Borrowing to Save?
The Impact of Automatic Enrollment on Debt

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Abstract: Does automatic enrollment into a retirement plan increase financial distress due to increased 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 increases cumulative contributions to the plan by 4.1% of annual salary, but we find little evidence of increased financial distress. Automatic enrollment causes no significant change in credit scores (point estimate = +0.001 standard deviations), debt balances excluding auto debt and first mortgages (point estimate = -0.6% of annual salary), or adverse credit outcomes such as late balances or balances in collection, with the possible exception of increased first mortgage balances in foreclosure.

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

Automatic enrollment is intended to increase economic security in retirement. Its effectiveness at doing so depends not only on whether it increases retirement plan contributions, but also on whether it has unintended consequences for 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 financial distress. This latter possibility is the focus of this paper.

We augment existing analyses of automatic enrollment by studying household *liabilities* —especially adverse credit outcomes—and thereby examining 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 increases in debt and financial distress.

We exploit 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 credit outcomes for the 32,072 employees hired in the year prior to the adoption of automatic enrollment to savings and credit 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.)

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.[3] 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 implementation of automatic enrollment would not have participated in the TSP within four years of hire in the absence of automatic enrollment, but do participate under automatic enrollment. If they remain at the 3% default contribution rate for four years, automatic enrollment increases their cumulative employee contributions by 12% of annual pay—a significant cumulative reduction in take-home pay that has the potential to trigger increased borrowing and financial distress.[4]

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

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

[4] 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 variables estimate as a local average treatment effect among compliers is not satisfied.

Our main results measure the effect of automatic enrollment on credit scores, debt balances excluding auto debt and first mortgages, and several measures of adverse credit outcomes. 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 an extremely tight interval around zero. 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%]. We also study the effect of automatic enrollment on the likelihood of one's credit score dropping at least 25 points or at least 50 points relative to its level immediately before hire, as well as the effect of automatic enrollment on the presence and extent of late balances, derogatory balances, and balances in collection.[5] Automatic enrollment does not have a statistically significant effect on any of these outcomes. Furthermore, we examine the effect of automatic enrollment on the above outcomes for subpopulations that might be financially vulnerable (e.g., employees with low salaries), where automatic enrollment has been shown to have a particularly large positive effect on contributions. We do not find statistically significant estimates more frequently than would be expected by chance given the number of hypotheses tested. In sum, this evidence does not support the hypothesis that automatic enrollment increases financial distress.

We study the effect of automatic enrollment on auto loans and first mortgages separately from other outcome variables because these types of debt are usually originated in order to finance the acquisition of an asset. Increases in auto loan and first mortgage balances have ambiguous implications for net worth because assets typically increase along with liabilities. (In Online Appendix B, we discuss a framework for thinking about this issue.) We find no significant increase in either kind of debt balance in our main regression specification, although the point estimates are positive, and the effect on first mortgages is estimated imprecisely due to the high variance of first mortgage balances. 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 is 2.2% of income (95% confidence interval =

[5] Late balances are balances for which the borrower has missed a required payment by one or more billing cycles. Balances are derogatory when the lender has taken action beyond merely requiring the minimum payment, primarily because the debt is more than 120 days past due in the case of installment loans or more than 180 days past due in the case of revolving debt. Balances in collection have been passed to an external collection agency and do not include balances for which the originating lender is using an internal collection group to seek repayment.

[-5.1%, 9.5%]). The auto and first mortgage debt balance effects are positive and statistically significant in some alternative (non-benchmark) regression specifications and for some financially vulnerable subpopulations. Concerns regarding increased auto and first mortgage debt balances are mitigated by our finding that automatic enrollment does not have statistically significant effects on auto loan delinquency, first mortgage delinquency, the likelihood of first mortgage foreclosure, or balances on foreclosed first mortgages at 43-48 months of tenure, with the caveat that we see statistically significant increases in foreclosed first mortgage balances at some other tenure levels. The results for foreclosed first mortgage balances are noteworthy but must be interpreted with caution because we have conducted a large number of hypothesis tests.

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 large 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

[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.

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 automatic enrollment's effect on credit outcomes. We show the main results on credit scores, debt excluding auto loans and first mortgages, and adverse credit outcomes in Section VII. In Section VIII, we show additional results on auto loans and first mortgages. Section IX discusses limitations and extensions of our analysis, and Section X concludes. Online Appendix A shows results from an alternative estimation strategy using a regression discontinuity design. Online Appendix B develops a framework for thinking about the implications an increase in auto or first mortgage debt could have for household net worth. Online Appendix C 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

[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.

IRS imposes limits on the total amount that can be contributed to the TSP within a calendar year. In 2010, the maximum employee contribution was $16,500 for those younger than 50 and $22,000 for those 50 and older. These limits have gradually risen over time. Participants can invest in five index funds—a U.S. Treasury security fund, a U.S. fixed income fund, a U.S. large cap equity fund, a U.S. small cap equity fund, and an international equity fund—and five lifecycle funds, which are mixes of the five index funds based on investor time horizons.

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

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

[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)

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 is one of the 25 largest employers in 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

[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.

middle of the first pay period of October and ended in the middle of the second pay period of October, no regularly scheduled payday passed without paychecks being issued to all employees. However, the first paycheck in October was abnormally low, and the second paycheck was abnormally high. Gelman et al. (forthcoming) find that employees affected by the October furloughs reduced spending and delayed debt payments during the period of temporarily low income. We only observe contributions at a monthly frequency and credit reports at a biannual frequency, so we make no adjustment for the government shutdown in October 2013.

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

[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

month of their tenure because of concerns that their payroll data are not reliable. Beyond an employee's second month of tenure, if payroll data are missing for a month, we assume that pay and TSP contributions in the missing month were the same as in the closest preceding non-missing month.[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 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., delinquent accounts, derogatory balances, mortgage foreclosures, etc.). In most but not all cases, the debt measures are broken up by category (e.g., mortgage, bankcard, student loans, auto loans, etc.). 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

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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.

[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. We further discuss the implications of observing individual-level instead of household-level debt outcomes in Section IX.

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

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]

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. 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.

In our primary regression specifications, which we describe in detail in Sections V and VI, we control flexibly for observable differences in characteristics. As a robustness check, we have also conducted our analysis using a coarsened exact matching approach (Blackwell et al., 2009; Iacus, King, and Porro, 2012), which we describe in detail in Online Appendix C. Online Appendix Tables C2-C4 report the results from the coarsened exact matching analysis, which are very similar to the results from our primary analyses.

[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 nearest adjacent reliable credit report before the flagged balances and the nearest adjacent reliable credit report after the flagged balances.

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 C1 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) to a much lesser extent 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 *n*th calendar month of tenure. We scale the last month's contributions of each individual to approximate how much that individual would have contributed by tenure month *n* had she experienced the benchmark number of paydays.[19]

Second, as explained in Section II, 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

[18] As explained in Section III, 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.

[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 C5-C7 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 C8-C10 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 Table C8 shows a significant negative effect of automatic enrollment on debt excluding first mortgages and auto loans, although the 95% confidence intervals almost always include the

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.

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

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.

period leading up to and including the June or December that is the employee's *n*th 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

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

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

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

where the only difference relative to the previous regression equation (1) is that the β coefficients on the employee characteristic control variables X_i 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

[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.

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]

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.

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. 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 less. 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.

[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.

Although our data do not contain withdrawal information, we can estimate an upper bound on how much hardship withdrawals undo the automatic enrollment contribution effect. Such withdrawals must be at least $1,000 and require the employee to stop contributing to the TSP for at least six months afterwards. For the bounding exercise, we assume that an employee has taken a hardship withdrawal on date t equal to 100% of her employee contributions to date if the employee was contributing to the TSP on date t, has at least $1,000 of cumulative employee contributions as of date t, and stops contributing for at least six months after date t.[25] Using this approach, we find that hardship withdrawals are rare. The estimated impact of automatic enrollment on cumulative TSP contributions at 43-48 months of tenure is 4.1% of first-year income, and subtracting our upper-bound measure of hardship withdrawals from contributions reduces this estimated impact by only 0.1% of first-year income.

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 and 5, which plot credit outcomes for the pre-AE and post-AE cohorts in June and December of the years 2007-2014.[26] 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.

[25] 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.
[26] See Online Appendix Figures C2 and C3 for analogous graphs of auto loan and first mortgage balances, which also show evidence of additive calendar time effects.

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

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

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 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.[27] The tenure buckets included in the summation are $\{\leq -18$, –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:

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

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

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

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 of equation (4) for two pre-AE individuals who are one tenure bucket apart at date *t*:

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

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

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

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

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[28], 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. Note that this line of reasoning provides the intuition for how our assumptions allow us to identify all of our coefficients, but the actual estimation of coefficients and standard errors is implemented by directly applying ordinary least squares regression to equation (3) or equation (4), clustering standard errors at the employee level.

Finally, there are two credit outcome variables for which equations (3) and (4) are inappropriate regression models. To examine whether automatic enrollment increases the likelihood of significant drops in credit scores, we create indicator variables for whether an employee has a Vantage score as of a given date that is at least 25 points or at least 50 points lower than her Vantage score in the credit file observation immediately prior to hire. Figure 4 shows that Vantage score *levels* seem subject to a common additive calendar time effect η_t,

[28] 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.

which means that the *change* in Vantage score between t and t' is subject to $\eta_{t'} - \eta_t$. Consider two employees observed at t', one hired at t_0 and the other hired at t_1. In a regression of Vantage score change since hire, controlling for date-of-observation fixed effects will not adequately capture the fact that the former employee is subject to $\eta_{t'} - \eta_{t_0}$ while the latter is subject to $\eta_{t'} - \eta_{t_1}$.

Instead, to analyze the likelihood of significant credit score drops since hire, we use the *change* over a six-month period in the indicator variable for having a credit score at least 25 or 50 points lower than at hire as the dependent variable in regression equation (1) or (2), and we report the cumulative sum of γ_s coefficient estimates up to and including the γ_s for the tenure bucket of interest. This technique is the same one we used for estimating the effect of automatic enrollment on cumulative TSP contributions.

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

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 fixed effects and calendar time effects.[29] 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 6 show that additionally controlling for demographic × tenure interactions barely moves the point estimates. At 43-48 months of 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

[29] Regressions with Vantage score as the outcome variable exclude 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 regressions with other credit outcome variables, so those regressions have more observations than the Vantage score regressions.

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 on average.

Our primary debt balance outcome of interest is debt balances 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.[30] 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.[31] 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 without controlling for demographic × tenure interactions. As with the credit score regressions, there is no 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

[30] 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.

[31] In Appendix Table C11, we separately estimate the effect of automatic enrollment on each subcomponent 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.

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 7, 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%].

The results in Table 3 indicate that automatic enrollment does not on average cause a deterioration in creditworthiness or an increase in the types of debt balances most likely to be associated with decreases in financial net worth. To investigate the effect of automatic enrollment on the probability of financial distress, we examine eight measures of adverse credit outcomes.

The first two outcomes we consider are the likelihood of having a Vantage score at a given tenure level that is at least 25 points or at least 50 points lower than the Vantage score in the credit file observation immediately prior to hire. We choose these thresholds because a late payment causes a Vantage score drop of about 50 points, and a balance moving from derogatory status to collections status causes an additional drop of about 25 points. Among employees in the pre-AE and post-AE cohorts who remained employed through 43-48 months of tenure, 22.8% had a Vantage score at 43-48 months of tenure that was at least 25 points lower than the Vantage score in the credit file observation immediately prior to hire, and 13.7% had a drop of at least 50 points at that tenure level.

The first two columns of Table 4 show that the automatic enrollment effect on the likelihood of a ≥25 point Vantage score drop at 43-48 months of tenure is a decrease of 0.8 percentage points when not controlling for demographic × tenure interactions, and a decrease of 1.0 percentage points when including those interactions in the regression model. The last two columns of Table 4 show that the effect on the likelihood of a ≥50 point Vantage score drop at 43-48 months of tenure is an increase of 0.2 percentage points when not controlling for the demographic × tenure interactions and an increase of 0.1 percentage points when controlling for the interactions. None of these estimates are statistically significant, and the confidence intervals are tight around zero relative to the overall prevalence of Vantage score decreases of these magnitudes.

In Table 5, we focus on the effect of automatic enrollment on balances that are late (columns 1-2), derogatory (columns 3-4), or in collections (columns 5-6) using the regression model that controls for demographic × tenure interactions. Auto loan and first mortgage balances are included in these three measures if they are late, derogatory, or in collections, respectively. The odd-numbered columns show the extensive margin effect of automatic enrollment, and the even-numbered columns show the effect on balances as a fraction of first-year annualized salary. At 43-48 months of tenure, the point estimate for the effect of automatic enrollment is negative in all six regressions and is never statistically significant. Thus, Table 5 provides no evidence across a number of measures that automatic enrollment increases the probability of financial distress.

Although we find little evidence in the full sample that automatic enrollment leads to adverse credit outcomes or increased debt excluding auto loans and first mortgages, in Table 6, we investigate whether there is evidence of such effects in 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 examine 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 immediately prior to hire is below 620 (approximately the bottom quintile of our sample).

We focus on effects at 43-48 months of tenure and use the regression model that controls for demographic × tenure interactions, augmenting the model to include the interaction of all explanatory variables with an indicator for being in the subpopulation of interest. For each subpopulation and outcome variable, Table 6 reports the estimated effect of automatic enrollment both in absolute terms (odd-numbered columns) and relative to the estimated effect for all other employees in the sample (even-numbered columns).

The first row in Table 6 shows that indeed, these subpopulations' contributions respond especially strongly to automatic enrollment. Whereas the effect on cumulative total TSP contributions in the overall sample is 4.1% of first-year salary (see Table 2), the point estimates for the subpopulations in Table 6 range from 4.2% for employees less than 30 years old to 7.5%

for those with a starting annualized salary less than $34,000 or baseline credit score below 620. The differences between the treatment effect estimates for the subpopulations of interest and the treatment effect estimates for others in the sample are statistically significant for four of the six subpopulations.

Looking at the estimated treatment effects for credit outcomes in Table 6, we see little evidence that automatic enrollment leads to financial distress for the subpopulations that we study, although small sample sizes make some of our inferences imprecise. The only statistically significant effect is a 4.7 percentage point increase in the likelihood that Black employees have a Vantage credit score that dropped by ≥50 points. Two-thirds of the insignificant point estimates are on the side of decreased debt levels or financial distress. Table 6 includes many hypothesis tests, so under the null hypothesis of no effects of automatic enrollment, we would expect to see some large point estimates and a few statistically significant estimates due to type I error. Overall, we conclude that the table presents little evidence that automatic enrollment causes adverse credit outcomes, even in subpopulations for which automatic enrollment has particularly large effects on TSP contributions.

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

In this section, we analyze auto debt and first mortgages separately from other debts. In contrast to the types of debt studied in the previous section, increases in auto debt and first mortgages are typically associated with the acquisition of an asset. Because both assets and liabilities increase upon origination of these loans, the inference we should draw about net worth from changes in these debt balances is ambiguous. Online Appendix B presents a framework for thinking about how an increase in auto or first mortgage debt does or does not affect net worth.

The first column of Table 7 shows that when we control only for calendar time and person fixed effects, we estimate that automatic enrollment significantly increases auto debt balances from tenure months 31-36 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. At 43-48 months of tenure, the effect on auto debt is 1.1% of first-year income, with a 95% confidence interval of [–0.1%, 2.3%].

The estimated effect of automatic enrollment on first mortgage debt, shown in the last two columns of Table 7, 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 an increase of 9.4% of income. 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%].

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 cannot analyze the effect of automatic enrollment on the types of mortgages that employees choose (e.g., interest-only versus amortizing), as our data do not contain this information. However, we have 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 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 increases by more than 10% of first-year income, (2) the individual's number of first mortgage accounts does not change, and (3) the individual's residential ZIP code does not change according to personnel records. These criteria are 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 criteria 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,[32] 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 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.

Overall, there is limited evidence that automatic enrollment increases auto and first mortgage debt. The implications of such an increase for net worth are ambiguous, and we do not have the necessary data on auto or home asset values that would be necessary for resolving the

[32] 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.

ambiguity. Nonetheless, we can investigate whether automatic enrollment leads to adverse credit consequences that are directly related to auto and first mortgage debt. In Table 8, we use regression equation (4) to study four outcome variables: an indicator for the individual's most recently originated auto loan being delinquent within the previous six months, an indicator for the individual's most recently originated first mortgage being delinquent within the previous six months, an indicator for having a first mortgage presently in foreclosure, and total balances on presently foreclosed first mortgages divided by annualized first-year salary.[33] Automatic enrollment does not have statistically significant effects on the two delinquency outcomes, but there is some evidence that automatic enrollment has a positive effect on first mortgage foreclosure. At 43-48 months of tenure, the point estimates indicate that automatic enrollment increases the likelihood of having a first mortgage in foreclosure by 0.2 percentage points and increases balances on foreclosed first mortgages by 1.1% of first-year income. Even though neither of these estimates is statistically significant, estimates of similar magnitude are statistically significant at lower tenures. Of course, these results must be interpreted with caution, as we have studied a large number of credit outcomes in this paper,[34] implying that we are likely to find some statistically significant effects due to type I error even if the null hypothesis of no automatic enrollment effects is true.

We also examine automatic enrollment effects on auto debt and first mortgages for subpopulations that are likely to have large effects of automatic enrollment on TSP contributions. Table 9 presents results at 43-48 months of tenure for the same subpopulations that we study in Table 6. Again, many of our estimates are imprecise, both because of small sample sizes and the large variance of first mortgage balances. For employees with low salaries, automatic enrollment significantly increases auto debt by 5.2% of first-year pay, which is significantly larger than the effect for other employees in the sample. It also has a large but not statistically significant effect of increasing first mortgage balances by 13.7% of first-year pay. For young employees, the automatic enrollment effect on balances on foreclosed first mortgages is a statistically significant 1.6% of first-year pay increase. Employees with only a high school education exhibit statistically significant increases in auto debt of 2.6% of first-year pay and first mortgage debt of 11.7% of

[33] We would like to examine outcomes such as delinquency and delinquent balances for auto loans and first mortgages other than the most recently originated ones, but our credit bureau data do not include these variables.
[34] There are 16 credit outcomes studied in Tables 3, 4, 5, 7, and 8, although some of these outcomes are correlated with each other.

first-year pay; both of these estimates are significantly larger than the estimates for the remainder of the sample. Turning to the subpopulations of employees with low baseline credit scores, Black employees, and Hispanic employees, we do not see any positive and statistically significant effects of automatic enrollment. For Hispanic employees, automatic enrollment is estimated to significantly *decrease* balances on foreclosed mortgages by 6.7% of first-year pay, which is significantly different than the effect among non-Hispanics. We also estimate a large positive effect on first mortgage balances among Hispanics of 14.6% of first-year pay, although the standard error is very large at 19.3% of first-year pay.

IX. Limitations and extensions

While this paper represents the first investigation to focus on the effects of automatic enrollment on credit outcomes—an important domain for potential unintended consequences—one limitation of our analysis is that we do not observe the full balance sheet of the employees in our sample. On the asset side, we do not have data on TSP investment returns, withdrawals, and loans, or on assets beyond the TSP. On the liability side, we do not have data on some sources of credit, such as payday lenders. In addition, we do not observe borrowing by other household members unless that borrowing is also linked to the employee's credit file.

To explore the extent to which our results might lead to misleading inferences about automatic enrollment's effects on *household* outcomes, we test whether our results differ for single employees versus employees who are members of a couple.[35] Single employees have less scope for automatic enrollment to affect household credit outcomes in ways that are not observable in the employee credit records. Therefore, differences in observed automatic enrollment effects by marital status would suggest that automatic enrollment has an important impact on unobserved credit outcomes of other household members of non-single employees, although single employees may differ from non-single employees in other ways too.

We do not directly observe marital status for all employees in our sample, so we must first construct proxies for being single. Our first proxy considers an employee to be single if they always elected health insurance coverage for a single person while employed as a civilian by the

[35] We use the term "single" to encompass all marital statuses that are likely to indicate that there is only one adult in the household. In particular, a person who is separated, divorced, or widowed is considered "single" for our purposes.

Army.[36] Our second proxy uses information that is only available for employees who were previously uniformed personnel in the Army. For these employees, the Army has administrative records of marital status, and we use the most recent record within six months of an employee's hire date as a civilian. When using this second proxy, we limit the analysis sample to employees for whom we observe such a record. Our third proxy combines the first two proxies, using information in the uniformed personnel administrative records when available and using health insurance elections otherwise.

We begin by validating our first proxy for being single. In the 2010-2016 Current Population Survey, we examine the 16,381 individuals who are full-time civilian federal government employees. For this group overall, 35.7% of individuals are single. Out of the 16,381 individuals, 4,820 elect employer-provided health insurance coverage for a single person, and 74.6% of those 4,820 individuals are single, suggesting that our first proxy is a good predictor of being single. We also examine employees in our primary analysis sample for whom we have uniformed personnel administrative records of marital status. Within this group, 26.9% of individuals are single in the administrative records.[37] Among those in this group who are classified as single according to the first proxy, 77.5% are single in the administrative records, again suggesting that our first proxy is a good predictor of being single.

In Online Appendix Table C12, we estimate the effects of automatic enrollment at 43-48 months of tenure for all outcome variables studied in Tables 2-5, 7, and 8, using regression equations (2) and (4) modified to include the interaction of all explanatory variables with one of our proxies for being single. Out of all the tests (16 credit outcomes × 3 marital status proxies), there is only one test out of 48 that shows a statistically significant (using a 5% threshold) difference between singles and others: under our second marital status proxy, the effect on debt excluding auto debt and first mortgages is 8.5% of annualized first-year salary greater for single

[36] We observe health insurance elections at a monthly frequency, and we classify an employee as single even if that employee has up to two missing monthly health insurance elections, provided that all non-missing monthly health insurance elections are for single coverage. If an employee transitions from single coverage to family coverage or vice versa, we classify the employee as not single throughout the sample period because we do not know whether the transitions correspond to changes in household structure or correspond only to changes in insurance choices (with a stable household structure).

[37] This percentage is slightly different from the 25.0% implied by the last two rows of Online Appendix Table C12 because these last two rows focus on the subset of individuals who remained employed at least through 43-48 months of tenure.

employees. In light of multiple hypothesis testing, there is little evidence that automatic enrollment effects on credit outcomes differ for single employees versus other employees.

To further explore the impact of automatic enrollment on other members of an employee's household, we estimate effects on debts that employees jointly hold with other individuals, as well as effects on debts for which employees are "authorized users" but other individuals are the primary account holders. In Online Appendix Table C13, we apply regression equation (4) to six new outcome variables: total debt that the employee jointly holds with other individuals, mortgage debt that the employee jointly holds with other individuals, non-mortgage installment debt that the employee jointly holds with other individuals, revolving debt that the employee jointly holds with other individuals, total debt associated with the employee as an authorized user on accounts where someone else is the primary account holder, and bankcard and charge card debt associated with the employee as an authorized user on accounts where someone else is the primary account holder.[38] All of these variables are normalized by annualized first-year salary. Also, all of these variables are captured in outcome variables that have been previously analyzed in this paper (e.g., authorized user bankcard and charge card debt is a subset of the employee's debt excluding auto loans and first mortgages). Nonetheless, joint and authorized user borrowing offer a (non-comprehensive) window into the debts of other household members.

At 43-48 months of tenure, none of the automatic enrollment effects on joint debt are statistically significant, although the point estimates suggest that to the extent total debt does increase, increases in joint debt could constitute an important component of that increase. The point estimate for the automatic enrollment effect on joint mortgage debt is 1.8% of first-year salary, which is close to the 2.4% point estimate for the effect on all (joint and non-joint) mortgage debt.[39] Similarly, the point estimate for the automatic enrollment effect on joint non-

[38] An example is helpful for clarifying the definitions of the last two variables. Consider an employee's spouse who is the primary account holder for a bankcard account and who makes the employee an authorized user. The employee's balances on the account (e.g., from swiping the authorized user card at a merchant's payment terminal) are included in both variables. The spouse's balances are not.

[39] We do not observe balances on joint first mortgages separately from balances on other joint mortgages. The point estimate of 2.4% for the effect on all mortgage debt is the sum of the 2.2% effect for first mortgage debt (Table 7) and the 0.2% effect for second mortgage debt (Online Appendix Table C11).

mortgage debt is 2.7% – 1.8% = 0.9% of first-year salary, which is greater than the 0.3% point estimate for the effect on total (joint and non-joint) debt excluding first and second mortgages.[40]

The effects of automatic enrollment on total authorized user debt and authorized user bankcard and charge card debt are statistically significant at 25-30 months of tenure and then again from 37-42 months to 49-53 months of tenure. At 43-48 months of tenure, automatic enrollment increases total authorized user debt by 0.3% of first-year salary and increases authorized user bankcard and charge card debt by 0.2% of first-year salary. Thus, automatic enrollment seems to have a positive effect on debts for which other members of the employee's household are liable, although the effects are small in magnitude.

Finally, keeping in mind the caveat that we do not observe all household debt for employees, in Appendix Table C14, we calculate the effects of automatic enrollment on aggregates of the debt variables that we do observe, as well as its effects on cumulative TSP contributions net of these debt aggregates. 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 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.[41]

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

[40] The point estimate of 0.3% for the automatic enrollment effect on all debt excluding first and second mortgages is the -0.6% effect for debt excluding auto loans and first mortgages (Table 3) plus the 1.1% effect for auto debt (Table 7) minus the 0.2% effect for second mortgages (Online Appendix Table C11). Note that we do not observe balances on jointly held auto loans separately from balances on other jointly held non-mortgage installment debt. Also, total joint debt is not equal to joint mortgage debt plus joint non-mortgage installment debt plus joint revolving debt because the credit bureau tracks a residual category of joint debt that is not counted in any of those three categories.

[41] Since we do not have information on employees' current and future marginal tax rates, the NETn measures do not adjust for the fact that TSP contributions were made with before-tax dollars (at least until Roth contributions became available in July 2012) and debts must be paid mostly with after-tax dollars. We compute standard errors of NETn 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.

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 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.

X. 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 there is little evidence that automatic enrollment increases financial distress. Automatic enrollment has a precisely estimated zero effect on credit scores, no significant effect on debt excluding auto loans and first mortgages, and no significant positive effects on a range of measures of adverse credit outcomes, with the exception of first mortgage foreclosures. Furthermore, we find little evidence that automatic enrollment increases financial distress within subpopulations that are particularly likely to have large automatic enrollment effects on TSP contributions, although some subpopulation effects are estimated with little precision. It would be valuable for future research to continue examining the effects of automatic enrollment on components of household balance sheets beyond savings in retirement plans, as such effects are important considerations when evaluating the welfare consequences of automatic enrollment policies.

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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 likelihood of large Vantage credit score drops

Each column reports regression-adjusted effects of automatic enrollment on an indicator for a Vantage credit score drop of the size indicated in the column heading as of the tenure months in the row label, relative to the credit score observed immediately prior to hire. The regressions are estimated according to equation (1) or (2). 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 dropped by ≥25 points	Vantage credit score dropped by ≥25 points	Vantage credit score dropped by ≥50 points	Vantage credit score dropped by ≥50 points
Tenure 1 to 6	-0.002 (0.005)	-0.004 (0.005)	0.000 (0.004)	-0.001 (0.004)
Tenure 7 to 12	-0.008 (0.007)	-0.010 (0.007)	-0.001 (0.005)	-0.002 (0.005)
Tenure 13 to 18	-0.009 (0.007)	-0.012 (0.007)	-0.001 (0.006)	-0.003 (0.006)
Tenure 19 to 24	-0.009 (0.008)	-0.012 (0.008)	-0.002 (0.006)	-0.004 (0.006)
Tenure 25 to 30	-0.006 (0.008)	-0.009 (0.008)	0.000 (0.006)	-0.001 (0.006)
Tenure 31 to 36	-0.006 (0.008)	-0.007 (0.008)	-0.003 (0.007)	-0.004 (0.007)
Tenure 37 to 42	-0.008 (0.009)	-0.008 (0.009)	-0.001 (0.007)	-0.001 (0.007)
Tenure 43 to 48	-0.008 (0.009)	-0.010 (0.009)	0.002 (0.007)	0.001 (0.007)
Tenure 49 to 53	-0.005 (0.010)	-0.005 (0.010)	-0.004 (0.008)	-0.004 (0.008)
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 5. Effect of automatic enrollment on credit delinquency

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 (4). The dependent variables capturing debt amounts 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.

	Has late balances	Amount of late balances	Has derogatory balances	Amount of derogatory balances	Has balances in collection	Amount of balances in collection
Tenure ≤ -18	0.003 (0.004)	-0.001 (0.001)	-0.004 (0.003)	0.001 (0.002)	-0.005 (0.004)	-0.001 (0.001)
Tenure -17 to -12	0.001 (0.003)	-0.002* (0.001)	0.001 (0.003)	0.000 (0.001)	-0.004 (0.003)	-0.001 (0.001)
Tenure -11 to -6	-0.003 (0.003)	-0.001 (0.001)	0.000 (0.003)	0.000 (0.001)	-0.001 (0.002)	-0.001* (0.000)
Tenure 1 to 6	-0.002 (0.003)	-0.002** (0.001)	0.000 (0.003)	-0.001 (0.001)	0.001 (0.002)	0.000 (0.000)
Tenure 7 to 12	-0.002 (0.004)	-0.001 (0.001)	0.000 (0.004)	0.000 (0.001)	-0.003 (0.003)	0.000 (0.001)
Tenure 13 to 18	-0.002 (0.004)	-0.001 (0.001)	-0.002 (0.004)	0.000 (0.001)	-0.003 (0.004)	0.000 (0.001)
Tenure 19 to 24	-0.004 (0.004)	-0.001 (0.001)	-0.005 (0.004)	0.000 (0.001)	-0.002 (0.004)	0.000 (0.001)
Tenure 25 to 30	0.001 (0.004)	-0.001 (0.001)	-0.007 (0.004)	0.000 (0.001)	-0.003 (0.005)	0.000 (0.001)
Tenure 31 to 36	0.000 (0.004)	-0.001 (0.001)	-0.009* (0.004)	0.000 (0.001)	-0.007 (0.005)	-0.001 (0.001)
Tenure 37 to 42	-0.002 (0.005)	-0.001 (0.001)	-0.003 (0.004)	0.001 (0.001)	-0.003 (0.005)	-0.001 (0.001)
Tenure 43 to 48	-0.004 (0.005)	-0.000 (0.001)	-0.004 (0.005)	-0.001 (0.001)	-0.002 (0.006)	-0.001 (0.001)
Tenure 49 to 53	-0.007 (0.005)	-0.001 (0.001)	-0.001 (0.005)	-0.001 (0.002)	-0.002 (0.007)	0.000 (0.001)
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	809,385	809,385	809,385

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

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

Each pair of cells contains estimates from its own separate regression. The left cell within a pair reports 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 right cell within a pair reports by how much the treatment effect in the left cell differs from the same treatment effect at 43-48 months of tenure for the complement of the group in the column header. The contribution regressions and the regressions that have an indicator for a credit score drop as the outcome variable are estimated according to equation (2), modified to include the interactions of all explanatory variables with an indicator for being in the group identified in the column header. The other regressions are estimated according to equation (4), modified in the same way. All dependent variables except for Vantage credit score and indicator variables are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses.

	Salary < $34,000		Age < 30		High school only	
	AE effect	Relative to others in sample	AE effect	Relative to others in sample	AE effect	Relative to others in sample
Cumulative total TSP contributions	0.075** (0.009)	0.041** (0.010)	0.042** (0.008)	0.000 (0.009)	0.056** (0.006)	0.027** (0.008)
Cumulative employee TSP contributions	0.029** (0.007)	0.018* (0.008)	0.014* (0.006)	-0.001 (0.007)	0.021** (0.005)	0.013 (0.007)
Vantage credit score	3.8 (3.4)	4.4 (3.7)	-1.9 (2.9)	-2.6 (3.2)	0.2 (1.9)	0.2 (2.5)
Debt excl. auto and first mortgages	0.004 (0.031)	0.011 (0.032)	-0.021 (0.017)	-0.020 (0.020)	0.006 (0.014)	0.022 (0.019)
Vantage credit score dropped by ≥25 points	-0.037 (0.024)	-0.033 (0.026)	-0.020 (0.020)	-0.010 (0.022)	-0.012 (0.014)	-0.004 (0.018)
Vantage credit score dropped by ≥50 points	-0.017 (0.020)	-0.022 (0.022)	-0.002 (0.016)	-0.004 (0.018)	-0.009 (0.012)	-0.016 (0.015)
Has late balances	-0.025 (0.014)	-0.025 (0.014)	-0.003 (0.009)	0.002 (0.011)	-0.005 (0.008)	-0.001 (0.010)
Amount of late balances	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.001)	-0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)
Has derogatory balances	-0.004 (0.014)	0.000 (0.015)	-0.004 (0.010)	-0.002 (0.011)	-0.004 (0.008)	0.000 (0.009)
Amount of derogatory balances	-0.001 (0.005)	-0.001 (0.005)	0.000 (0.002)	0.001 (0.002)	0.002 (0.002)	0.005 (0.003)
Has balances in collection	-0.017 (0.017)	-0.017 (0.018)	0.022 (0.013)	0.029* (0.014)	-0.008 (0.010)	-0.011 (0.012)
Amount of balances in collection	-0.004 (0.004)	-0.004 (0.005)	0.001 (0.002)	0.002 (0.003)	-0.004 (0.002)	-0.005 (0.002)
# of employees at 43-48 months	5,882		7,358		15,576	

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

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

	Baseline Vantage<620		Black		Hispanic	
	AE effect	Relative to others in sample	AE effect	Relative to others in sample	AE effect	Relative to others in sample
Cumulative total TSP contributions	0.075** (0.007)	0.042** (0.008)	0.067** (0.012)	0.029* (0.012)	0.058** (0.020)	0.017 (0.020)
Cumulative employee TSP contributions	0.034** (0.005)	0.025** (0.006)	0.026** (0.009)	0.013 (0.010)	0.029 (0.016)	0.016 (0.016)
Vantage credit score	4.8 (3.1)	5.4 (3.4)	-1.6 (4.1)	-1.9 (4.2)	6.5 (7.7)	6.5 (7.8)
Debt excl. auto and first mortgages	0.038 (0.028)	0.054 (0.030)	-0.026 (0.033)	-0.023 (0.034)	-0.010 (0.046)	-0.003 (0.047)
Vantage credit score dropped by ≥25 points	-0.030 (0.018)	-0.024 (0.020)	0.028 (0.029)	0.042 (0.030)	-0.003 (0.055)	0.008 (0.056)
Vantage credit score dropped by ≥50 points	-0.005 (0.014)	-0.007 (0.016)	0.047* (0.024)	0.051* (0.025)	0.035 (0.045)	0.035 (0.045)
Has late balances	-0.032 (0.020)	-0.032 (0.021)	0.002 (0.019)	0.006 (0.019)	-0.016 (0.027)	-0.012 (0.027)
Amount of late balances	-0.006 (0.006)	-0.007 (0.006)	-0.003 (0.005)	-0.003 (0.006)	-0.006 (0.020)	-0.003 (0.020)
Has derogatory balances	-0.029 (0.020)	-0.030 (0.020)	-0.001 (0.018)	0.003 (0.018)	-0.016 (0.027)	-0.013 (0.027)
Amount of derogatory balances	-0.004 (0.006)	-0.005 (0.006)	-0.001 (0.006)	-0.001 (0.006)	0.003 (0.008)	0.004 (0.008)
Has balances in collection	0.006 (0.022)	0.010 (0.023)	0.026 (0.022)	0.032 (0.023)	0.013 (0.034)	0.015 (0.035)
Amount of balances in collection	-0.007 (0.006)	-0.007 (0.006)	-0.001 (0.004)	0.000 (0.004)	-0.001 (0.007)	0.000 (0.007)
# of employees at 43-48 months	6,572		4,009		1,448	

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

Table 7. 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 8. Effect of automatic enrollment on auto loan delinquency, first mortgage delinquency, and first mortgage foreclosure

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 (4). The dependent variable capturing balances on foreclosed first mortgages 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.

	Most recent auto loan delinquent in last 6 months	Most recent first mortgage delinquent in last 6 months	Has presently foreclosed first mortgage	Total balances on presently foreclosed first mortgages
Tenure ≤ -18	0.000 (0.003)	0.001 (0.002)	0.001 (0.001)	0.006 (0.005)
Tenure -17 to -12	0.000 (0.002)	0.002 (0.002)	0.001 (0.001)	0.005 (0.005)
Tenure -11 to -6	-0.001 (0.002)	0000 (0.002)	0.000 (0.001)	0.005 (0.004)
Tenure 1 to 6	0.000 (0.002)	-0.001 (0.002)	0.000 (0.001)	0.001 (0.004)
Tenure 7 to 12	0.002 (0.002)	0.000 (0.002)	0.002* (0.001)	0.011* (0.005)
Tenure 13 to 18	-0.001 (0.003)	0.002 (0.003)	0.001 (0.001)	0.007 (0.005)
Tenure 19 to 24	-0.001 (0.003)	0.003 (0.003)	0.001 (0.001)	0.006 (0.005)
Tenure 25 to 30	-0.001 (0.003)	0.002 (0.003)	0.002 (0.001)	0.012* (0.006)
Tenure 31 to 36	0.000 (0.003)	0.001 (0.003)	0.002 (0.001)	0.011 (0.006)
Tenure 37 to 42	-0.002 (0.003)	0.003 (0.003)	0.003 (0.001)	0.012* (0.006)
Tenure 43 to 48	-0.003 (0.004)	0.003 (0.003)	0.002 (0.001)	0.011 (0.006)
Tenure 49 to 53	-0.002 (0.004)	0.003 (0.004)	0.000 (0.002)	0.002 (0.007)
Calendar time fixed effects	Yes	Yes	Yes	Yes
Person fixed effects	Yes	Yes	Yes	Yes
Demographic × tenure controls	Yes	Yes	Yes	Yes
# of obs. (*N*)	809,385	809,385	809,385	809,385

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

Table 9. More effects of automatic enrollment on subpopulations at 43-48 months of tenure

Each pair of cells contains estimates from its own separate regression. The left cell within a pair reports 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 right cell within a pair reports by how much the treatment effect in the left cell differs from the same treatment effect at 43-48 months of tenure for the complement of the group in the column header. All regressions are estimated according to equation (4), modified to include the interactions of all explanatory variables with an indicator for being in the group identified in the column header. All dependent variables except for indicator variables are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses.

	Salary < $34K		Age < 30		High school only	
	AE effect	Relative to others in sample	AE effect	Relative to others in sample	AE effect	Relative to others in sample
Auto debt	0.052* (0.023)	0.049* (0.023)	0.026 (0.014)	0.018 (0.015)	0.026* (0.011)	0.027* (0.013)
First mortgage debt	0.137 (0.117)	0.134 (0.123)	-0.080 (0.086)	-0.126 (0.095)	0.117* (0.059)	0.169* (0.076)
Most recent auto loan delinquent, last 6 mos.	-0.009 (0.011)	-0.007 (0.011)	0.007 (0.007)	0.012 (0.008)	-0.010 (0.006)	-0.010 (0.007)
Most recent first mortg. delinquent, last 6 mos.	-0.007 (0.008)	-0.012 (0.009)	-0.009 (0.005)	-0.015* (0.006)	-0.001 (0.006)	-0.007 (0.007)
Has foreclosed first mortgage	-0.002 (0.004)	-0.005 (0.004)	0.003 (0.002)	0.001 (0.003)	0.002 (0.002)	0.000 (0.003)
Balances on foreclosed first mortgages	0.000 (0.019)	-0.013 (0.020)	0.016* (0.007)	0.006 (0.011)	0.018 (0.010)	0.012 (0.013)
# of employees at 43-48 months	5,882		7,358		15,576	

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

Table 9 continued. More effects of automatic enrollment on subpopulations at 43-48 months of tenure

	Baseline Vantage<620		Black		Hispanic	
	AE effect	Relative to others in sample	AE effect	Relative to others in sample	AE effect	Relative to others in sample
Auto debt	0.034	0.028	-0.008	-0.021	-0.015	-0.027
	(0.018)	(0.019)	(0.022)	(0.023)	(0.036)	(0.036)
First mortgage debt	-0.012	-0.032	0.010	-0.017	0.146	0.127
	(0.091)	(0.099)	(0.122)	(0.128)	(0.193)	(0.197)
Most recent auto loan delinquent, last 6 mos.	-0.027	-0.028	0.005	0.009	-0.006	-0.003
	(0.016)	(0.017)	(0.015)	(0.015)	(0.020)	(0.020)
Most recent first mortg. delinquent, last 6 mos.	-0.001	-0.004	0.001	-0.002	0.012	0.010
	(0.014)	(0.014)	(0.013)	(0.013)	(0.019)	(0.019)
Has foreclosed first mortgage	0.007	0.005	0.008	0.006	-0.006	-0.008
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Balances on foreclosed first mortgages	0.039	0.035	0.009	-0.002	-0.067*	-0.081**
	(0.029)	(0.029)	(0.028)	(0.029)	(0.030)	(0.031)
# of employees at 43-48 months	6,572		4,009		1,448	

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

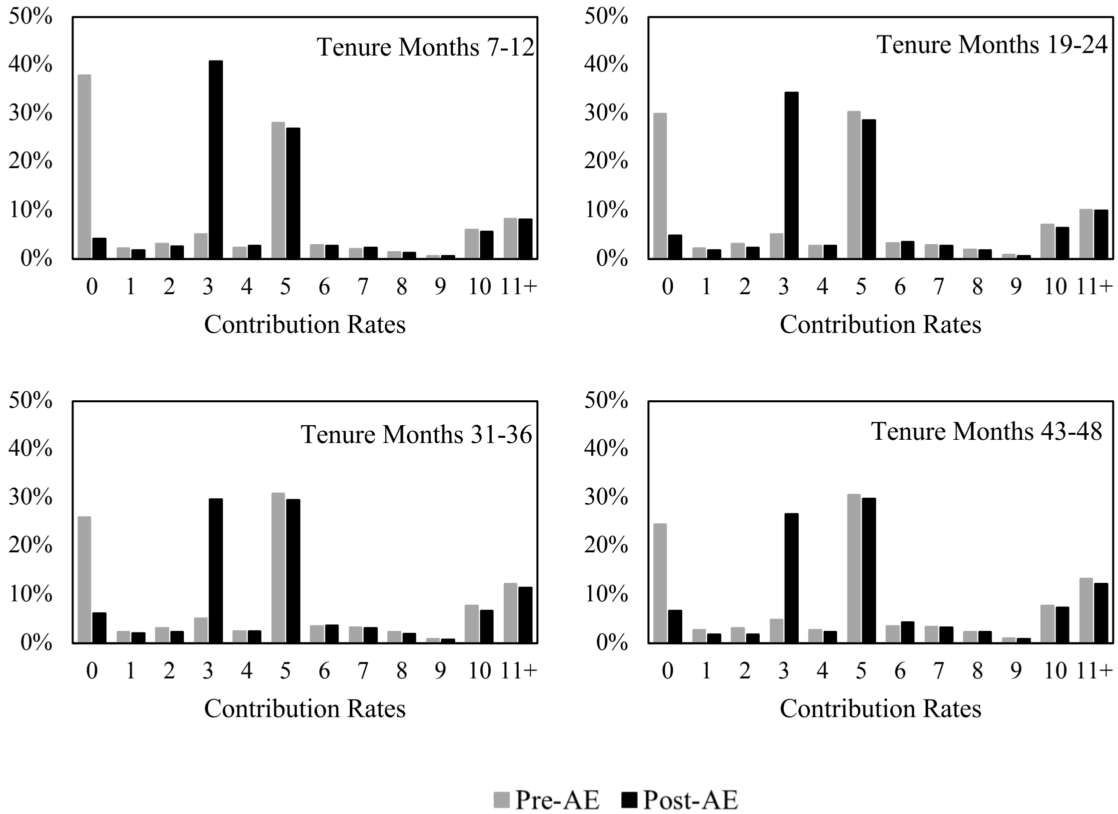

Figure 1. Distributions of employee contribution rates by cohort. The employee contribution rate is the sum of employee before-tax and Roth contribution rates, expressed as percentages of pay, and is measured over the entirety of the June or the December when the employee reached the tenure level indicated. 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.

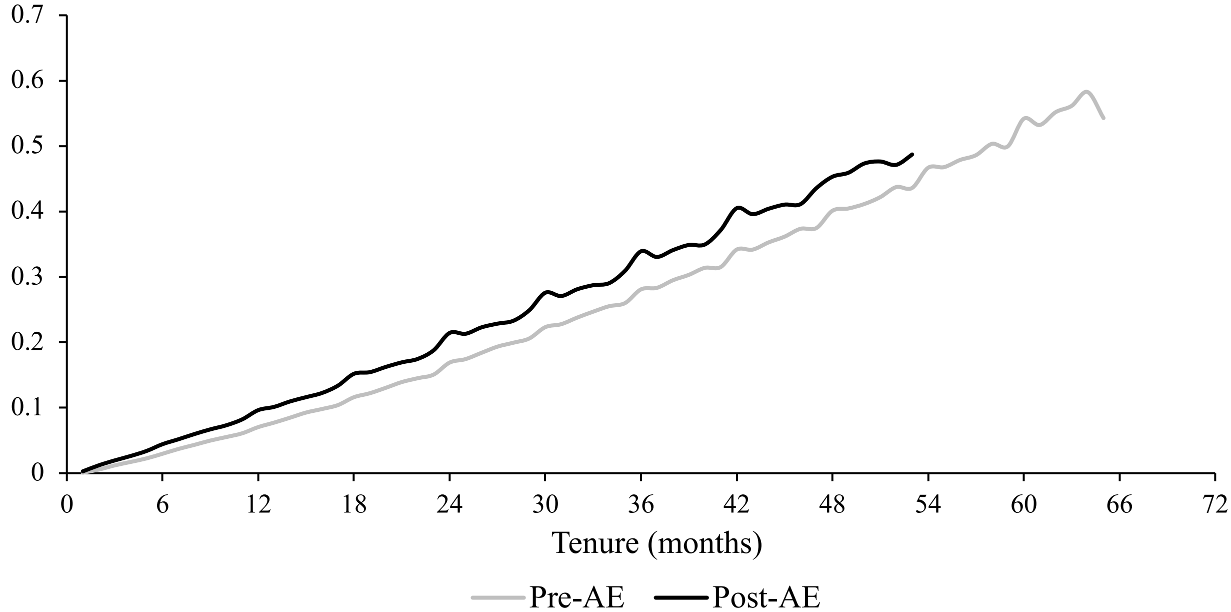

Figure 2. The ratio of cumulative total TSP contributions to annualized first-year pay. Every point in the graphed series corresponds to individuals who reached the tenure level indicated on the horizontal axis in a June or a December. The pre-AE cohort consists of August 2009 – July 2010 hires, and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

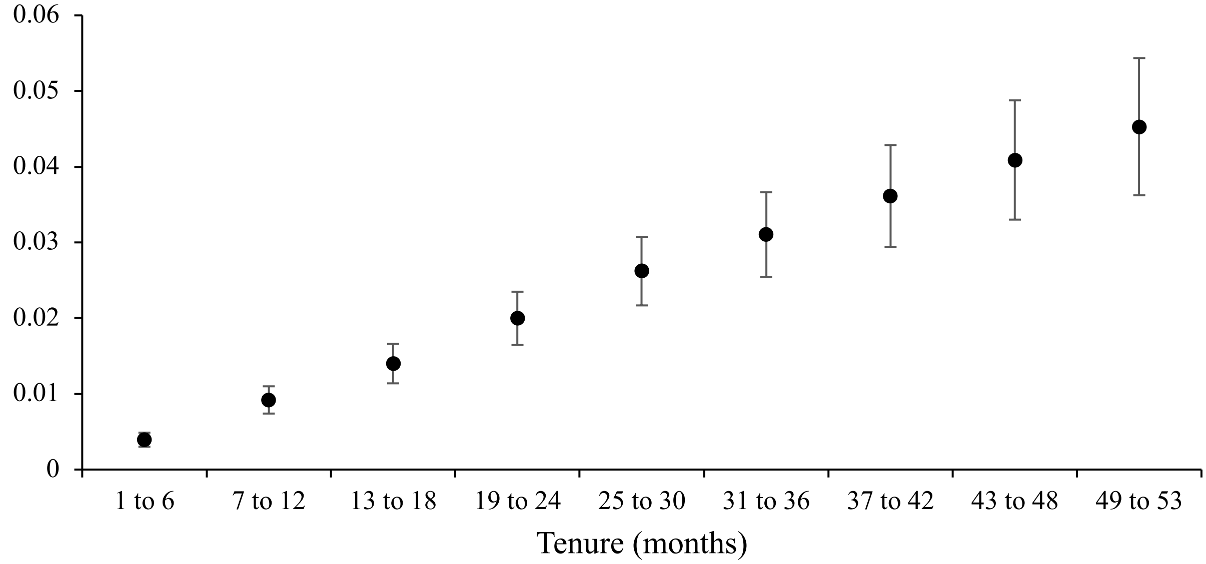

Figure 3. The effect of automatic enrollment on cumulative total TSP contributions to annualized first-year pay ratio. The estimates come from the regression in column 2 of Table 2. Point estimates and 95% confidence intervals are shown.

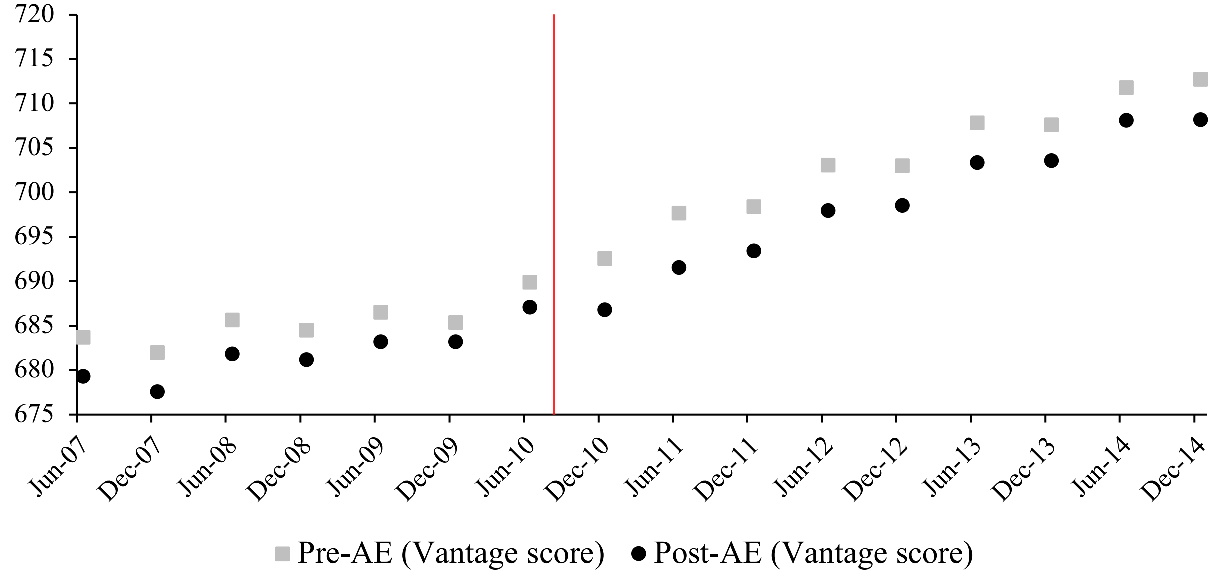

Figure 4. Average Vantage score at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. Individuals are dropped from the sample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

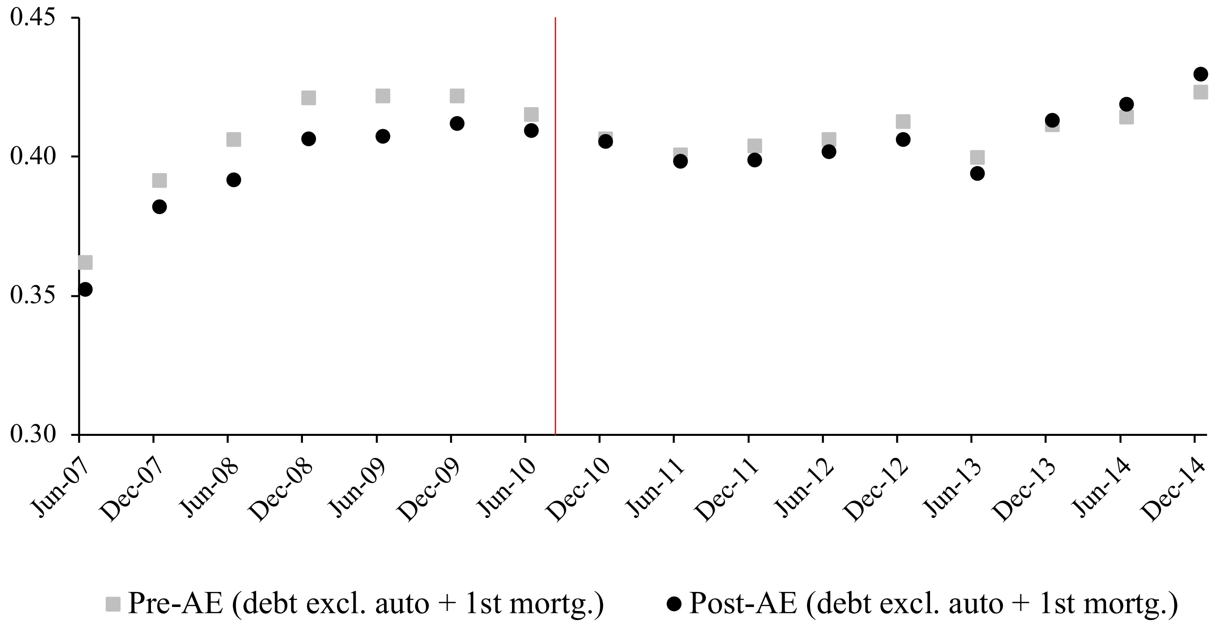

Figure 5. Average debt balance excluding auto debt and first mortgages normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic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

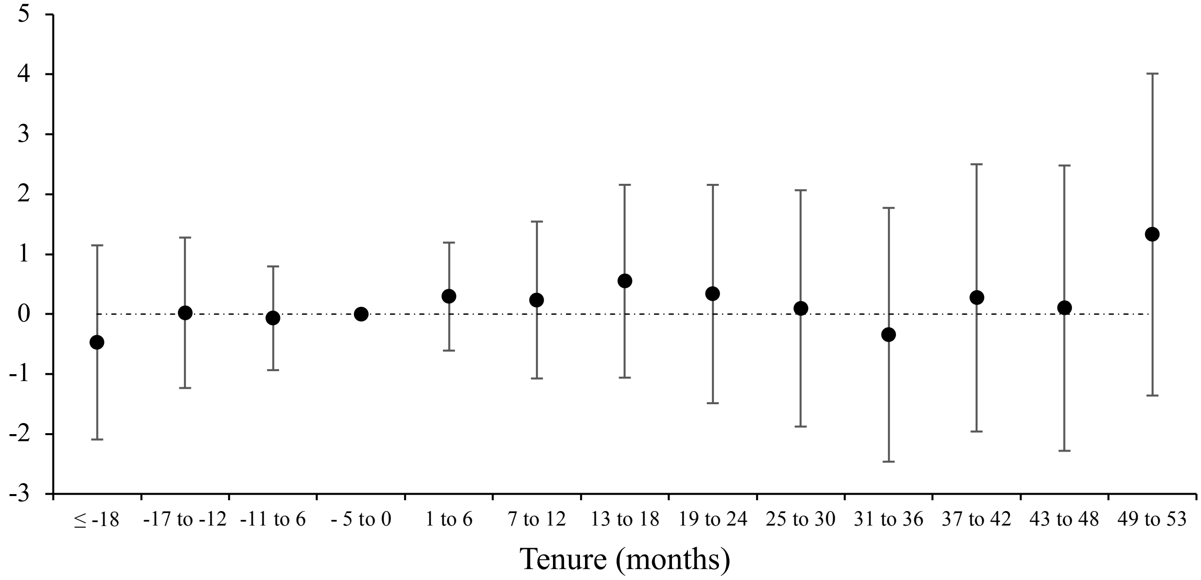

Figure 6. The effect of automatic enrollment on Vantage score. The estimates come from the regression in column 2 of Table 3. Point estimates and 95% confidence intervals are shown.

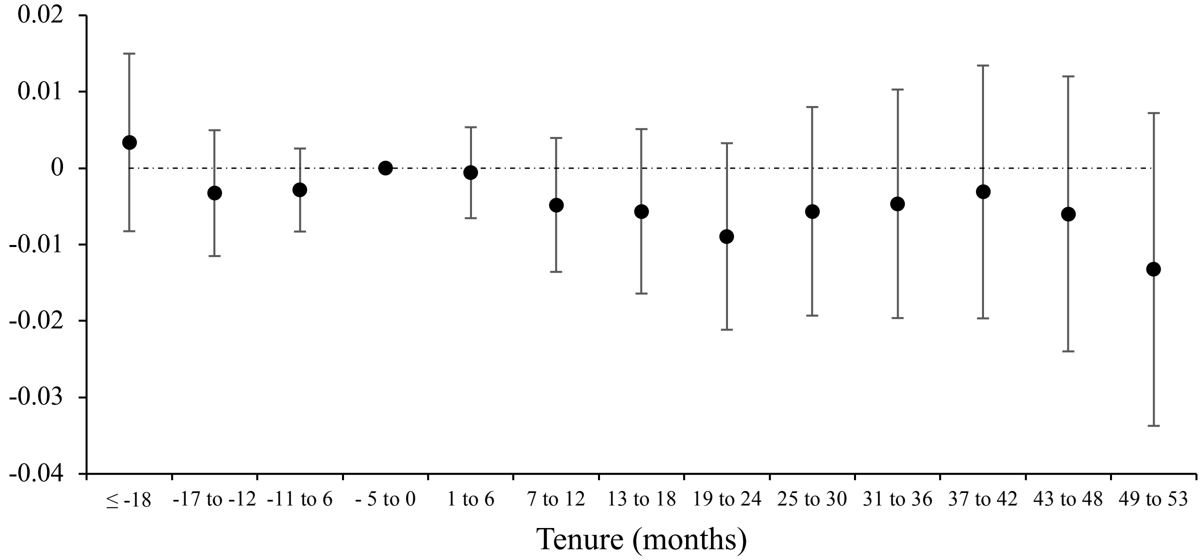

Figure 7. The effect of automatic enrollment on debt balance excluding first mortgages and auto loans normalized by annualized first-year pay. The estimates come from the regression in column 4 of Table 3. Point estimates and 95% confidence intervals are shown.