0.031)	0.032** (0.007)	0.027** (0.009)	0.023 (0.031)
Tenure 31 to 36	-0.005 (0.008)	0.004 (0.010)	0.023 (0.034)	0.036** (0.008)	0.027* (0.010)	0.008 (0.035)
Tenure 37 to 42	-0.003 (0.008)	0.006 (0.011)	0.025 (0.037)	0.039** (0.009)	0.030** (0.011)	0.011 (0.038)
Tenure 43 to 48	-0.006 (0.009)	0.005 (0.011)	0.027 (0.040)	0.047** (0.010)	0.036** (0.012)	0.014 (0.042)
Tenure 49 to 53	-0.013 (0.010)	-0.006 (0.013)	0.019 (0.046)	0.059** (0.012)	0.051** (0.014)	0.027 (0.047)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes	Yes	Yes
# of obs. (*N*)	809,385	809,385	809,385	--	--	--

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

Table 7. The effect of automatic enrollment on subpopulations at 43-48 months of tenure

Each cell except those in the rows labeled NET1-NET3 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 (2), and the credit regressions are estimated according to equation (4). The cells in the NET1-NET3 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** (0.009)	0.042** (0.008)	0.056** (0.006)	0.075** (0.007)	0.067** (0.012)	0.058** (0.020)
Cumulative employee TSP contributions	0.029** (0.007)	0.014* (0.006)	0.021** (0.005)	0.034** (0.005)	0.026** (0.009)	0.029 (0.016)
Vantage credit score	3.8 (3.4)	-1.9 (2.9)	0.2 (1.9)	4.8 (3.1)	-1.6 (4.1)	6.5 (7.8)
D1 (debt excl. auto and first mortgages)	0.004 (0.031)	-0.021 (0.017)	0.006 (0.014)	0.038 (0.029)	-0.026 (0.033)	-0.010 (0.047)
Auto debt	0.052* (0.023)	0.026 (0.014)	0.026* (0.011)	0.034 (0.018)	-0.008 (0.022)	-0.015 (0.036)
First mortgage debt	0.137 (0.117)	-0.080 (0.086)	0.117* (0.059)	-0.012 (0.091)	0.010 (0.122)	0.146 (0.195)
D2	0.057 (0.039)	0.005 (0.023)	0.033 (0.018)	0.073* (0.035)	-0.034 (0.041)	-0.025 (0.059)
D3	0.194 (0.129)	-0.075 (0.091)	0.150* (0.064)	0.060 (0.104)	-0.024 (0.136)	0.121 (0.211)
NET1	0.070* (0.032)	0.063** (0.020)	0.049** (0.016)	0.037 (0.030)	0.094** (0.036)	0.067 (0.050)
NET2	0.018 (0.042)	0.037 (0.025)	0.023 (0.020)	0.002 (0.037)	0.101* (0.043)	0.082 (0.061)
NET3	-0.119 (0.126)	0.116 (0.089)	-0.094 (0.064)	0.015 (0.103)	0.092 (0.140)	-0.064 (0.217)
# 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.

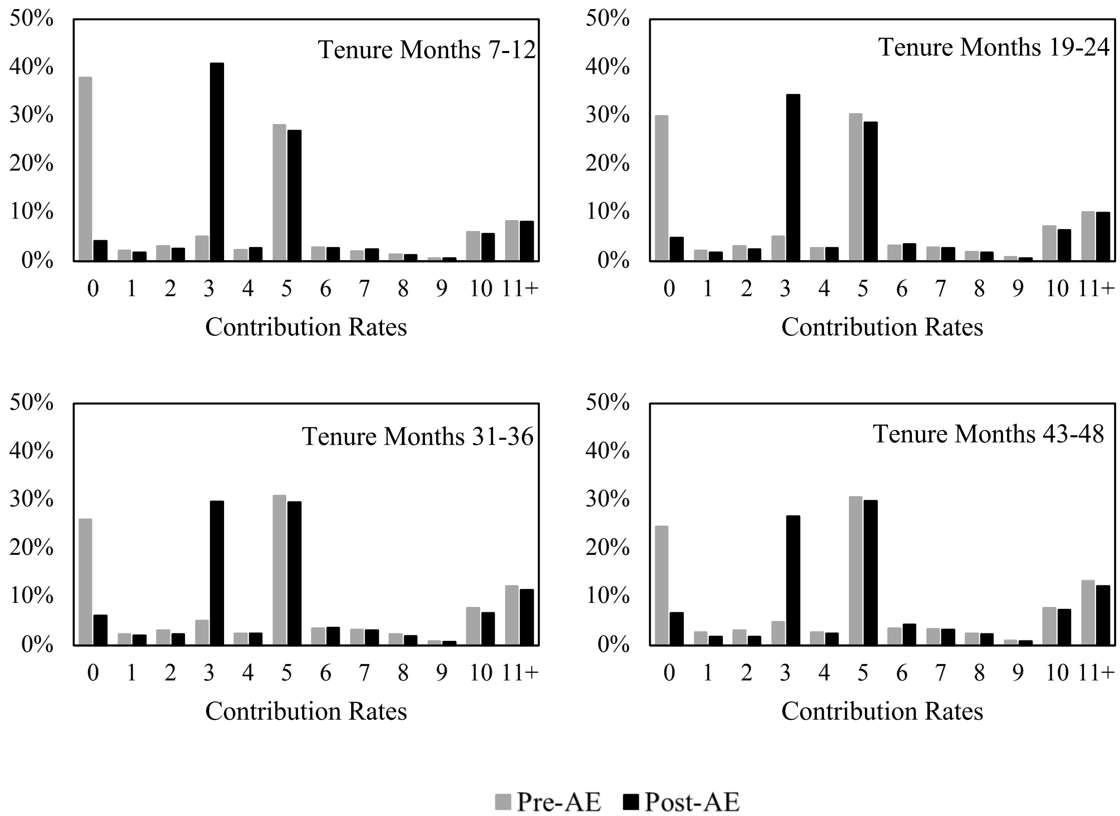


Figure 1. Distributions of employee contribution rates by cohort. The employee contribution rate is the sum of employee before-tax and Roth contribution rates, expressed as percentages of pay, and is measured over the entirety of the June or the December when the employee reached the tenure level indicated. Contribution rates are rounded to whole numbers, and rates at or above 11% are grouped together. The pre-automatic enrollment ("pre-AE") cohort consists of August 2009 – July 2010 hires, and the post-automatic enrollment ("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.

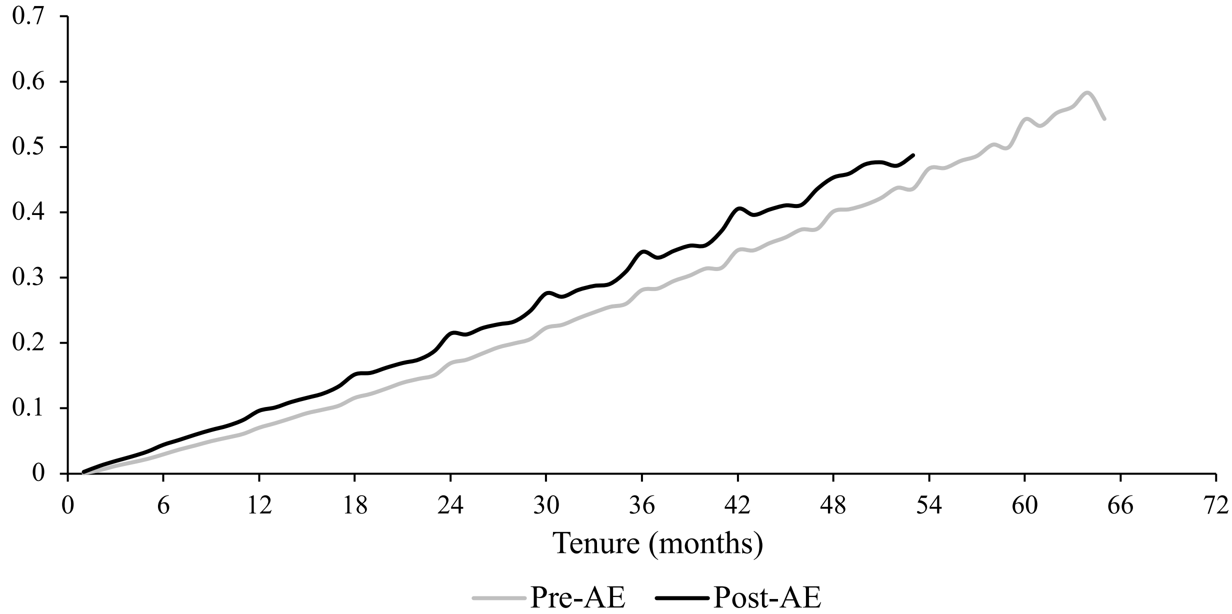


Figure 2. The ratio of cumulative total TSP contributions to annualized first-year pay. Every point in the graphed series corresponds to individuals who reached the tenure level indicated on the horizontal axis in a June or a December. The pre-AE cohort consists of August 2009 – July 2010 hires, and 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.

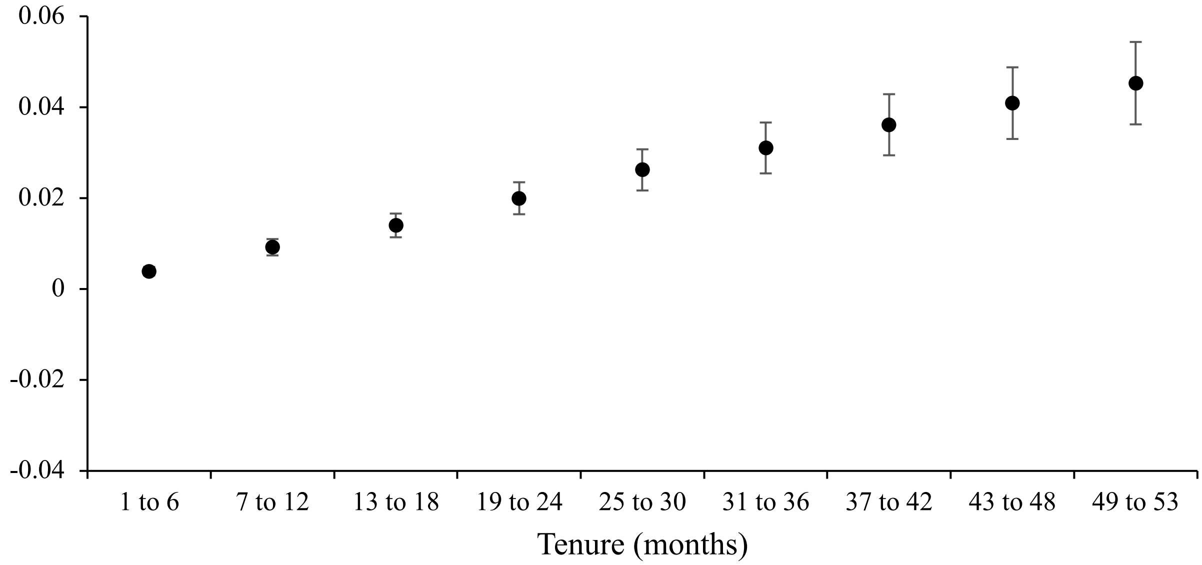


Figure 3. The effect of automatic enrollment on cumulative total TSP contributions to annualized first-year pay ratio. The estimates come from the regression in column 2 of Table 2. Point estimates and 95% confidence intervals are shown.

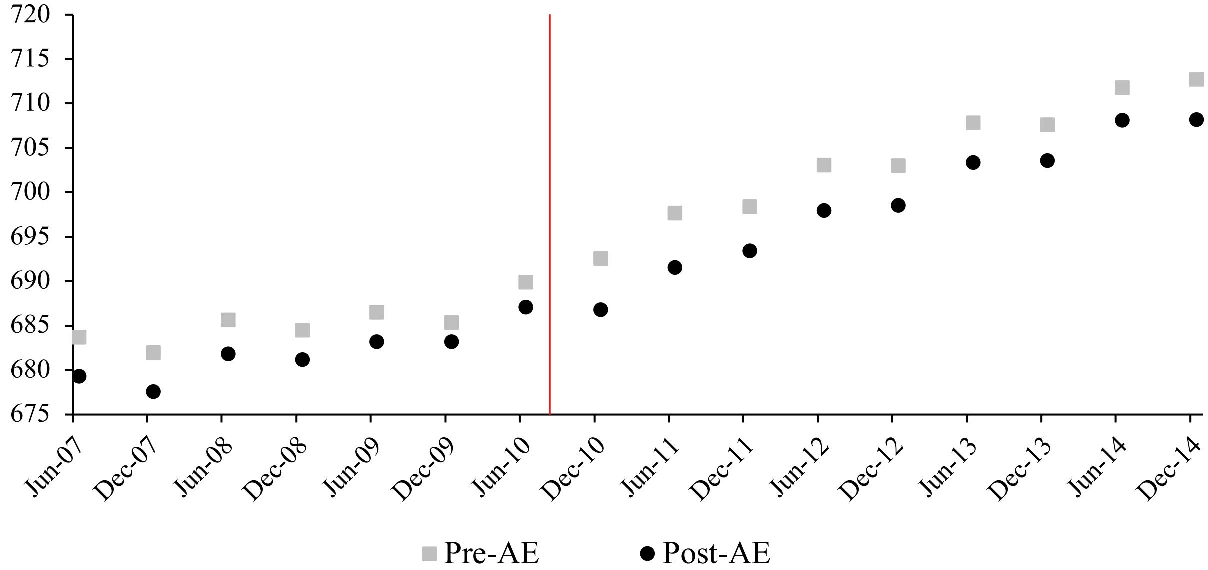


Figure 4. Vantage score at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

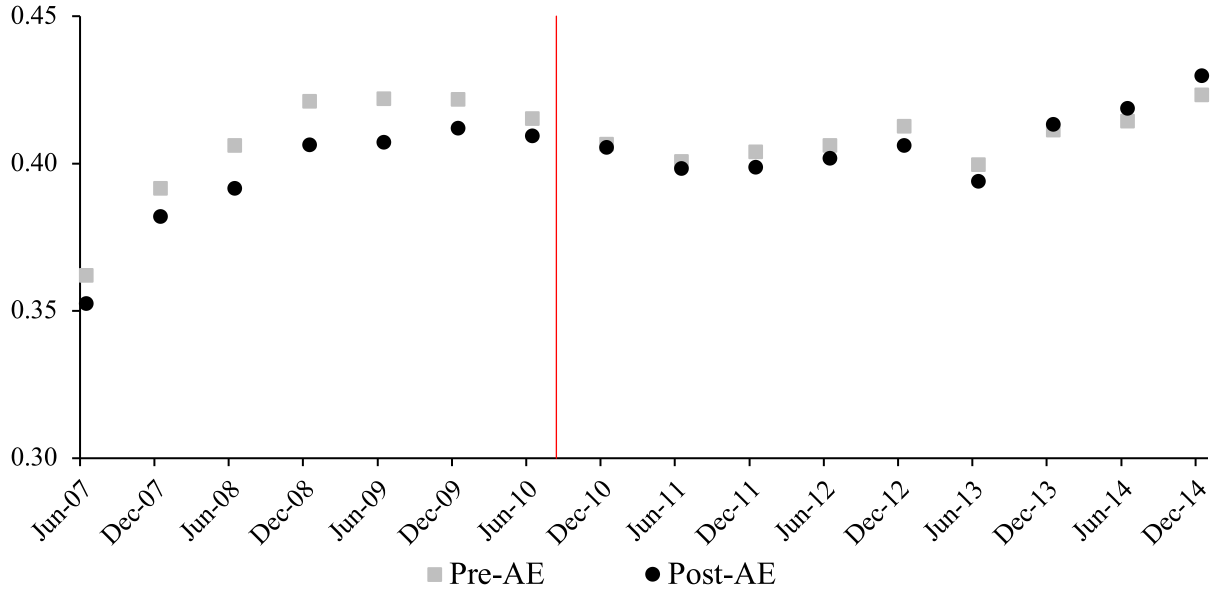


Figure 5. Debt balance excluding auto debt and first mortgages normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment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

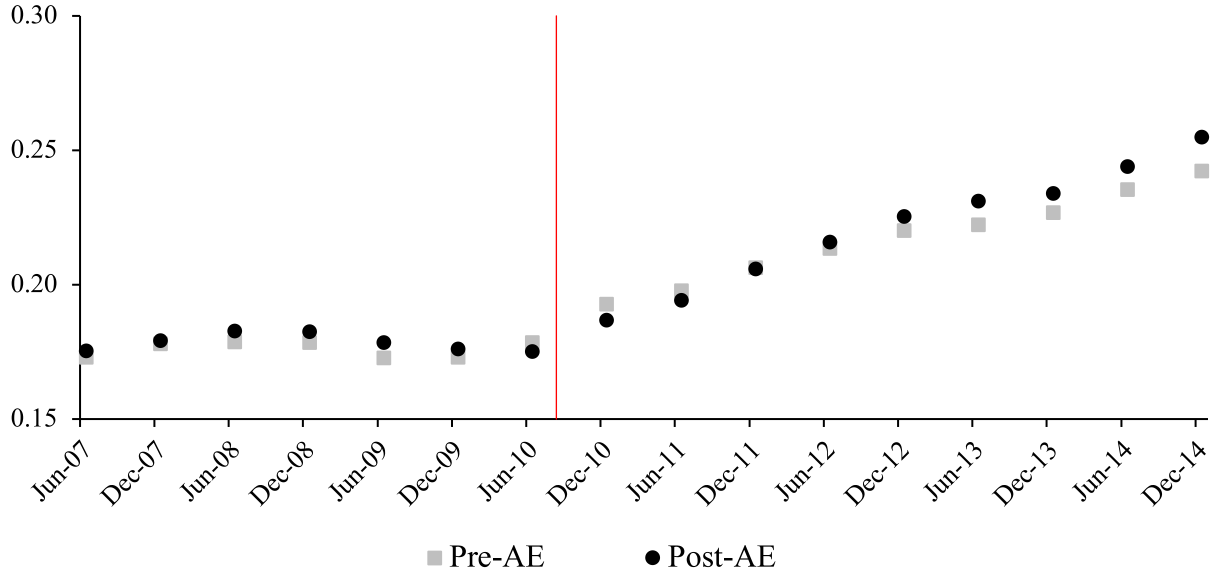


Figure 6. Auto loan and lease balance normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

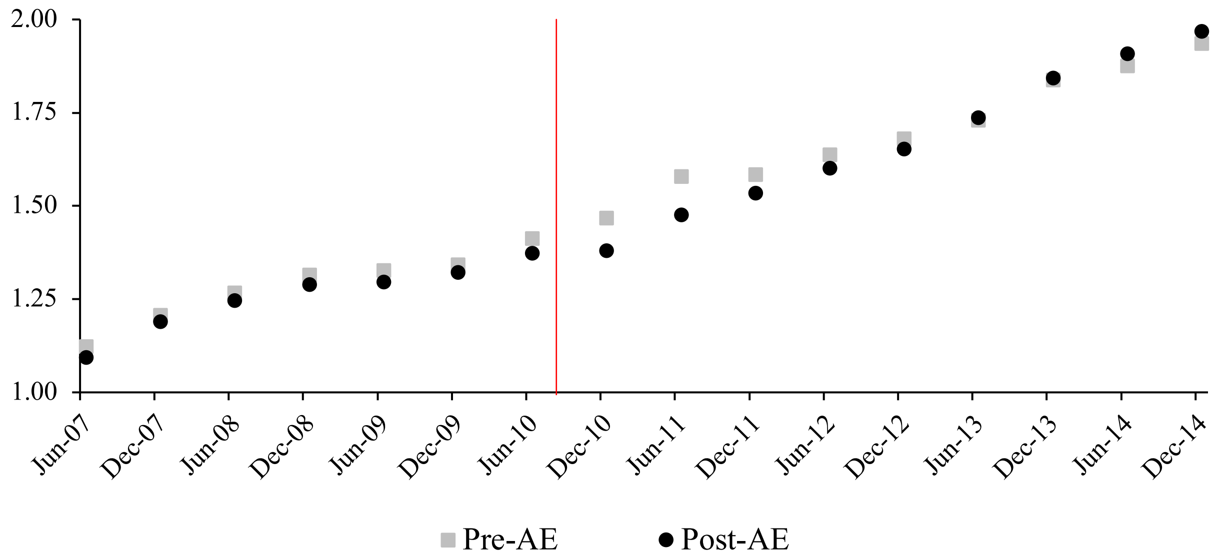


Figure 7. First mortgage balance normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample once they have left Army employment.

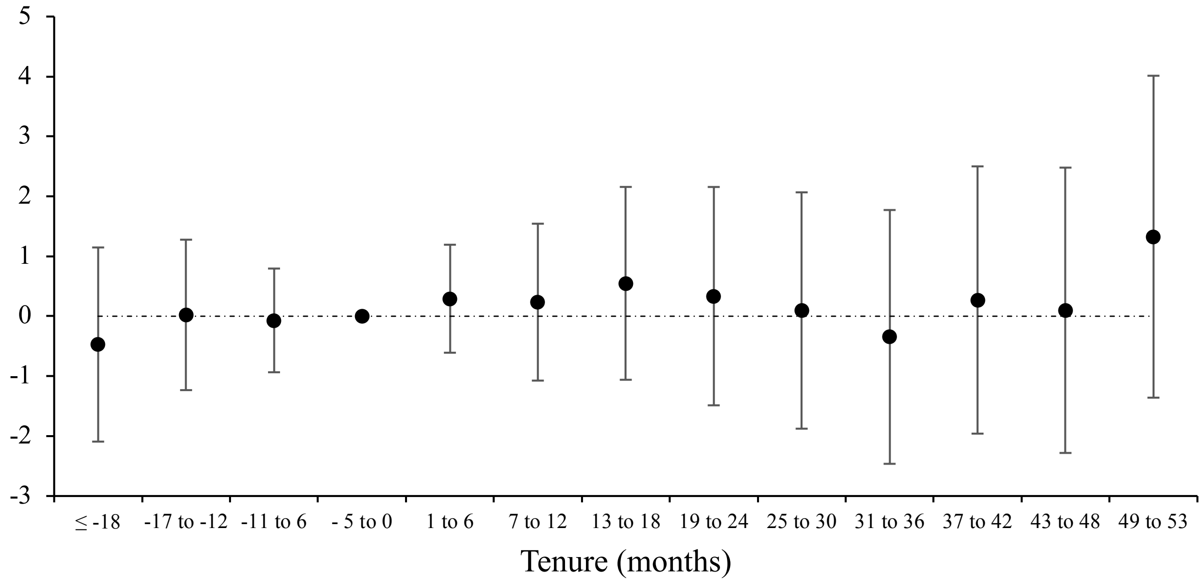


Figure 8. The effect of automatic enrollment on Vantage score. The estimates come from the regression in column 2 of Table 3. Point estimates and 95% confidence intervals are shown.

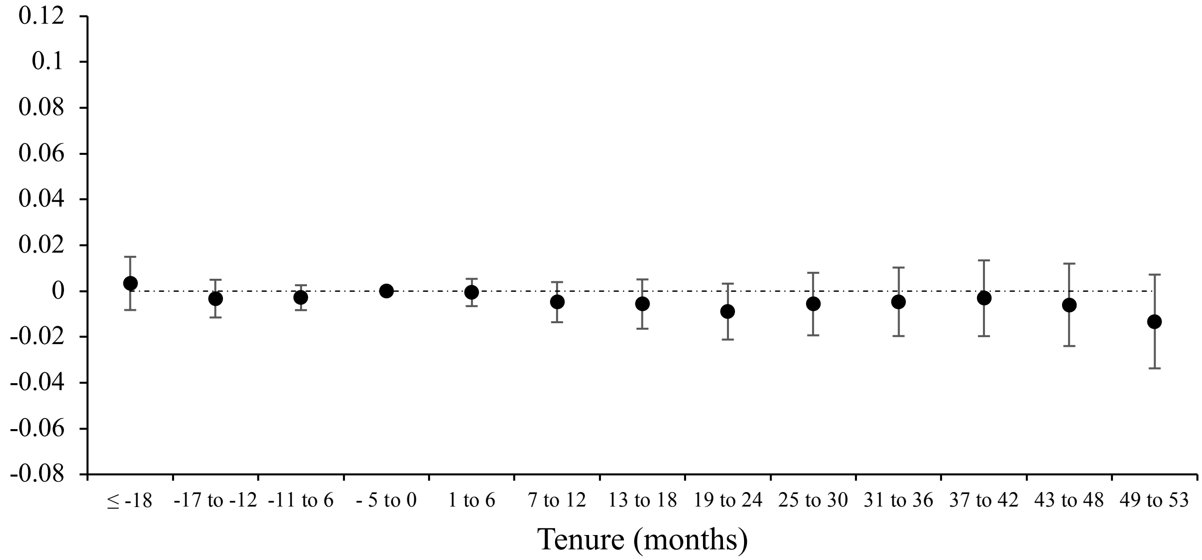


Figure 9. The effect of automatic enrollment on debt balance excluding first mortgages and auto loans normalized by annualized first-year pay (D1). The estimates come from the regression in column 4 of Table 3. Point estimates and 95% confidence intervals are shown.

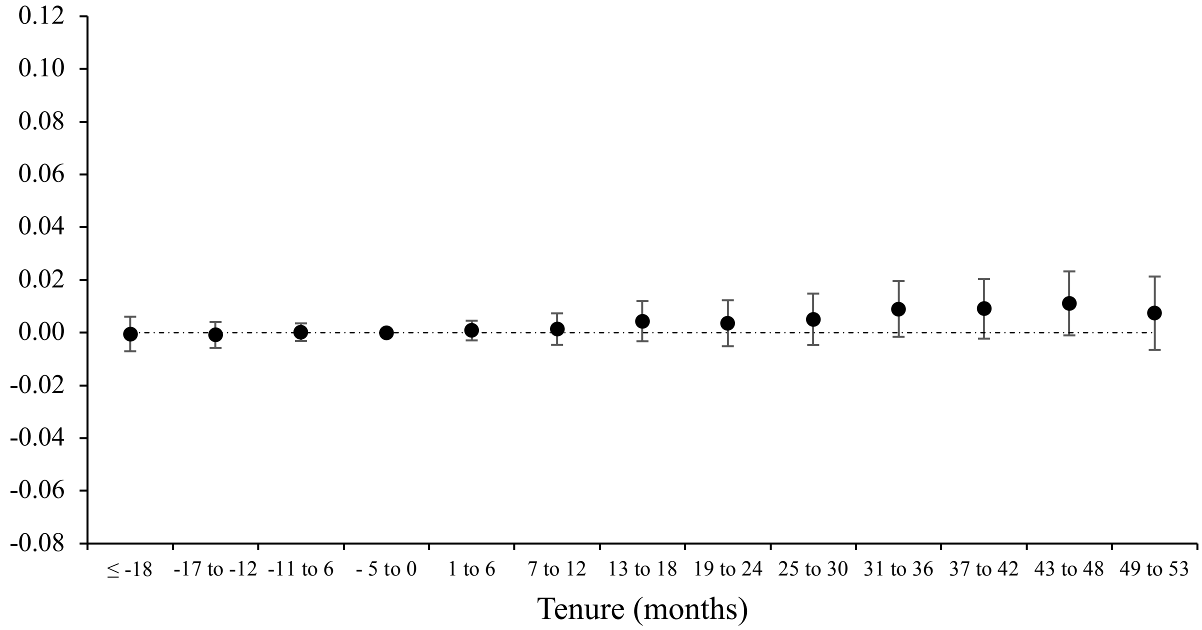


Figure 10. The effect of automatic enrollment on auto loan balance normalized by annualized first-year pay. The estimates come from the regression in column 2 of Table 5. Point estimates and 95% confidence intervals are shown.

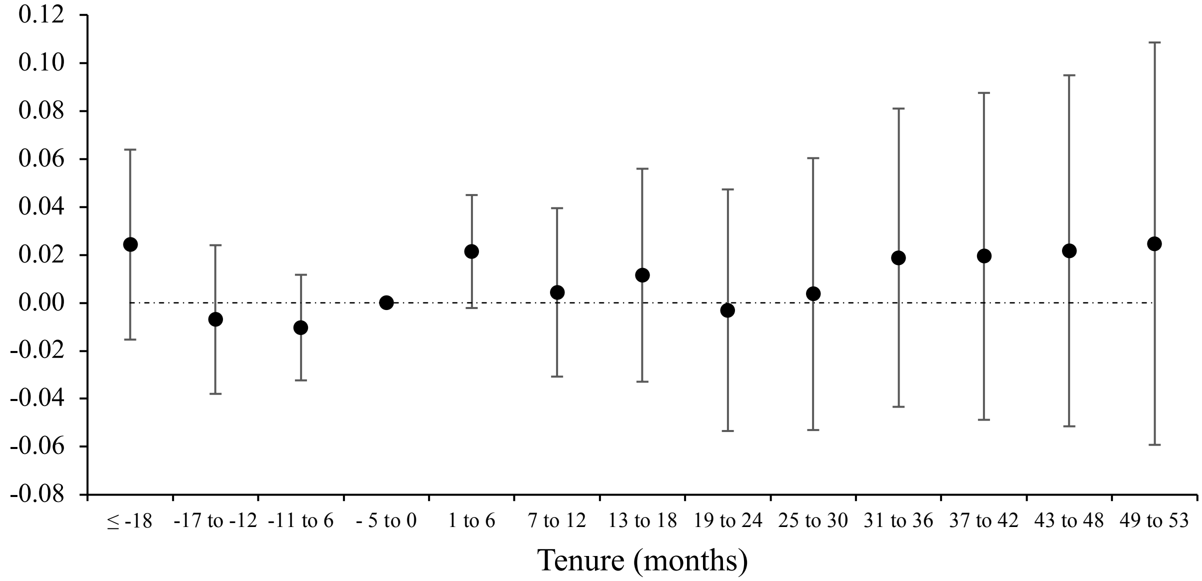


Figure 11. The effect of automatic enrollment on first mortgage balance normalized by annualized first-year pay. The estimates come from the regression in column 4 of Table 5. Point estimates and 95% confidence intervals are shown.

Online Appendix A for
"Borrowing to Save? The Impact of Automatic Enrollment on Debt"

John Beshears, James J. Choi, David Laibson, Brigitte C. Madrian, William L. Skimmyhorn
January 7, 2020

In this appendix, we estimate the automatic enrollment effect on TSP contributions and debt using a regression discontinuity design with hire month as the assignment variable. In our baseline estimate, we find that automatic enrollment causes cumulative TSP contributions to increase by 5.8% of first-year pay at 47 months of tenure.[1] The 95% confidence interval for this estimate is [4.8%, 6.7%], which does not contain the 4.1% estimate we obtain in the main text of the paper. Thus, the estimate in the main text of the paper is on the conservative end of estimates of the effect of automatic enrollment on TSP contributions. At the same horizon, we detect no statistically significant automatic enrollment effect on debt excluding auto loans and first mortgages or on auto loans. We do find a statistically significant increase in first mortgage balances of 12.5% of first-year pay. We also estimate that automatic enrollment causes a negligible change in Vantage score. The confidence intervals of the estimates for the debt and credit score outcome measures include the point estimates in the main text, except in the case of first mortgage balances, where the regression discontinuity design yields a larger positive treatment effect estimate.

We prefer the empirical methodology in the main text for two reasons. First, the main text's methodology yields smaller standard errors than the regression discontinuity design. Second, and more importantly, we are concerned that the regression discontinuity estimates may be misleading because they likely reflect not only the effect of automatic enrollment but also the effect of month-to-month variability in the types of employees hired. The regression results in the main text are less vulnerable to this concern because they involve averages over many months of new hires. Appendix Table A1 suggests that this concern regarding the regression discontinuity design is warranted. The table reports demographic characteristics of the cohort hired during the month before the implementation of automatic enrollment (July 2010) and the

[1] Intuitively, the regression discontinuity design compares individuals hired immediately before versus immediately after August 1, 2010, when automatic enrollment was implemented. We observe credit variables at the end of June and at the end of December in each year, so the regression discontinuity design estimates the effect of automatic enrollment at the end of December 2010 (5 months of tenure), at the end of June 2011 (11 months of tenure), etc. We focus on estimates at 43-48 months of tenure in the main text of the paper, so in this appendix we focus on estimates at 47 months of tenure.

cohort hired during the first month after the implementation of automatic enrollment (August 2010). These two cohorts play an outsized role in the regression discontinuity estimates. Compared to the July 2010 cohort, the August 2010 cohort is lower-income, less educated, and less likely to hold a professional position. The magnitudes of the differences are larger between the two one-month cohorts than between the two one-year cohorts analyzed in the main text, suggesting that month-to-month variation is indeed smoothed out when averaging over more months.[2]

A.I. Methodology

Individuals in our sample were subject to automatic enrollment if and only if they were hired on or after August 1, 2010. We run a separate ordinary least squares regression for each date *t* on which an outcome is measured:

$$y_{it} = \alpha + \beta_1 post_i + \beta_2 hiremonth_i + \beta_3 (post_i \times hiremonth_i) + \gamma X_i + \epsilon_i, \qquad (\text{A}.1)$$

where i indexes individuals, y_{it} is the outcome for person i as of date *t*, $post_i$ indicates whether person *i* was hired in August 2010 or later, $hiremonth_i$ is the signed number of months between person *i*'s hire month and August 2010, and X_i is a vector of individual characteristics measured as of hire (log deflated salary, geographic location, education, college major, job type, gender, race, and age).

Our data contain the employee's month of hire but not the day of hire, so we assume that employees were hired in the middle of each month. For example, August 2010 hires are coded as having $hiremonth_i = 0.5$, and July 2010 hires are coded as having $hiremonth_i = -0.5$. The coefficient of interest is β_1, the extrapolated difference in y between those hired an instant before August 1, 2010, and those hired at the very beginning of August 1, 2010. Because all outcomes in the regression are measured as of the same calendar date, there is no need to control separately for calendar time effects. Also, on a given calendar date, those hired an instant before August 1, 2010, have the same tenure as those hired at the beginning of August 1, 2010, so there is no need

[2] For the purpose of conducting statistical inference, the issue of month-to-month variation could be addressed in the regression discontinuity design by clustering standard errors by month of hire, which is the finest level of granularity on hire date available in the data set. However, asymptotically valid standard error calculations may not be reliable given the small number of observed hire months. Furthermore, these calculations do not remedy the problem that the point estimates may be misleading.

to control separately for tenure effects. Due to the small number of running variable values, we do not cluster standard errors by the assignment variable.

In order for β_1 to be an unbiased estimate of the treatment effect of automatic enrollment, month of hire around August 2010 must be as-if randomized, conditional on observables. Since employees can easily implement the automatic enrollment defaults themselves, there is little incentive to self-sort across the August 2010 hire threshold, so as-if conditional randomization is plausible. However, recall from our discussion of Appendix Table A1 that we are concerned that month-to-month variability in the types of employees hired may have made July 2010 new hires different from August 2010 hires in observable and unobservable ways.

When analyzing credit outcomes, we use as our outcome variable the change relative to the June 2009 level. This within-individual differencing purges time-invariant individual differences in credit levels from the outcome.

We present our results under several bandwidths (4, 8, and 12 months on each side of the hire date threshold) to illustrate that they are not particularly sensitive to bandwidth choice. We also implement a formal bandwidth selection algorithm. The optimal bandwidth algorithms of Imbens and Kalyanaraman (2012) and Calonico, Cattaneo, and Titiunik (2014) do not apply to our setting because they require a continuous assignment variable, whereas the assignment variable in our data set (hire month) is discrete.[3] We instead use a leave-one-out cross-validation procedure as in Ludwig and Miller (2005).[4] For each outcome variable measured at 47 months of tenure, we perform the following procedure for each possible bandwidth up to 12 months. For each observation corresponding to an employee hired in July 2010 or August 2010, we estimate a separate regression using the regression discontinuity specification with the bandwidth under consideration, dropping the focal observation from the sample. We then calculate the squared difference between the regression's predicted outcome value for the focal observation and the focal observation's actual outcome value. Taking the mean of these squared prediction errors across the regressions that use the bandwidth under consideration (one regression for each employee hired in July 2010 or August 2010) gives a measure of the accuracy with which the

[3] Imbens, Guido, and Karthik Kalyanaraman, 2012. "Optimal bandwidth choice for the regression discontinuity estimator." *Review of Economic Studies* 79, pp. 833-959. Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik, 2014. "Robust nonparametric confidence intervals for regression-discontinuity designs." *Econometrica* 82, pp. 2295-2326.

[4] Ludwig, Jens, and Douglas L. Miller, 2005. "Does Head Start improve children's life chances? Evidence from a regression discontinuity design." NBER Working Paper 11702.

bandwidth under consideration predicts outcomes at the regression discontinuity threshold. The leave-one-out cross-validation procedure favors the bandwidth with the lowest mean squared prediction error.

A.II. Results

Appendix Table A2 presents the results from the regression discontinuity analysis. The leave-one-out cross-validation procedure applied to outcomes measured at 47 months of tenure indicates that the lowest mean squared prediction error is achieved with a bandwidth of 9 months for cumulative total TSP contributions; a bandwidth of 11 months for debt excluding auto loans and first mortgages; a bandwidth of 12 months for auto debt; a bandwidth of 11 months for first mortgage debt; and a bandwidth of 12 months for Vantage score. For the sake of consistency across outcome variables, our discussion below focuses on results obtained using a 12-month bandwidth, but the findings are qualitatively similar using other bandwidths.

At 47 months of tenure (corresponding to the 43-48 month tenure bucket that is our preferred long-run horizon in the main text), automatic enrollment increases cumulative total TSP contributions by 5.8% of first-year income (95% confidence interval = [4.8%, 6.7%]). There is no significant effect on debt excluding auto loans and first mortgages at the same horizon; the point estimate is 1.1% of first-year income (95% confidence interval = [-1.1%, 3.2%]). There is a statistically significant increase in auto debt of 1.6% of income at 35 months of tenure, but the statistical significance disappears at later horizons, and the point estimate at 47 months is 1.1% (95% confidence interval = [-0.4%, 2.6%]). First mortgage debt shows a significant increase starting at 17 months of tenure, and by 47 months, automatic enrollment increases first mortgage debt balances by 12.5% of first-year income (95% confidence interval = [3.6%, 21.4%]). There is an economically negligible effect on Vantage score at 47 months of 0.2 points (95% confidence interval = [-2.7, 3.1]).

Appendix Figures A1-A5 present visual analogues of the above analysis using the 12-month bandwidth. The vertical axes represent the residual values $\tilde{y}_{it}$ from regressions of the outcome variable y_{it} on the covariates X_i. The fitted lines are from regressions of the form:

$$\tilde{y}_{it} = a + b_1 post_i + b_2 hiremonth_i + b_3(post_i \times hiremonth_i) + u_i. \qquad (A.2)$$

The data points plotted are the average residualized value of the outcome for people with that hire month.[5]

If our identifying assumptions are valid, we should estimate no effect of automatic enrollment on outcomes *prior* to hire. Appendix Table A3 shows the results of these placebo tests. There are no estimates for outcomes at tenure month -13, since this tenure corresponds to June 2009, the baseline date from which we compute differences. We also do not use bandwidths of 8 and 12 months for tenure month -7 (December 2009), since the wider bandwidths cause both individuals hired and individuals not hired as of December 2009 to be included in the pre-AE cohort sample, and the assumption of local linearity may not hold across a sample of both hired and not-yet-hired individuals.

We find no significant pre-hire effects on auto debt and credit score through tenure month -37. For first mortgage debt, there are significant positive effects at tenure months -25, -31, and -37, but only when using a 12-month bandwidth. For debt excluding auto loans and first mortgages, there are significant positive effects at tenure months -25, -31, and -37 when using a 12-month bandwidth, and there are significant positive effects at tenure months -31 and -37 when using an 8-month bandwidth. On the one hand, these significant differences appear only at a point fairly distant in the past, and with *t*-statistics hovering around 2, their statistical significance is not overwhelming given the large number of tests we have run in Appendix Table A3. On the other hand, the fact that there are any significant placebo results at all casts some doubt on the validity of the regression discontinuity design.

[5] The b coefficients are close but not identical to the β coefficients in Appendix Table A2. Per the Frisch-Waugh-Lovell Theorem, we could produce identical estimates by residualizing the regressors in a similar way, but at the cost of visual clarity.

Appendix Table A1. Comparison of employees hired in month before versus month of automatic enrollment implementation

	Pre-AE (Jul '10 hires)	Post-AE (Aug '10 hires)	Difference	*p*-value of difference
Avg. starting salary	$56,981	$53,849	-3,132	0.000
Avg. age at hire	39.0	38.9	0.0	0.918
Male	63.2%	65.2%	2.0%	0.109
White	52.4%	57.8%	5.5%	0.000
Black	10.9%	10.3%	-0.5%	0.533
Hispanic	2.8%	3.4%	0.6%	0.197
Asian	2.8%	3.9%	1.1%	0.029
Native American	0.7%	0.9%	0.1%	0.636
Missing race	30.4%	23.7%	-6.7%	0.000
High school only	42.1%	47.3%	5.3%	0.000
Some college, no degree	12.3%	12.6%	0.3%	0.744
Associate degree	5.1%	5.1%	0.1%	0.882
Bachelor's degree	21.8%	18.1%	-3.6%	0.001
Graduate degree	17.7%	15.8%	-1.9%	0.057
Unknown education	1.1%	1.0%	-0.1%	0.601
STEM major | college	30.8%	28.2%	-2.6%	0.165
Business major | college	25.4%	27.3%	2.0%	0.279
Other major | college	43.8%	44.4%	0.6%	0.755
Administrative position	29.5%	31.7%	2.2%	0.076
Blue collar position	8.6%	7.4%	-1.2%	0.107
Clerical position	7.6%	6.8%	-0.8%	0.250
Professional position	25.5%	19.4%	-6.2%	0.000
Technical position	16.6%	16.2%	-0.4%	0.651
Other position	12.2%	18.5%	6.4%	0.000
Has credit report in six months before hire	82.8%	83.2%	0.4%	0.677
Avg. Vantage Score in six months before hire, conditional on having Vantage Score	689.3	688.1	-1.2	0.671
# of obs. (*N*)	2,432	3,402		

Appendix Table A2. The effect of automatic enrollment on cumulative TSP contributions and debt changes since June 2009

Each cell shows the treatment effect estimated from a separate regression for which the specification is found in equation (A.1). All dependent variables except Vantage credit score are normalized by first-year income. Bandwidth refers to the number of hire months on either side of August 2010 that are included in the regression. The regressions include all people who remain employed as of that calendar date. Standard errors robust to heteroskedasticity are in parentheses below point estimates.

		Tenure (months)								
	Bandwidth	5	11	17	23	29	35	41	47	53
Cumulative total TSP contributions	4 months	0.012**	0.021**	0.028**	0.030**	0.038**	0.041**	0.045**	0.048**	0.047**
		(0.001)	(0.001)	(0.002)	(0.003)	(0.004)	(0.006)	(0.007)	(0.008)	(0.010)
	8 months	0.013**	0.023**	0.032**	0.035**	0.045**	0.049**	0.055**	0.061**	0.061**
		(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
	12 months	0.013**	0.021**	0.031**	0.033**	0.043**	0.046**	0.052**	0.058**	0.059**
		(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)
Debt excluding auto, first mortgage (D1)	4 months	0.005	-0.010	-0.010	0.001	0.010	-0.001	0.013	0.023	0.025
		(0.011)	(0.012)	(0.013)	(0.014)	(0.015)	(0.016)	(0.018)	(0.019)	(0.020)
	8 months	0.005	-0.004	-0.001	0.002	0.002	0.006	0.009	0.015	0.009
		(0.008)	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)	(0.013)	(0.013)	(0.014)
	12 months	0.007	-0.003	-0.004	0.001	0.000	0.005	0.007	0.011	0.010
		(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.011)	(0.011)	(0.012)
Auto debt	4 months	0.012	0.008	0.006	0.005	0.017	0.024*	0.009	0.011	0.004
		(0.007)	(0.008)	(0.010)	(0.010)	(0.011)	(0.012)	(0.012)	(0.013)	(0.014)
	8 months	0.009	0.011	0.006	0.008	0.015	0.018*	0.010	0.009	0.004
		(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
	12 months	0.003	0.010*	0.007	0.008	0.012	0.016*	0.009	0.011	0.009
		(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
First mortgage debt	4 months	0.063	0.056	0.165**	0.166**	0.036	0.102	0.088	0.162*	0.089
		(0.045)	(0.049)	(0.057)	(0.060)	(0.067)	(0.070)	(0.076)	(0.078)	(0.083)
	8 months	0.065*	0.077*	0.140**	0.131**	0.089	0.117*	0.094	0.169**	0.118*
		(0.032)	(0.035)	(0.040)	(0.043)	(0.047)	(0.050)	(0.054)	(0.056)	(0.059)
	12 months	0.024	0.042	0.091**	0.076*	0.069	0.096*	0.050	0.125**	0.080
		(0.026)	(0.028)	(0.032)	(0.034)	(0.038)	(0.041)	(0.044)	(0.045)	(0.048)
Vantage credit score	4 months	1.05	0.35	0.16	0.28	2.06	0.19	0.13	-1.13	0.62
		(1.64)	(1.80)	(2.01)	(2.11)	(2.27)	(2.39)	(2.49)	(2.59)	(2.75)
	8 months	-0.65	0.46	1.11	0.00	0.70	0.49	0.95	1.48	1.89
		(1.16)	(1.27)	(1.42)	(1.50)	(1.61)	(1.69)	(1.78)	(1.83)	(1.94)
	12 months	-1.27	0.33	0.26	-0.42	-0.16	0.02	-0.53	0.21	-0.12
		(0.95)	(1.04)	(1.16)	(1.22)	(1.31)	(1.37)	(1.45)	(1.50)	(1.58)

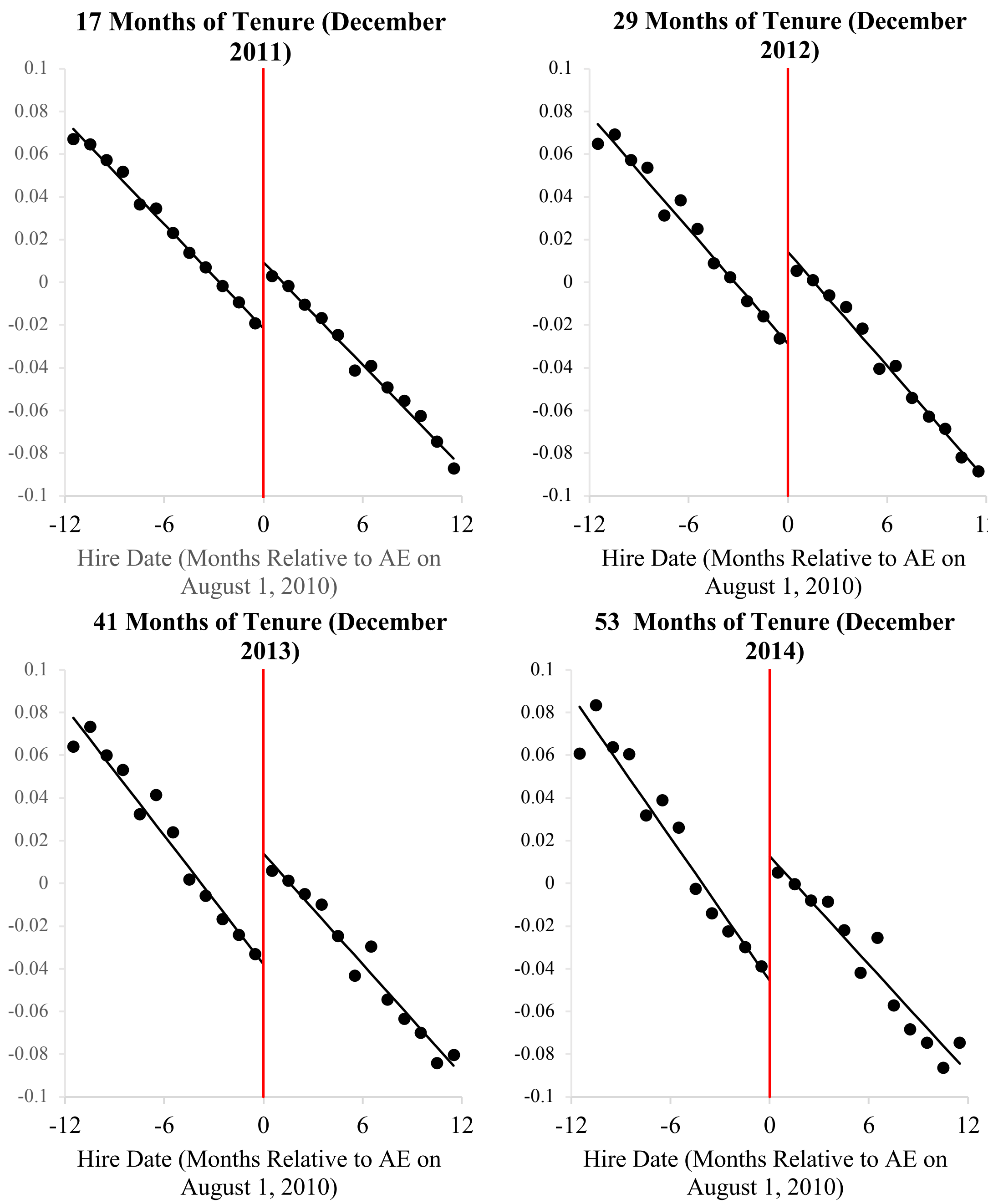
* Significant at 5% level. ** Significant at 1% level.

Appendix Table A3. Placebo tests: The effect of automatic enrollment on debt changes relative to June 2009, prior to hire

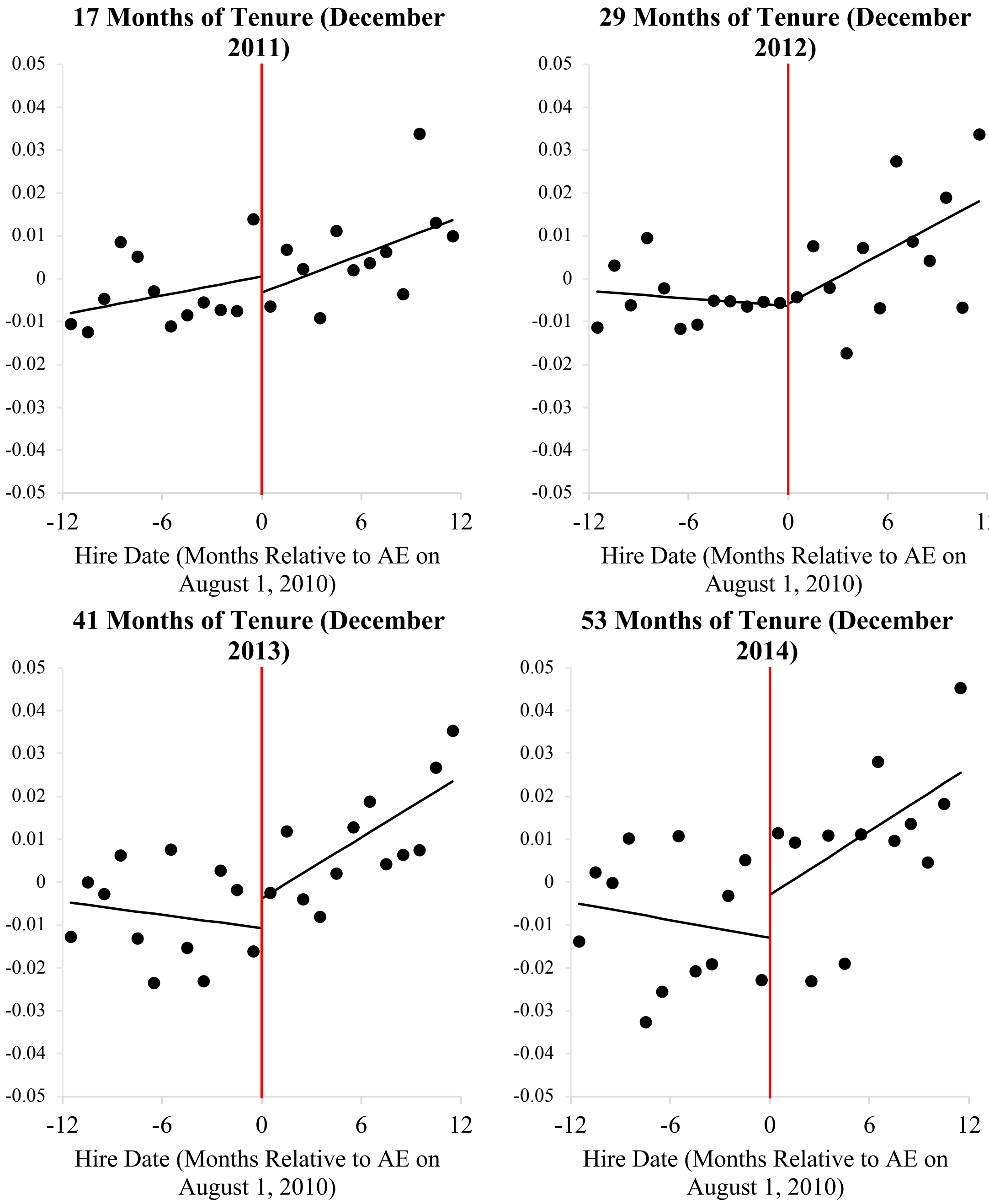
Each cell shows the treatment effect estimated from a separate regression for which the specification is found in equation (A.1). All dependent variables except Vantage credit score are normalized by first-year income. All variables are changes relative to June 2009. Bandwidth refers to the number of hire months on either side of August 2010 that are included in the regression. The regressions include all people who ever appear in our data with a positive tenure. Standard errors robust to heteroskedasticity are in parentheses below point estimates.

		Tenure (months)					
	Bandwidth	-37	-31	-25	-19	-13	-7
Debt excluding auto,	4 months	0.012	0.021	0.013	0.009	--	-0.005
first mortgage (D1)		(0.012)	(0.011)	(0.009)	(0.007)		(0.007)
	8 months	0.020*	0.020*	0.012	0.007	--	--
		(0.009)	(0.008)	(0.006)	(0.005)		
	12 months	0.017*	0.017**	0.011*	0.004	--	--
		(0.007)	(0.007)	(0.005)	(0.004)		
Auto debt	4 months	-0.007	-0.008	-0.004	-0.003	--	0.003
		(0.007)	(0.006)	(0.005)	(0.004)		(0.004)
	8 months	0.000	-0.003	-0.003	-0.003	--	--
		(0.005)	(0.004)	(0.004)	(0.003)		
	12 months	0.003	-0.001	-0.001	-0.003	--	--
		(0.004)	(0.004)	(0.003)	(0.002)		
First mortgage debt	4 months	-0.040	-0.028	-0.056	-0.045	--	0.013
		(0.044)	(0.040)	(0.034)	(0.028)		(0.031)
	8 months	0.044	0.054	0.028	0.002	--	--
		(0.031)	(0.028)	(0.024)	(0.019)		
	12 months	0.054*	0.050*	0.046*	0.014	--	--
		(0.026)	(0.023)	(0.019)	(0.016)		
Vantage credit score	4 months	-1.32	-1.86	-1.46	-0.36	--	0.69
		(1.83)	(1.69)	(1.46)	(1.16)		(1.16)
	8 months	-1.23	-0.92	-0.57	0.04	--	--
		(1.30)	(1.20)	(1.03)	(0.82)		
	12 months	-1.20	-0.78	-0.26	0.46	--	--
		(1.07)	(0.98)	(0.85)	(0.67)		

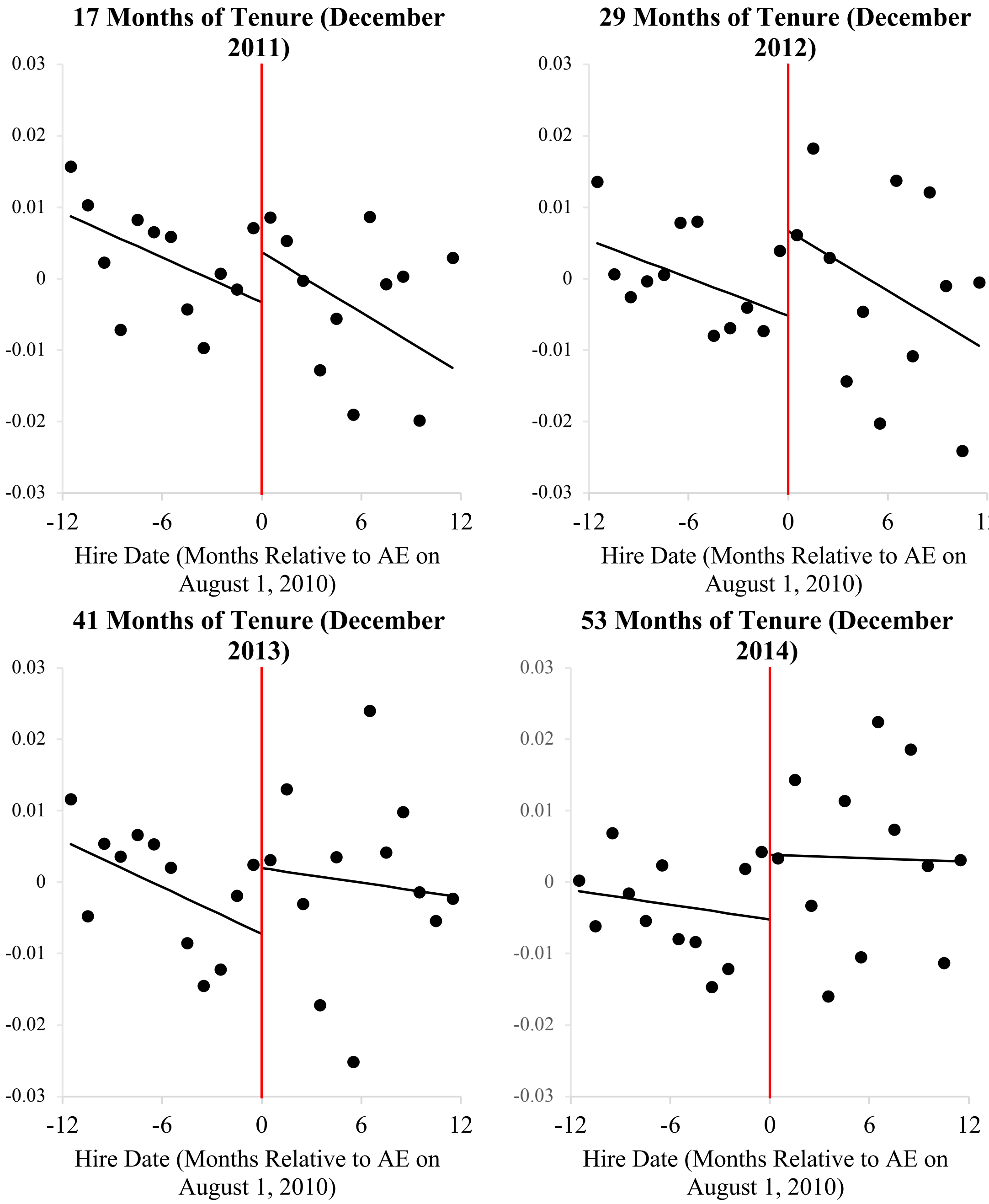
* Significant at 5% level. ** Significant at 1% level.



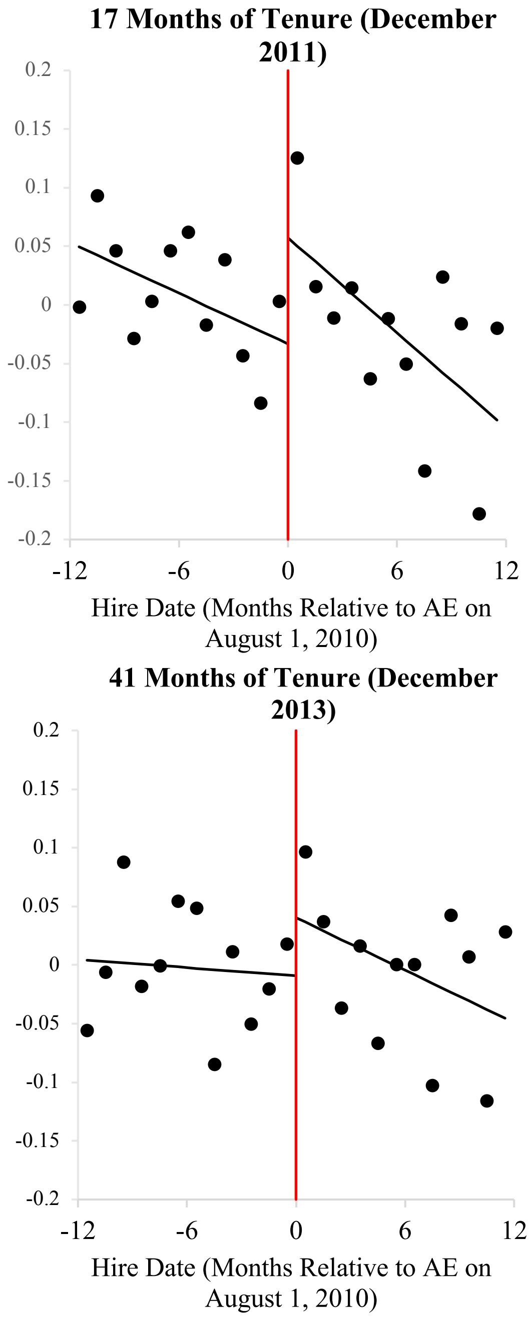
Appendix Figure A1. The effect of automatic enrollment on cumulative total TSP contributions to annualized first-year pay ratio, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



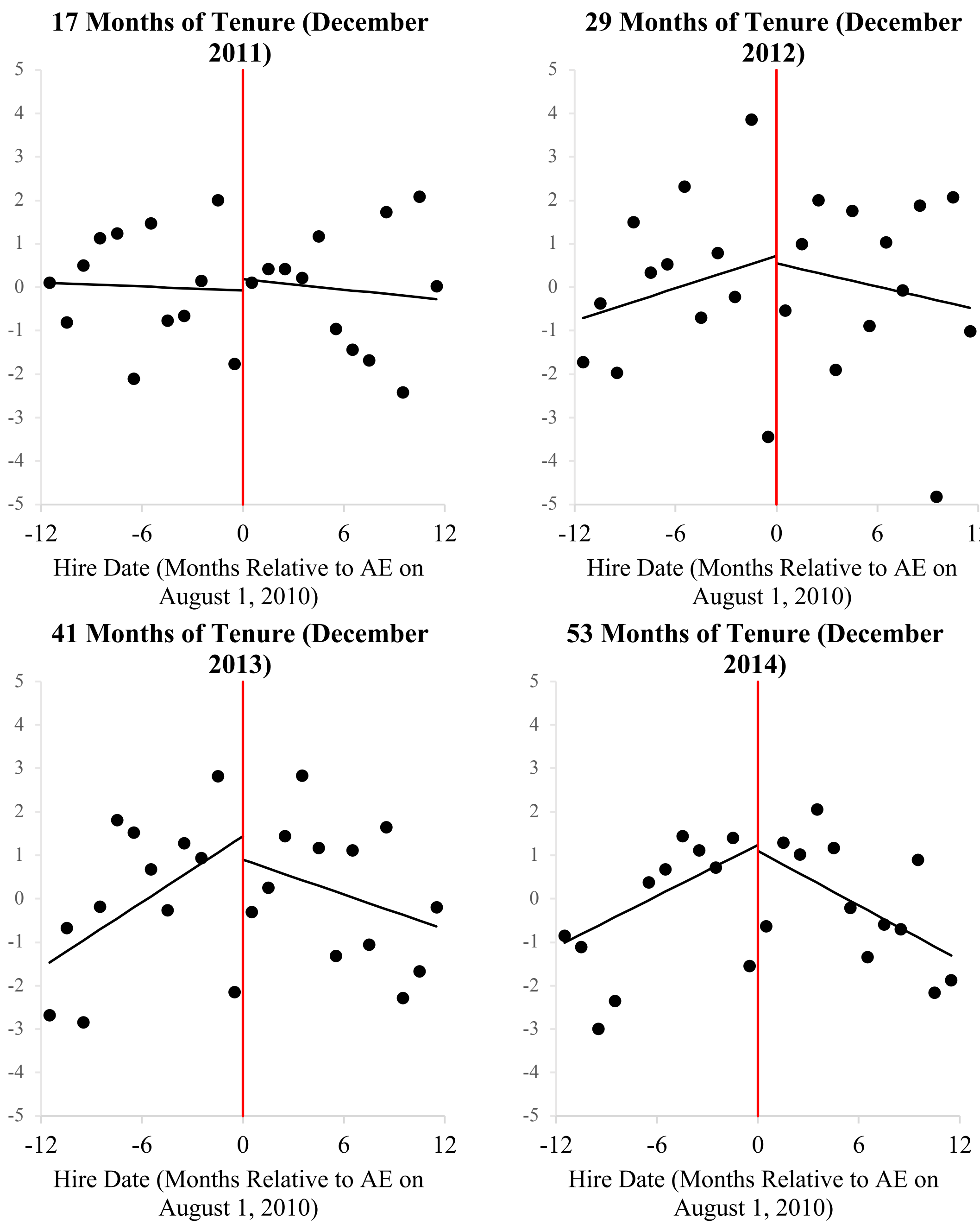
Appendix Figure A2. The effect of automatic enrollment on change since June 2009 in debt excluding auto and first mortgage debt (D1) to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



Appendix Figure A3. The effect of automatic enrollment on change since June 2009 in auto debt to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



Appendix Figure A4. The effect of automatic enrollment on change since June 2009 in first mortgage debt to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



Appendix Figure A5. The effect of automatic enrollment on change since June 2009 in Vantage score, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).

Online Appendix B for
"Borrowing to Save? The Impact of Automatic Enrollment on Debt"

John Beshears, James J. Choi, David Laibson, Brigitte C. Madrian, William L. Skimmyhorn
January 7, 2020

In this appendix, we report analyses that supplement the analyses presented in the main text of the paper.

In Tables 2-5, the regression sample includes individuals as long as they remain employed by the Army, so the sample composition changes as tenure increases and individuals terminate employment. Appendix Tables B1-B4 conduct the same analysis holding the sample fixed as tenure increases. Appendix Tables B1 and B2 examine the sample of employees who remain employed at least until they reach 43-48 months of tenure. Appendix Tables B3 and B4 examine the sample of employees who were ever hired, setting their contribution flows to zero after separation from employment.

Appendix Tables B5, B6, and B7 conduct the same analysis as in Tables 4, 6, and 7, respectively, except with an alternative regression specification that does not control for the interaction of tenure and demographics.

Appendix Table B8 conducts the same analysis as in Tables 2, 3, and 5, except with a regression specification that does not control for the interaction of tenure and demographics and instead controls for the interaction of calendar time and demographics.

Appendix Figure B1 shows participation rates in the TSP for the pre-AE and post-AE cohorts. Participation is defined as making a positive employee contribution to the TSP.

Appendix Table B1. Effect of automatic enrollment on cumulative TSP contributions and debt components: Constant sample of employees who remain at least 43-48 months

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 (2), and the credit regressions are estimated according to equation (4). 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. The sample contains only people who remain employed at 43-48 months of tenure.

	Cumulative total TSP contributions	Cumulative employee TSP contributions	Vantage credit score	Debt excluding auto, first mortgage	Auto debt	First mortgage debt
Tenure ≤ -18	--	--	-1.5 (1.0)	0.009 (0.007)	-0.005 (0.004)	0.010 (0.026)
Tenure -17 to -12	--	--	-0.9 (0.8)	-0.003 (0.005)	-0.005 (0.003)	-0.025 (0.020)
Tenure -11 to -6	--	--	-0.5 (0.6)	-0.003 (0.004)	-0.001 (0.002)	-0.011 (0.014)
Tenure 1 to 6	0.003** (0.001)	0.000 (0.000)	0.6 (0.6)	0.001 (0.004)	0.001 (0.002)	0.031* (0.015)
Tenure 7 to 12	0.009** (0.001)	0.003** (0.001)	0.1 (0.8)	-0.002 (0.006)	0.003 (0.004)	0.021 (0.023)
Tenure 13 to 18	0.015** (0.002)	0.005** (0.001)	0.7 (1.0)	-0.001 (0.007)	0.005 (0.005)	0.037 (0.028)
Tenure 19 to 24	0.022** (0.002)	0.008** (0.002)	0.3 (1.1)	-0.002 (0.007)	0.003 (0.005)	0.000 (0.031)
Tenure 25 to 30	0.029** (0.003)	0.011** (0.002)	-0.1 (1.2)	0.002 (0.008)	0.003 (0.006)	0.008 (0.034)
Tenure 31 to 36	0.034** (0.004)	0.013** (0.003)	-0.8 (1.2)	0.004 (0.009)	0.004 (0.006)	0.028 (0.036)
Tenure 37 to 42	0.040** (0.004)	0.016** (0.003)	-0.2 (1.3)	0.008 (0.009)	0.006 (0.006)	0.027 (0.039)
Tenure 43 to 48	0.046** (0.005)	0.018** (0.004)	-0.5 (1.3)	0.004 (0.010)	0.007 (0.007)	0.038 (0.041)
Tenure 49 to 53	0.051** (0.006)	0.021** (0.005)	0.7 (1.5)	-0.004 (0.011)	0.004 (0.008)	0.043 (0.047)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	No	No	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes	Yes	Yes
# of obs. (*N*)	344,208	344,208	478,067	574,313	574,313	574,313

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

Appendix Table B2. Effect of automatic enrollment on debt subcomponents: Constant sample of employees who remain at least 43-48 months

Each column reports coefficients from a regression estimated according to equation (4) 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. The sample contains only people who remain employed at 43-48 months of tenure.

	HELOC revolving	Non-HELOC revolving	Other installment loans	Second mortgages	Student loans	External collections	Residual debt
Tenure ≤ -18	0.002	0.005	0.000	0.006	-0.003	-0.001	0.000
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Tenure -17 to -12	-0.001	0.001	-0.002	0.001	-0.002	-0.001	0.000
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.000)
Tenure -11 to -6	-0.001	0.001	-0.003	0.000	-0.001	0.000	0.000
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Tenure 1 to 6	0.000	0.000	-0.001	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.000)	(0.000)
Tenure 7 to 12	0.000	0.002	-0.005	-0.001	0.001	0.000	0.000
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.001)	(0.000)
Tenure 13 to 18	0.000	0.003	-0.005	0.001	0.000	0.000	0.000
	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Tenure 19 to 24	-0.002	0.004	-0.004	0.000	-0.001	0.000	0.000
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.001)	(0.001)
Tenure 25 to 30	-0.002	0.004	-0.003	0.001	0.000	0.000	0.001
	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.001)	(0.001)
Tenure 31 to 36	-0.003	0.004	-0.001	0.002	0.001	0.000	0.001
	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.001)	(0.001)
Tenure 37 to 42	-0.002	0.005	-0.004	0.003	0.004	-0.001	0.001
	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.001)	(0.001)
Tenure 43 to 48	-0.003	0.005	-0.004	0.003	0.003	-0.001	0.001
	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.001)	(0.001)
Tenure 49 to 53	-0.003	0.003	-0.008	0.006	-0.002	0.000	0.001
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.001)	(0.001)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of obs. (*N*)	574,313	574,313	574,313	574,313	574,313	574,313	574,313

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

Appendix Table B3. Effect of automatic enrollment on cumulative TSP contributions and debt components: Constant sample of employees who were ever hired

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 (2), and the credit regressions are estimated according to equation (4). 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. The constant sample contains all employees who were ever hired, setting their contribution flows to zero after separation.

	Cumulative total TSP contributions	Cumulative employee TSP contributions	Vantage credit score	Debt excluding auto, first mortgage	Auto debt	First mortgage debt
Tenure ≤ -18	--	--	-0.5 (0.8)	0.003 (0.006)	-0.001 (0.003)	0.021 (0.020)
Tenure -17 to -12	--	--	0.0 (0.6)	-0.003 (0.004)	-0.001 (0.003)	-0.008 (0.016)
Tenure -11 to -6	--	--	-0.1 (0.4)	-0.003 (0.003)	0.000 (0.002)	-0.011 (0.011)
Tenure 1 to 6	0.005** (0.000)	0.002** (0.000)	0.3 (0.5)	-0.001 (0.003)	0.001 (0.002)	0.021 (0.012)
Tenure 7 to 12	0.010** (0.001)	0.004** (0.001)	0.1 (0.6)	-0.006 (0.004)	0.000 (0.003)	0.006 (0.017)
Tenure 13 to 18	0.015** (0.001)	0.005** (0.001)	0.4 (0.8)	-0.008 (0.005)	0.001 (0.004)	0.013 (0.021)
Tenure 19 to 24	0.021** (0.002)	0.008** (0.001)	0.3 (0.9)	-0.014* (0.006)	-0.001 (0.004)	-0.005 (0.024)
Tenure 25 to 30	0.027** (0.002)	0.010** (0.002)	0.4 (0.9)	-0.014* (0.006)	0.001 (0.005)	-0.001 (0.026)
Tenure 31 to 36	0.031** (0.003)	0.012** (0.002)	-0.1 (1.0)	-0.017* (0.007)	0.002 (0.005)	-0.005 (0.029)
Tenure 37 to 42	0.036** (0.003)	0.014** (0.002)	0.4 (1.0)	-0.018* (0.008)	0.004 (0.005)	-0.006 (0.031)
Tenure 43 to 48	0.041** (0.004)	0.016** (0.003)	-0.1 (1.1)	-0.023** (0.008)	0.006 (0.005)	-0.002 (0.033)
Tenure 49 to 53	0.046** (0.004)	0.018** (0.003)	0.3 (1.2)	-0.029** (0.009)	0.002 (0.006)	0.010 (0.037)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	No	No	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes	Yes	Yes
# of obs. (*N*)	560,223	560,223	779,283	941,984	941,984	941,984

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

Appendix Table B4. Effect of automatic enrollment on debt subcomponents: Constant sample of employees who were ever hired

Each column reports coefficients from a regression estimated according to equation (4) 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. The constant sample contains all employees who were ever hired, setting their contribution flows to zero after separation.

	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)	0.003 (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)	0.001 (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)	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)
Tenure 7 to 12	0.000 (0.002)	-0.001 (0.001)	-0.005* (0.003)	0.001 (0.002)	-0.002 (0.002)	0.000 (0.001)	0.001 (0.000)
Tenure 13 to 18	-0.001 (0.002)	0.000 (0.002)	-0.006 (0.003)	0.001 (0.002)	-0.003 (0.002)	-0.001 (0.001)	0.001 (0.001)
Tenure 19 to 24	-0.001 (0.002)	0.000 (0.002)	-0.006 (0.003)	-0.001 (0.003)	-0.006* (0.003)	-0.001 (0.001)	0.001 (0.001)
Tenure 25 to 30	-0.002 (0.002)	0.000 (0.002)	-0.005 (0.003)	-0.001 (0.003)	-0.007* (0.003)	-0.001 (0.001)	0.002* (0.001)
Tenure 31 to 36	-0.003 (0.003)	-0.001 (0.003)	-0.004 (0.004)	-0.001 (0.003)	-0.009* (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 37 to 42	-0.003 (0.003)	-0.001 (0.003)	-0.005 (0.004)	0.000 (0.003)	-0.009* (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 43 to 48	-0.005 (0.003)	-0.002 (0.003)	-0.006 (0.004)	-0.001 (0.003)	-0.011* (0.005)	-0.001 (0.001)	0.001 (0.001)
Tenure 49 to 53	-0.003 (0.004)	-0.003 (0.003)	-0.008* (0.004)	0.000 (0.003)	-0.015** (0.005)	-0.001 (0.001)	0.001 (0.001)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of obs. (*N*)	941,984	941,984	941,984	941,984	941,984	941,984	941,984

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

Appendix Table B5. Effect of automatic enrollment on D1 subcomponents: Alternative specification

Each column reports coefficients from a regression whose dependent variable is in the column heading. The regressions are estimated according to equation (3), which omits controls 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 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.

	HELOC revolving	Non-HELOC revolving	Other installment loans	Second mortgages	Student loans	External collections	Residual debt
Tenure ≤ -18	0.005 (0.002)	0.002 (0.002)	-0.003 (0.003)	0.004 (0.003)	-0.005 (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)	-0.003 (0.002)	-0.001 (0.001)	0.000 (0.000)
Tenure -11 to -6	-0.001 (0.001)	0.001 (0.001)	-0.003* (0.001)	0.000 (0.001)	-0.001 (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)	0.002 (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)	0.001 (0.002)	-0.001 (0.001)	0.001 (0.001)
Tenure 19 to 24	0.000 (0.002)	0.002 (0.002)	-0.005 (0.004)	0.000 (0.003)	-0.001 (0.003)	0.000 (0.001)	0.001 (0.001)
Tenure 25 to 30	-0.001 (0.003)	0.003 (0.003)	-0.005 (0.004)	0.002 (0.003)	0.001 (0.003)	0.000 (0.001)	0.001* (0.001)
Tenure 31 to 36	-0.002 (0.003)	0.004 (0.003)	-0.002 (0.004)	0.002 (0.003)	0.001 (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 37 to 42	-0.001 (0.003)	0.004 (0.003)	-0.004 (0.004)	0.004 (0.004)	0.004 (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 43 to 48	-0.002 (0.004)	0.005 (0.003)	-0.002 (0.004)	0.003 (0.004)	0.004 (0.005)	-0.002 (0.001)	0.002** (0.001)
Tenure 49 to 53	-0.001 (0.004)	0.004 (0.004)	-0.006 (0.005)	0.005 (0.004)	0.000 (0.006)	0.000 (0.001)	0.002 (0.001)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic × tenure controls	No	No	No	No	No	No	No
# 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.

Appendix Table B6. Effect of automatic enrollment on debt aggregates and cumulative TSP contributions net of debt aggregates: Alternative specification

The first three columns report coefficients from regressions estimated according to equation (3), 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 last three columns report the estimated treatment effects on cumulative TSP contributions minus the D1, D2, or D3 effect estimates, where the contribution effect estimates are taken from the first column of Table 2. 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 NET1-NET3 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	NET1	NET2	NET3
Tenure ≤ -18	0.002 (0.006)	0.002 (0.007)	0.010 (0.022)	--	--	--
Tenure -17 to -12	-0.005 (0.004)	-0.006 (0.005)	-0.022 (0.017)	--	--	--
Tenure -11 to -6	-0.005 (0.003)	-0.004 (0.003)	-0.021 (0.012)	--	--	--
Tenure 1 to 6	0.001 (0.003)	0.002 (0.004)	0.024 (0.013)	0.004 (0.003)	0.003 (0.004)	-0.019 (0.013)
Tenure 7 to 12	-0.002 (0.004)	0.000 (0.005)	0.015 (0.019)	0.012** (0.004)	0.010 (0.005)	-0.005 (0.019)
Tenure 13 to 18	-0.002 (0.005)	0.004 (0.007)	0.031 (0.024)	0.017** (0.006)	0.011 (0.007)	-0.016 (0.024)
Tenure 19 to 24	-0.004 (0.006)	0.003 (0.008)	0.017 (0.027)	0.024** (0.006)	0.018* (0.008)	0.003 (0.027)
Tenure 25 to 30	0.001 (0.007)	0.010 (0.009)	0.040 (0.031)	0.026** (0.007)	0.016 (0.009)	-0.013 (0.031)
Tenure 31 to 36	0.004 (0.008)	0.018 (0.010)	0.069* (0.034)	0.027** (0.008)	0.012 (0.010)	-0.038 (0.035)
Tenure 37 to 42	0.007 (0.008)	0.023* (0.011)	0.076* (0.038)	0.029** (0.009)	0.013 (0.011)	-0.040 (0.038)
Tenure 43 to 48	0.009 (0.009)	0.028* (0.011)	0.102* (0.041)	0.032** (0.010)	0.012 (0.012)	-0.061 (0.041)
Tenure 49 to 53	0.003 (0.010)	0.020 (0.013)	0.114* (0.047)	0.042** (0.011)	0.025 (0.014)	-0.069 (0.047)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic × tenure controls	No	No	No	No	No	No
# of obs. (N)	809,385	809,385	809,385	--	--	--

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

Appendix Table B7. Effect of automatic enrollment on subpopulations at 43-48 months of tenure: Alternative specification

Each cell except those in the rows labeled NET1-NET3 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 (3), both of which omit controls for interactions between demographics and tenure. The cells in the NET1-NET3 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	0.076** (0.009)	0.043** (0.008)	0.055** (0.006)	0.075** (0.007)	0.066** (0.012)	0.057** (0.020)
Cumulative employee TSP contributions	0.030** (0.007)	0.015* (0.006)	0.021** (0.005)	0.034** (0.005)	0.025** (0.009)	0.029 (0.016)
Vantage credit score	2.2 (3.4)	-4.0 (2.9)	0.5 (1.9)	4.6 (3.1)	-0.5 (4.1)	1.5 (7.4)
Auto loans and leases	0.049* (0.022)	0.021 (0.014)	0.037** (0.011)	0.037* (0.018)	-0.002 (0.022)	0.007 (0.035)
First mortgages	0.181 (0.117)	-0.058 (0.086)	0.170** (0.059)	0.010 (0.091)	0.031 (0.122)	0.100 (0.197)
D1 (debt excl. auto and first mortgages)	0.000 (0.031)	-0.015 (0.017)	0.021 (0.014)	0.048 (0.029)	-0.007 (0.033)	0.045 (0.043)
D2	0.049 (0.040)	0.006 (0.024)	0.058** (0.018)	0.085* (0.036)	-0.009 (0.041)	0.052 (0.056)
D3	0.229 (0.129)	-0.052 (0.092)	0.228** (0.065)	0.095 (0.105)	0.022 (0.135)	0.152 (0.214)
NET1	0.076* (0.032)	0.058** (0.020)	0.034* (0.016)	0.027 (0.030)	0.074* (0.036)	0.012 (0.046)
NET2	0.027 (0.041)	0.037 (0.026)	-0.003 (0.019)	-0.010 (0.037)	0.075 (0.044)	0.004 (0.059)
NET3	-0.154 (0.128)	0.095 (0.092)	-0.173* (0.066)	-0.020 (0.108)	0.044 (0.136)	-0.095 (0.213)
# of employees at 43-48 months	5,882	7,358	15,576	6,572	4,009	1,448

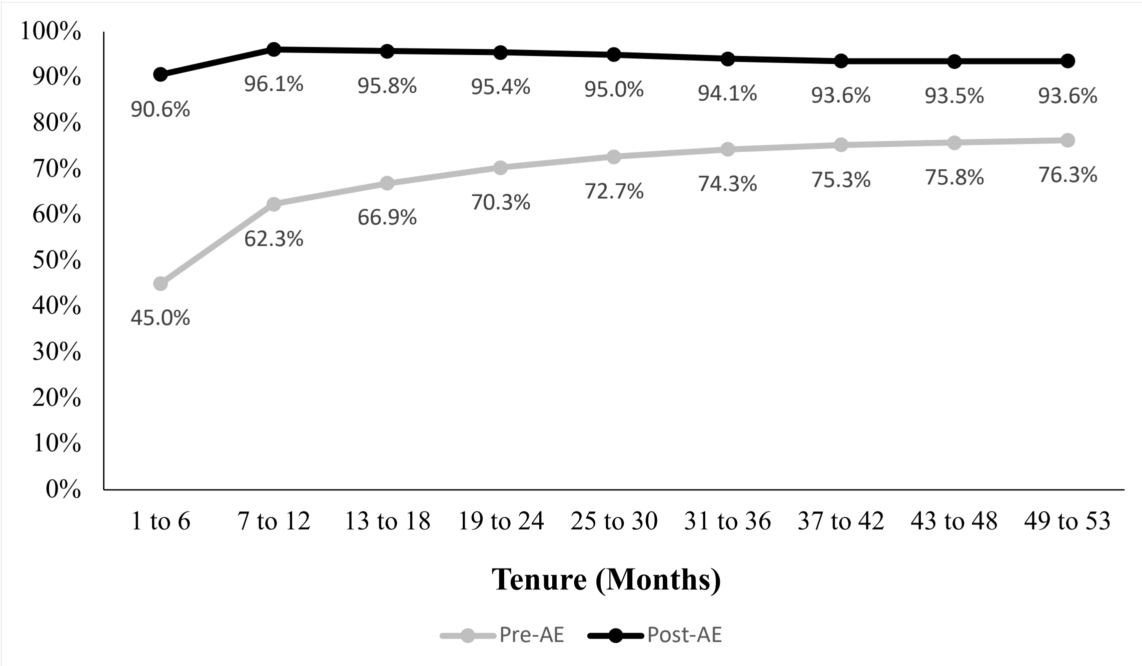
* Significant at 5% level. ** Significant at 1% level.

Appendix Table B8. Effect of automatic enrollment on cumulative TSP contributions and debt components: Specification controlling for the interaction of demographics and calendar time

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 (2), and the credit regressions are estimated according to equation (4), except the terms capturing the interaction of demographics and tenure are removed and replaced with terms capturing the interaction of demographics and calendar time. The reported numbers 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	Vantage credit score	Debt excluding auto, first mortgage	Auto debt	First mortgage debt
Tenure ≤ -18	--	--	-0.5 (0.8)	0.005 (0.006)	-0.001 (0.003)	0.026 (0.020)
Tenure -17 to -12	--	--	-0.2 (0.6)	-0.003 (0.004)	-0.001 (0.003)	-0.002 (0.016)
Tenure -11 to -6	--	--	-0.1 (0.4)	-0.004 (0.003)	0.001 (0.002)	-0.010 (0.011)
Tenure 1 to 6	0.004** (0.000)	0.001** (0.000)	0.2 (0.5)	0.000 (0.003)	0.000 (0.002)	0.017 (0.012)
Tenure 7 to 12	0.010** (0.001)	0.003** (0.001)	0.1 (0.7)	-0.004 (0.004)	0.001 (0.003)	0.003 (0.018)
Tenure 13 to 18	0.014** (0.001)	0.005** (0.001)	0.5 (0.8)	-0.005 (0.005)	0.003 (0.004)	0.011 (0.023)
Tenure 19 to 24	0.020** (0.002)	0.007** (0.001)	0.2 (0.9)	-0.009 (0.006)	0.002 (0.004)	-0.003 (0.026)
Tenure 25 to 30	0.026** (0.002)	0.009** (0.002)	-0.2 (1.0)	-0.006 (0.007)	0.004 (0.005)	0.003 (0.029)
Tenure 31 to 36	0.031** (0.003)	0.011** (0.002)	-0.5 (1.1)	-0.004 (0.008)	0.008 (0.005)	0.019 (0.032)
Tenure 37 to 42	0.036** (0.003)	0.012** (0.003)	0.1 (1.1)	-0.003 (0.008)	0.007 (0.006)	0.015 (0.035)
Tenure 43 to 48	0.040** (0.004)	0.014** (0.003)	-0.2 (1.2)	-0.005 (0.009)	0.009 (0.006)	0.025 (0.037)
Tenure 49 to 53	0.044** (0.005)	0.015** (0.004)	1.0 (1.4)	-0.012 (0.010)	0.006 (0.007)	0.027 (0.043)
Calendar time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Person fixed effects	No	No	Yes	Yes	Yes	Yes
Demographic × calendar time controls	Yes	Yes	Yes	Yes	Yes	Yes
# of obs. (*N*)	427,624	427,624	670,225	809,385	809,385	809,385

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



Appendix Figure B1. Participation rates in the TSP by cohort. Participation is defined as making a positive employee contribution to the TSP in the June or December when an individual reached the tenure level indicated on the horizontal axis. 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. The participation rates do not exactly equal 100 minus the fraction contributing 0% in Figure 1 because the 0% bar in Figure 1 includes individuals making positive contributions that are less than 0.5% of their salary.