Borrowing to Save?
The Impact of Automatic Enrollment on Debt

John Beshears
Harvard University and NBER

James J. Choi
Yale University and NBER

David Laibson
Harvard University and NBER

Brigitte C. Madrian
Harvard University and NBER

William L. Skimmyhorn
United States Military Academy

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Abstract: How much of the retirement savings induced by automatic enrollment is offset by increased borrowing outside the retirement savings plan? We study a natural experiment created when the U.S. Army began automatically enrolling its newly hired civilian employees into the Thrift Savings Plan (TSP) at a default contribution rate of 3% of income. Four years after hire, automatic enrollment causes no significant change in debt excluding auto loans and first mortgages (point estimate = 0.9% of income, 95% confidence interval = [-0.9%, 2.7%]). Automatic enrollment does significantly increase auto loan balances by 2.0% of income and first mortgage balances by 7.4% of income. These secured liabilities have muted immediate effects on net worth because they are used to acquire assets, but their increase could signal that automatic enrollment previously decreased non-TSP assets. Larger secured loans could also decrease long-run net worth through greater depreciation and financing costs.

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Automatically enrolling employees in defined contribution retirement savings plans has become increasingly common. In the U.S., adoption of automatic enrollment has been encouraged by the Pension Protection Act of 2006 and evidence that it 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 (2016) reports that 58% of the 401(k) plans in its 2015 survey sample automatically enroll employees. The United Kingdom and New Zealand have also enacted automatic enrollment in their national pension schemes.

Automatic enrollment is intended to increase economic security in retirement. But its effectiveness at doing so depends upon how the contributions it induces are financed. The implicit presumption among those adopting automatic enrollment has been that the incremental contributions are financed by decreased consumption. However, there has been no evidence to date that allows us to rule out the possibility that the incremental contributions are funded by other asset accounts or debt, which would work against the purpose of automatic enrollment.

In this paper, we link individual employee payroll records to credit reports to identify the amount of crowding out that occurs on the borrowing margin. The setting we study is a natural experiment created by the introduction of automatic enrollment for civilian employees of the U.S. Army, which occurred simultaneously with the introduction of automatic enrollment for all other U.S. federal civil servants. Gelman et al. (2015) find that on the day before payday, the median federal government employee in their sample has liquid assets (checking plus savings account balances) that can cover only five days of spending.[1] Therefore, it seems likely that if automatic enrollment does not reduce consumption, much of the additional contributions it induces should result in increased borrowing in this population.

Prior to August 1, 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 that is similar to a 401(k) plan. Afterwards, newly hired employees were automatically enrolled in the TSP at a default contribution rate of 3% of their income unless they opted out. Importantly,

[1] Gelman et al. (2015) find that federal employees sharply reduced their debt repayments in response to the two-week delay of 40% of one paycheck caused by the 2013 federal government shutdown, even though it was known before the paycheck delay that any pay lost during the shutdown would be fully paid retroactively. Living paycheck to paycheck is not unusual; 46% of U.S. adults report that they could not come up with $400 to cover an emergency, or would have to borrow or sell something to do so (Board of Governors of the Federal Reserve System, 2016).

employees hired prior to August 1, 2010 were never subject to automatic enrollment. We therefore identify the effect of automatic enrollment by comparing the 32,073 employees hired in the year prior to the regime change to the 26,803 employees hired in the year after, controlling for time since hire. For credit outcomes, we also control for calendar time and individual fixed effects. Our results are similar but less precise if we use a regression discontinuity methodology to estimate the treatment effect by comparing those hired immediately before the automatic enrollment implementation date to those hired immediately after (see online appendix).

Consistent with prior evidence, we find that automatic enrollment at the low 3% default contribution rate chosen by the TSP (which is the most common non-zero default implemented in 401(k) plans; see Vanguard (2014)) has a modest positive effect on contributions to the TSP on average. At 43-48 months of tenure, automatic enrollment increases cumulative employer plus employee contributions since hire by 5.8% of first-year annualized salary.

To assess automatic enrollment's impact on net worth, we examine its effect on three measures of debt. Our first measure, which we call D1, encompasses all debt except first mortgages and auto debt. We exclude these two categories of debt because they are used to acquire durable assets; to a first approximation, increases in these liabilities have little immediate impact on net worth, as they are offset by a similar increase in the household's assets.[2] On the other hand, taking on more auto or first mortgage debt may be a signal that spending was higher in the *past*, so that net worth was lower at the time the loan was taken. In addition, to the extent that a larger auto loan indicates that one has bought more of an asset that depreciates quickly, higher auto debt presages future erosion of net worth. We therefore construct a second debt measure, D2, which adds auto debt to D1. Our third measure, D3, adds first mortgages to D2. Homes depreciate more slowly than vehicles on average and may even have expected appreciation (Case and Shiller, 1989), so taking out a larger mortgage to buy a more expensive home has a more ambiguous effect on future net worth than purchasing a more expensive vehicle. A larger mortgage could also have a positive net worth effect if it forces the household to save more than it otherwise would have because of the mortgage's scheduled acquisition of home equity.

[2] In a frictionless market, assets and liabilities increase by exactly the same amount, since assuming a liability whose present value is $1 allows one to obtain enough financing to buy an asset worth exactly $1.

Our results paint a nuanced picture of automatic enrollment's effects. We find no significant evidence that automatic enrollment increases debt excluding first mortgage and auto loans (D1), suggesting that there is relatively little definitive short-run crowd-out of net worth. At 43-48 months of tenure, the point estimate of the automatic enrollment effect as a fraction of first-year salary is 0.9%, and the 95% confidence interval is [-0.9%, 2.7%]. Nor do we find any meaningful effect on credit scores or financial distress measured by debt balances in third-party collections.

On the other hand, at the same horizon, automatic enrollment does significantly increase auto debt by 2.0% of first-year salary (95% confidence interval = [0.7%, 3.2%]), causing debt excluding first mortgages (D2) to increase by 2.8% (95% confidence interval = [0.5%, 5.1%]). This suggests there might be more meaningful net worth crowd-out that has either happened in the past through faster asset spend-down or will happen in the future through increased vehicle depreciation and interest payments. Nevertheless, the treatment effect on debt excluding first mortgages is significantly smaller than the 5.8% treatment effect on total TSP contributions, so by this measure, automatic enrollment still has a positive overall effect on net worth. But comparing the increase in debt excluding first mortgages to the cumulative increase in *employee* contributions caused by automatic enrollment, it seems that all of the additional employee contributions are eventually fully offset by additional non-first-mortgage debt; the increase in total TSP contributions exceeds the increase in debt excluding first mortgages only because of the employer matching contributions that automatic enrollment causes employees to earn.

Finally, automatic enrollment increases first mortgage balances by 7.4% of first-year salary (95% confidence interval = [-0.01%, 14.7%]) at four years of tenure; thus, total debt (D3) increases by 10.2% of first-year salary (95% confidence interval = [2.2%, 18.2%]). The confidence interval of the total debt effect is wide enough that we cannot reject equality with the treatment effect on total TSP contributions. However, as noted above, the effect of first mortgages on net worth is approximately zero in the short run and ambiguous in the long run. Because the point estimate of the increase in first mortgage debt exceeds the cumulative increase in employee TSP contributions, it seems likely that much of the increase in first mortgage debt is caused by automatically enrolled employees being able to obtain larger mortgages due to their extra TSP balances loosening down payment constraints.

Our paper is related to Blumenstock, Callen, and Ghani (2017), who run a field experiment on automatic enrollment in Afghanistan. They estimate positive effects of automatic enrollment on total savings that are mostly statistically insignificant, but because they rely on self-reports from only 470 employees, their standard errors are large. Our paper is also related to Chetty et al. (2014), who study how mandatory contributions to Danish retirement accounts affect total savings. They find that a 1% increase in these mandatory contributions results in a 0.8% increase in the total savings rate. Although mandatory contributions have similarities with automatic enrollment, these are two different kinds of policies, as demonstrated by the difference in employees' responses to them. Chetty et al. (2014) show that when employees move to an employer whose mandatory contribution rate is 1% higher, the employee's total savings rate response is virtually unchanged over the first ten years after the job change. In contrast, Choi et al. (2004) show 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. Our paper is also related to the long literature on whether the availability of 401(k) plans on an opt-in basis increases total savings (Poterba, Venti, and Wise, 1995, 1996; Venti and Wise, 1997; Engen, Gale, and Scholz, 1994, 1996; Engen and Gale, 2000; Benjamin, 2003; Gelber, 2011).

The remainder of the paper proceeds as follows. Section I summarizes the relevant institutional details of the TSP and the policy change we exploit. Section II describes our data, and Section III compares the two hire cohorts that are the focus of our analysis. Section IV discusses our empirical findings on automatic enrollment's effect on TSP contributions. Section V describes a conceptual framework for thinking about what our various debt measures imply about net worth, and our empirical findings on automatic enrollment's effect on debt. Section VI discusses empirical results on subpopulations that previous literature suggests would have especially large automatic enrollment effects on contributions. Section VII concludes. An online appendix presents an alternative estimation using a regression discontinuity design and some supplementary tables.[3]

[3] 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. As in the body of this paper, we link credit bureau records to 401(k) records in each of the firms, which separately introduced automatic enrollment from 2006 to 2011. 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. In one larger firm, we find that automatic enrollment causes a 3.8 point decline in Vantage score (95% confidence interval = [-8.7, 1.2]), or less than 0.04 standard deviations.

I. Thrift Savings Plan institutional details and the natural experiment

The institutional details of the Thrift Savings Plan are similar to many private-sector 401(k) plans. Contributions to the TSP are made on each payday. Employee contributions are made via payroll deduction. Civilian employees receive matching contributions from the government: the first 3% of their own income contributed garners a dollar-for-dollar match, and the next 2% of income contributed is matched at a 50% rate. All civilian employee accounts also receive a government contribution called the Agency Automatic (1%) Contribution equal to 1% of their income, regardless of their own contribution rate. Matching contributions are immediately vested, while Agency Automatic (1%) Contributions vest after three years of service or upon the employee's death if she is still employed by the government. The IRS imposes limits on the total dollars 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 various 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 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 any number of withdrawals at any age if financial hardship was certified.[4] Hardship withdrawals required the

[4] 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)

employee to not contribute to the TSP for the following six months, 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, or a life annuity.

Beginning on August 1, 2010, the U.S. federal government implemented automatic enrollment for all U.S. federal employees covered by the Federal Employees' Retirement System (FERS), including those in the Army. The Army is the second-largest Cabinet-level agency in the federal government, with over 215,000 civilian employees throughout our sample period (United States Office of Personnel Management, 2016). Before this change, all civilian Army 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 into 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 are 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.[5] Second, furloughs in the federal government reduced pay in 2013. For a period of six weeks beginning on July 8, 2013, most Army civilian employees received one less day of pay per week due to Department of Defense furloughs. Some employees—referred to as excepted employees—whose work was deemed essential continued to work on and receive pay for all regular workdays during this period. To account for the effect of

[5] 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.

furloughs, we make an adjustment to TSP contributions in July and August 2013, as detailed in Section IV. On October 1, 2013, the federal government shut down and furloughed all of its civilian employees, although excepted employees were required to continue working without pay. On October 5, the Pentagon recalled most of its employees from furlough, and Congress passed a bill guaranteeing that all employees would be paid wages lost due to the shutdown once it ended. The shutdown ended on October 16. Because the shutdown began in the middle of the first pay period of October and ended in the middle of the second pay period of October, no regularly scheduled payday passed without paychecks being issued to all employees. However, the first paycheck in October was abnormally low and the second paycheck was abnormally high. Gelman et al. (2015) 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 the monthly frequency and biannual credit reports, so we make no adjustment for the government shutdown.

II. Data description

To measure savings in the TSP, we use employee-level administrative payroll data from the Department of Defense. The payroll data consist of monthly cross-sections from January 2007 to December 2015 of all Army employees hired or re-hired during that period of time. We observe the dollar amounts of employee and employer TSP contributions for each month in this database. We link these records to information from Army personnel data on personal characteristics (year of birth, gender, race, education level, and any academic discipline in which that education specialized) and employment information (most recent hire date, date the employee first became TSP-eligible, creditable service time as a federal government employee, and annualized pay rate).[6] For the purposes of determining whether an employee was subject to automatic enrollment, we use the FERS eligibility date[7], which almost always corresponds to the employee's hire date; for simplicity, we will hereafter refer to FERS eligibility dates as "hire dates." When an employee's monthly payroll records don't begin until his second calendar month of employment (which occurs for 29% of employees) or third calendar month of

[6] The payroll and personnel data were merged by the Office of Economic and Manpower Analysis (OEMA), which then completed a matching and de-identification for the credit data.

[7] If an employee converts from being ineligible for FERS to being eligible during the automatic enrollment regime, she would by default be enrolled in TSP upon converting.

employment (which occurs for 0.4% of employees), we assume he did not contribute to the TSP in the missing month(s).[8] We drop the 0.8% of the sample that does not have a payroll record by the third month of their tenure because of concerns that their payroll data are not reliable. Beyond an employee's second month of tenure, if payroll data are missing for a month, we assume her contribution rate in the missing month was the same as in the closest preceding non-missing month.[9]

We observe only contribution flows into the TSP; we do not observe balances or the funds in which balances are invested. Furthermore, we do not observe in-service 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 in-service 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 automatic enrollment cohort. At the end of Section IV, we will show that in-service withdrawals are unlikely to materially affect our results.

For the credit analysis, we use matched and de-identified individual-level credit reports from a national credit bureau.[10] The credit data consist of biannual month-end cross-sections from June 2007 to December 2014. In each cross-section we observe debt balances[11], number of accounts, and various measures of distress (e.g., late payments, delinquent accounts, open liens, bankruptcy proceedings, etc.). The debt measures are broken up by source (e.g., mortgage, bankcard, student loans, auto loans, etc.). We also observe Vantage scores—an estimate of creditworthiness calculated by the credit bureaus that ranges from 300 (least creditworthy) to 850

[8] We suspect that employees who have no payroll record in their first calendar month of employment tend to be those who were hired later in the month, since under opt-in enrollment, their TSP participation rate at the end of the second and third calendar months of employment is lower than that of employees who have a payroll record in their first calendar month of employment, but then equalizes afterwards. However, we cannot directly test this hypothesis because our hire date variable gives no intra-month information.

[9] Only 1.4% 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.

[10] 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.

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

(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.[12]

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

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

Table 1 compares the characteristics of these two cohorts. The post-AE cohort is somewhat lower-paid at hire, and this average gap increases to 2.0% of the pre-AE cohort average salary when annualized starting salary is deflated by the 2.0% average federal pay increase between 2010 and 2011. The post-AE cohort is also older, less likely to be missing race information, less educated, more likely to be in an administrative or clerical position, and less likely to be in a blue collar, professional, or technical position. Although these differences are statistically significant due to the large sample size, their economic magnitudes tend to be small. We will control for differences via regression. There is no significant difference between the cohorts in the probability of having a credit report in the six months prior to hire and the average Vantage score conditional on having a score in the six months prior to hire.

IV. Effect of automatic enrollment on TSP contributions

In keeping with the previous literature on automatic enrollment, we estimate the effect of automatic enrollment by comparing the pre-AE cohort to the post-AE cohort at equivalent levels of job tenure. Because our payroll data are monthly, we can compute cumulative contributions for every employee at every tenure month during our sample period. However, to maintain comparability with the credit analysis, where we can only observe outcomes in June and

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

December, we will compute cumulative contributions at n months of tenure using only employees hired n months before a June or December. For example, cumulative contributions at five months of tenure for the post-AE cohort are computed using only August 2010 hires (cumulating their contributions from August 2010 through December 2010) and February 2011 hires (cumulating their contributions from February 2011 through June 2011).

We make one adjustment to maximize comparability of cumulative contributions across cohorts at a given tenure level. Sometimes, when a pre-AE hire has achieved n months of tenure, he has experienced m paydays in total (and hence has had m TSP contribution opportunities), while a corresponding post-AE hire with n months of tenure has experienced $m' \neq m$ paydays due to where calendar month boundaries fall with respect to the biweekly pay schedule.[13] Even within a 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.[14] 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 were experienced by somebody hired at the beginning of the applicable calendar months and employed continuously until the end of the nth calendar month of tenure. We scale the last month's contributions of each individual to approximate how much that individual would have contributed by month n had she experienced the benchmark number of paydays.[15]

As explained in Section I, mandatory federal government furloughs reduced most employees' pay by 20% for three-quarters of the weeks in the July and August 2013 pay periods. Employees subject to furloughs who did not adjust their contribution rate 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 1.15.[16] This adjustment does not meaningfully change our results

[13] We assume that if the employee was missing a payroll record in the first month or first two months of her tenure, then she did not have any paydays in those months.
[14] Although we do not have intra-month information on hire or separation dates, we can infer the number of paychecks received by comparing salary paid in that month to the annualized pay rate variable.
[15] We do not make a payday adjustment in our debt analysis.
[16] 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.

because our main outcome of interest is cumulative contributions since hire, so a small adjustment to two months of contributions makes little difference.

Figure 1 plots the average cumulative employer plus employee TSP contributions to annualized first-year pay ratio 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 the same amount of creditable service are dropped from the sample from their initial departure date onwards.[17] We see that automatic enrollment has an average effect in the TSP similar to that previously documented in 401(k) plans.[18] With a low default contribution rate of 3% of income, automatic enrollment raises average cumulative TSP contributions to first-year annualized salary modestly. Averaging over six-month tenure windows, automatic enrollment raises cumulative contributions by 1.8%, 3.3%, 4.3%, and 5.3% of first-year salary at 7-12, 19-24, 31-36, and 43-48 months of tenure, respectively.

To compute regression-adjusted estimates of automatic enrollment's effect on cumulative contributions, we stack all observations through tenure 53 into a single regression and estimate the equation

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

where $y_{i\tau}$ is the outcome variable for person *i* at tenure τ, $I(\tau \in T_s)$ 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, and race), and $PostAE_i$ is an indicator variable for being in the post-AE cohort.[19] The coefficient γ_s represents the treatment effect of automatic enrollment for tenure bucket T_s. These are what are shown in Figure 2 and the first column of Table 2. We find results that are slightly larger than those computed from the raw differences: automatic enrollment raises

[17] Attrition across the two cohorts is similar. 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. Appendix Tables 4-5 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 (except that there is no significant effect on auto debt, although the 95% confidence intervals include the main analysis's point estimates).

[18] The apparent seasonality in the series that occurs at a six-month frequency arises because a given calendar month's hires appear in the graph only once every six months. That is, the seasonality reflects differences across calendar-month hire cohorts. The seasonality disappears if we include every tenure month of every calendar-month hire cohort in the graph.

[19] The education level, job type, college, major, and race categories are those shown in Table 1. We combine 13 smaller states and territories into a single dummy because only a small number of people are located there.

cumulative contributions by 2.2%, 3.9%, 5.1%, and 5.8% 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) above 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 49-53 month estimate, since they are never observed at those tenures in our data.)

The second column of Table 2 shows the regression-adjusted estimate of automatic enrollment's effect on cumulative employee contributions, which exclude the employer match and Agency Automatic (1%) Contributions. As expected given the TSP match structure of 100% on the first 3% of income contributed and 50% on the next 2% of income contributed, the effects on employee contributions are about half of those on total contributions. However, the effect on employee contributions levels off at about 2.5% of first-year salary after three years of tenure, whereas the effect on total contributions continues to grow through the end of our sample period. This divergence can be understood by looking at Figure 3, which shows the 10th, 25th, 50th, and 90th percentiles of the cumulative employee TSP contributions to first-year pay ratio at each tenure level. From the slopes of the series, we see that automatic enrollment continues to raise contribution rates at the 10th percentile through the end of the sample period, whereas the contribution rate at the 25th percentile equalizes across cohorts between two and three years of tenure. There is never any meaningful difference between the cohorts at the median, and if anything, the post-AE cohort contribution rate is slightly lower than the pre-AE cohort contribution rate at the 90th percentile at higher tenures. So at higher tenures, automatic enrollment increases contribution rates among those who have a high marginal match rate and decreases contribution rates among those who have no marginal match. The net result is to increase *total* contribution flows at high tenures even though the effect on *employee* contribution rates disappears.

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 because such withdrawals must be at least $1,000 and require the employee to stop contributing to the TSP for at least six months afterwards. We therefore assume that an employee has taken a hardship withdrawal on date *t* equal to 100% of her employee contributions to date if she was contributing to the TSP on date *t*, has at least $1,000 of cumulative employee contributions as of *t*, and stops contributing for at least six months after *t*.[20] Hardship withdrawals are rare, and subtracting estimated hardship withdrawals from contributions causes the estimated automatic enrollment effect on TSP balances to fall by only 0.1% of first-year income.

Recall that employees over the age of 59½ may also take one age-based withdrawal of at least $1,000 or 100% of their vested balance (whichever is greater). We have separate data on the number of withdrawals taken from 2016 through the first half of 2017. The fraction of 60+ year old employees who took an age-based withdrawal during this period is 3.83% in the pre-AE cohort and 3.79% in the post-AE cohort. We can also compare the two cohorts at equivalent levels of tenure. The fraction of 60+ year old pre-AE employees who took an age-based withdrawal from January to June 2016 is 1.15%, and the equivalent for post-AE employees from January to June 2017 is 0.94%. In short, there is little indication that there are meaningful differences in withdrawal behavior across the two cohorts.

V. Effect of automatic enrollment on credit outcomes

A. Debt measures

To get a more comprehensive picture of how automatic enrollment affects household net worth, we examine three main measures of debt: debt excluding first mortgages and auto debt (D1), debt including auto balances but excluding first mortgage balances (D2), and total debt (D3). D1 consists of debt that is not explicitly associated with the purchase of a durable asset, and hence most likely to be used to finance non-durable and service expenditures that contemporaneously decrease net worth. D2 and D3 successively add components of debt that are increasingly less likely to be associated with net worth erosion. All three debt measures include non-derogatory balances on installment and revolving loans (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) held by creditors who report to the credit bureau. Creditors such as payday lenders that do not report to the credit bureau are excluded from our debt measure. We also include derogatory debt that has been passed to an external collection agency.[21]

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[20] 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.

[21] Our delinquent debt measure excludes charge-off accounts (these are accounts where the original creditor has given up trying to collect on the debt) that have not been passed to an external collection agency, debts included in

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

$$\Delta Durable\ asset + \Delta Financial\ assets = \Delta Secured\ debt. \tag{2}$$

This equation says that the present value of the secured debt repayments equals the value of the durable asset minus the value of any financial assets that were spent down to help finance the durable purchase. Taking out a larger secured loan indicates the purchase of a more valuable asset and/or a smaller spend-down of financial assets. In either scenario, the contemporaneous impact on net worth—the increase in assets minus the increase in liabilities—is zero.

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

Conversely, an automatically enrolled household might spend down fewer financial assets to acquire a durable because it has less liquid assets available. Even though the borrow-and-buy transaction itself still has no effect on net worth in this case, the larger loan signals that automatic enrollment caused the household to draw down its non-TSP financial assets in the *past*. Hence, the portion of the loan increase that is attributable to non-TSP asset spenddown should be subtracted from TSP assets when calculating the net worth effect of automatic enrollment. Because most federal employees have minimal liquid assets outside the TSP (Gelman et al., 2015) and automatic enrollment affects the left tail of the savings distribution most powerfully, this channel may be relatively small.

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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.

Taking out a larger secured loan has potential implications for *future* net worth. Let W_t be wealth at time t, P_t be the price of asset a at time t, and r be the interest rate. In a frictionless market, the following two strategies for getting use of a for T periods have an identical effect on wealth T periods later: renting a for T periods, or buying it and then selling it T periods later. Therefore, if a secured loan is used to purchase an asset but the household otherwise would have rented another asset that has the same rental value as the purchased asset, then there is no effect on the path of future net worth. Expressing the $T = 1$ version of the above relationship, we get

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

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

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

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

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

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

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

The price growth rate g is highly negative for vehicles; the average new car loses about 60% of its value over the first five years of its life.[22] In contrast, the Bureau of Economic

[22] https://www.carfax.com/blog/car-depreciation/ (accessed November 24, 2017). Arguably, a good deal of the depreciation occurs the moment the vehicle is driven off the dealer's lot. However, there is a difference between the "hold to maturity" value of the car—the present discounted value of the service flows it provides the owner over the course of its entire useful life—and the liquidation value of the car, which is depressed by adverse selection in the

Analysis estimates that a new one-to-four unit residential structure loses only 6% of its value to depreciation over the first five years of its life, and in many markets, homes experience price appreciation.[23] Therefore, a debt-financed purchase of a more expensive car is likely to result in future net worth erosion, but a debt-financed purchase of a more expensive house has ambiguous effects.

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

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

B. Econometric methodology

Although it is tempting to analyze credit outcomes using the same econometric framework that we used to analyze its effect on TSP contributions—simply comparing cohorts to each other at equivalent levels of tenure—Figures 4 through 7 show that such an analysis would be confounded by calendar time effects that shift credit outcomes in both cohorts roughly additively. In contrast, Figure 8 indicates that such calendar time effects are absent from cumulative TSP contributions. Additionally, we see in Figures 4 through 7 that at a given calendar date before either cohort was hired, the post-AE cohort's credit variables are often at a

used car market. The "hold to maturity" value probably does not drop much immediately after purchase, whereas the liquidation value does.

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

different level than the pre-AE cohort's, which is at least partially due to the post-AE cohort being younger than the pre-AE cohort at each calendar date.

To estimate automatic enrollment effects while controlling for calendar time effects and fixed differences across cohorts, we run the regression

$$y_{i\tau t} = \alpha_i + \eta_t + \Sigma_\tau[I_i(t \in \tau)(\beta_\tau + \gamma_\tau PostAE_i)] + \epsilon_{it}, \tag{6}$$

where $y_{i\tau t}$ is the credit outcome for employee *i* who is in tenure bucket τ at calendar date *t*, α_i is the employee fixed effect, η_t is the calendar time effect, $I_i(t \in \tau)$ is an indicator variable for calendar date *t* corresponding to when employee *i*'s tenure is in tenure bucket τ, 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.[24] So the tenure buckets included in the summation are {≤ -18, -17 to -12, -11 to -6, 1 to 6, 7 to 12, …, 43 to 48, 49 to 53}. The coefficient β_τ represents how much the credit outcome differs from its value at tenures -5 to 0 due to achieving a tenure level in bucket τ under an opt-in TSP enrollment regime. The main coefficient of interest γ_τ is the incremental treatment effect of being in tenure bucket τ under an automatic enrollment regime instead of an opt-in enrollment regime.

It is well-known that even with perfect panel data, calendar time, tenure, and cohort effects cannot be separately identified without additional identifying assumptions because the three variables are collinear (e.g., Ameriks and Zeldes, 2004). Our identifying assumption is that tenure effects are constant for all tenures less than or equal to -18 months. To see how this enables us to estimate all our coefficients, take the expectation of first differences for two pre-AE individuals who are one tenure bucket apart at date *t*:

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

$$E\left(\Delta y_{i(\tau-1)t}\right) = (\beta_{\tau-1} - \beta_{\tau-2}) + (\eta_t - \eta_{t-1}). \tag{8}$$

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

$$E(\Delta y_{i\tau t}) - E\left(\Delta y_{i(\tau-1)t}\right) = (\beta_\tau - \beta_{\tau-1}) - (\beta_{\tau-1} - \beta_{\tau-2}). \tag{9}$$

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

For τ sufficiently negative, $\beta_{\tau-1} - \beta_{\tau-2} = 0$, allowing us to identify $\beta_{\tau} - \beta_{\tau-1}$. Normalizing the tenure effect at a certain tenure bucket to be zero, we obtain estimates for every other β. Analogous reasoning shows how the post-AE cohort's γ coefficients are identified as well.

C. Empirical results

Figure 9 and the third column of Table 2 show the treatment effects of automatic enrollment on D1 at each tenure bucket. Reassuringly, there is no significant difference between the pre- and post-AE cohorts before hire, when neither of them should have experienced differing treatments on average. The pattern of no significant difference continues after hire, all the way out to 49-53 months of tenure. At 43-48 months of tenure, the point estimate of the automatic enrollment effect is 0.9% of first-year income, with a 95% confidence interval of [-0.9%, 2.7%]. The point estimates at lower tenures never exceed 0.9% of first-year income.

We can also decompose D1 into seven categories: home equity lines of credit (HELOCs), non-HELOC revolving debt, other installment debt, second mortgages, student loans, accounts in external collections, and residual debt that does not belong to the other categories. Non-HELOC revolving debt consists of credit cards and personal lines of credit. Other installment debt consists almost entirely of non-mortgage/non-student/non-auto personal installment loans (both secured and unsecured) from personal finance companies, banks, and credit unions, but it also includes retail installment loans from retailers, which are usually used to finance a major purchase such as an appliance or furniture.[25] Examples of debt that falls in the residual category are charge cards such as American Express cards that must be paid in full at the end of each month.

Table 3 shows the automatic enrollment effect on each of the above components of debt. Few of the coefficients are statistically significant, and the significant coefficients are often negative. The magnitude of the positive and significant coefficients is small—only 0.1% to 0.2% of first-year income for residual debt. Note that there are 84 (non-independent) hypothesis tests shown in the table, so we would naturally expect some of these coefficients to be statistically significant at conventional levels due to Type I error. We see no increase in debt in third-party

[25] There is an argument that we should exclude retail installment loans from D1 because they, like auto loans, are being used to purchase a durable good. However, furniture and appliances are much harder to sell on the secondary market than cars, so the ability to use these assets to generate cash is quite limited.

collections, suggesting that automatic enrollment has no impact on the probability of financial distress.

Figure 10 and the fourth column of Table 2 show automatic enrollment's effect on Vantage scores, conditional on having a Vantage score. Consistent with what we saw with debt in third-party collections, we find no statistically significant effects on credit scores, and the point estimates lie between -0.1 and 1.4 points across the tenure spectrum. To calibrate the economic significance of the results, note that the full sample's Vantage score standard deviation at baseline is 95. Therefore, the point estimates indicate an effect that is no more than 0.01 standard deviations in magnitude, with the lower end of the 95% confidence intervals reaching only -0.02 standard deviations. In sum, there is no indication that automatic enrollment creates any meaningful decrease in creditworthiness.

In contrast to its effect on D1, automatic enrollment does increase auto and first-mortgage debt. Figures 11-12 and the last two columns of Table 2 show significant increases in these debt categories at longer tenures, and the point estimates indicate steady post-hire increases in these loan balances even before they reach statistical significance. Auto debt becomes statistically significant at 31-36 months of tenure, and by 43-48 months of tenure— our preferred long-horizon tenure bucket for reasons described in Section IV—automatic enrollment has induced a 2.0% of first-year income increase in auto debt. As discussed in Section V.A, much of the increase in auto debt we estimate likely represents net worth reductions that have already occurred or will occur in the future.

The first mortgage effect, which is estimated relatively imprecisely because of the large variance in mortgage balances, does not become statistically significant until 49-53 months of tenure, at which point we estimate that automatic enrollment increases first mortgage balances by 9.4% of first-year income. At 43-48 months of tenure, the point estimate of the automatic enrollment effect on first mortgage debt is 7.4% of first-year income, which barely misses significance at the 5% level. Section V.A. catalogued why the effect of higher first mortgage balances have an ambiguous effect on net worth.

Table 4 adds the individual debt effects together to produce automatic enrollment effects on D2 and D3. At 43-48 months of tenure, automatic enrollment significantly increases D2 by 2.8% of first-year income (95% confidence interval = [0.5%, 5.1%]) and D3 by 10.2% of first-year income (95% confidence interval = [2.2%, 18.2%]). To gauge the net impact of automatic enrollment, we calculate the difference between the treatment effect on cumulative total TSP contributions and the treatment effect on D1, D2, or D3.[26] We call these differences the "net wealth" effects as a shorthand, or NW1, NW2, and NW3, respectively. Note that these measures miss changes in assets outside the TSP that would be necessary to measure true net wealth changes. In addition, since we do not have information on employees' current and future marginal tax rates, the measures do not adjust for the fact that TSP contributions were made with before-tax dollars (at least until Roth contributions became available in July 2012) and debts must be paid mostly with after-tax dollars.

We see in the fourth column of Table 4 that the automatic enrollment effect on NW1 is positive and highly significant at all tenure levels. Automatic enrollment raises NW1 by 2.4%, 4.2%, 4.7%, and 4.9% of first-year salary at 7-12, 19-24, 31-36, and 43-48 months of tenure, respectively. Put differently, D1 crowded out at most 16% of the automatic enrollment effect on cumulative total contributions in any of these tenure buckets. The increase in debt as a percent of cumulative *employee* contributions, excluding employer contributions, is at most 35%. Because before-tax TSP contributions (the predominant type of contribution) are deductible from taxable income in the contribution year, if marginal tax rates were constant and debt accrued no interest, this percentage divided by (1 – marginal tax rate) would indicate what fraction of the drop in take-home pay induced by automatic enrollment was financed by debt.

The effect of automatic enrollment on cumulative contributions minus D2 is still positive and significant. The fifth column of Table 4 shows that the NW2 effect is somewhat smaller than the NW1 effect, reflecting the positive effect automatic enrollment has on auto debt. At 43-48 months, automatic enrollment raises NW2 by 2.9% of first-year income. Comparing the path of the cumulative employee contributions effect over time in Table 2 to the path of the D2 effect in Table 4, we see that through the first two to three years of tenure, the increase in D2 substantially lags the increase in cumulative employee contributions. But by 43-48 months of tenure, approximately all of the cumulative decrease in take-home pay induced by automatic enrollment

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

has been offset by increased D2. Therefore, the positive effect of automatic enrollment on NW2 at 43-48 months is coming entirely through the employer match that automatic enrollment caused the employee to earn.

The final column of Table 4 shows that automatic enrollment never has a significant effect on NW3, and the point estimates become relatively large and negative at later tenures. At nearly all tenures, the increase in D3 exceeds the increase in cumulative employee TSP contributions shown in Table 2, although the confidence interval on the former effect is wide enough that equality between the two effects cannot be rejected. If we take the point estimates seriously, it seems implausible that a decrease in cumulative take-home pay of 2.6% of first-year income over four years would lead to a decrease in net worth equal to 4.4% of first-year income. More likely, much of the increase in D3 is caused by some of the additional TSP balances being used to secure a larger mortgage by increasing a home down payment.

In an untabulated linear probability regression, we estimate positive but insignificant effects of automatic enrollment on the probability of having a first mortgage, with a point estimate at 43-48 months of tenure of 0.9 percentage points. Similarly, we find that automatic enrollment causes a 1.3 percentage point increase in the probability of having auto debt at 43-48 months, but the effect is not significant.

VI. Automatic enrollment effects on subpopulations

We saw in Figure 3 that automatic enrollment affects only the left tail of TSP contributions. In this subsection, we analyze how automatic enrollment affects net wealth accumulation in various 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 the low-income, the young, blacks, and Hispanics. Therefore, we estimate treatment effects for these groups in our sample, as well as for those who have only a high school education and those whose credit score at baseline is below 620 (approximately the bottom quintile of our sample).

For brevity, we focus on effects at 43-48 months of tenure. The first row in Table 5 shows that except for the young (those under age 30 at hire), automatic enrollment appears to have a larger treatment effect on cumulative total TSP contributions at 43-48 months of tenure than the sample-wide average of 5.8% of first-year salary. The point estimates range from 6.9% for high school graduates to 9.4% for those with a starting annualized salary less than $34,000.

In the fifth row, we see that automatic enrollment does not have a statistically significant effect on D1 for any group. However, our estimates are imprecise, and two of the positive point estimates—4.5% for Hispanics and 4.8% for those with low credit scores—are large. The automatic enrollment effect on NW1 is positive for all groups and significant except for Hispanics and those with low credit scores. The significant effects have point estimates that are larger than the 4.9% effect on NW1 found for the entire sample, but their 95% confidence intervals all contain 4.9%.

Lack of statistical power also plagues our NW2 and NW3 estimates. D2 increases significantly for high school graduates and those with low credit scores, and D3 increases significantly for high school graduates. The only statistically significant automatic enrollment effect on NW2 is for blacks and those under age 30, whose NW2s increase by 9.9% and 5.0% of first-year income, respectively. But every group's 95% confidence interval includes the 2.9% effect on NW2 for the entire sample. The NW3 effects are estimated with even more noise and are largely uninformative. More precisely estimated are effects on Vantage scores, which are insignificant and small in magnitude for all groups.

VII. Conclusion

Automatic enrollment in the TSP at a 3% of income default contribution rate is extremely successful at increasing contributions to the TSP at the left tail of the distribution while leaving the middle and right of the distribution unchanged. At 43-48 months of tenure, this policy raises cumulative contributions to the TSP by 5.8% of first-year annualized salary on average. We find that little of this accumulation is offset by increased debt excluding first mortgages and auto debt—the type of debt most likely to be associated with net worth reductions—and there is no impact on credit scores or debt in third-party collections. However, we also estimate that automatic enrollment raises auto and first mortgage debt at three to four years of tenure.

Taking on secured debt such as an auto loan to purchase an asset does not immediately affect net worth to a first approximation, since assets and liabilities increase by a similar amount. But the fact that secured debt balances increase might signal that automatic enrollment caused liquid assets outside the TSP to be spent down *before* the loan was taken out, creating the need for a larger loan. If instead, a larger loan was taken out in order to buy a more expensive car, the loan portends higher *future* net worth erosion. It seems likely that some combination of these two scenarios is true: (1) 2.8 percentage points of automatic enrollment's 5.8% TSP contribution effect has already been offset by non-TSP asset spend-down at four years of tenure, or (2) the future net worth offset due to depreciation and increased interest payments will be a large fraction of 2.8 percentage points. In fact, the only reason why the long-run effect on asset accumulation exceeds the long-run effect on non-first-mortgage debt is the employer match that automatic enrollment causes employees to earn; every dollar of employee contributions that automatic enrollment induces is eventually fully offset by an additional dollar of non-first-mortgage debt.

The increase in first mortgage debt may be more benign. Like auto debt, taking out a mortgage to buy a house has approximately no immediate impact on net worth, but a larger first mortgage may indicate that automatic enrollment induced past spend-down of non-TSP assets. The fact that we estimate (relatively imprecisely) that automatic enrollment created a larger increase in first mortgage debt than its cumulative impact on TSP employee contributions suggests that much of the increase in first mortgage debt is due to the extra TSP balances generated by automatic enrollment relaxing financial constraints, allowing employees to make a bigger down payment and thus obtain a larger mortgage. In the long run, a larger mortgage may actually increase net worth because homes depreciate relatively slowly (and often appreciate) and the mortgage payment schedule forces the borrower to build home equity at a pace that may exceed her counterfactual savings rate.

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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	$56,962	$55,825	-$1,137	0.000
Avg. age at hire	39.7	39.9	0.2	0.012
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%	0.227
Unknown education	1.0%	1.0%	0.0%	0.979
STEM college major	13.3%	10.5%	-2.8%	0.000
Business college major	12.8%	11.4%	-1.4%	0.000
Other college major	19.8%	19.9%	0.1%	0.705
Administrative position	31.0%	31.6%	0.7%	0.090
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
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	0.245
N	32,073	26,803		

Table 2. Automatic enrollment effects on contributions and debt components

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

	Cumulative total TSP contributions	Cumulative employee TSP contributions	Debt excluding auto, first mortgage (D1)	Vantage credit score	Auto debt	First mortgage debt
Tenure ≤ -18	--	--	0.002	-0.5	0.000	0.008
			(0.006)	(0.8)	(0.003)	(0.020)
Tenure -17 to -12	--	--	-0.005	-0.1	-0.001	-0.016
			(0.004)	(0.6)	(0.003)	(0.016)
Tenure -11 to -6	--	--	-0.005	-0.1	0.000	-0.016
			(0.003)	(0.4)	(0.002)	(0.011)
Tenure 1 to 6	0.010**	0.005**	0.001	0.2	0.001	0.022
	(0.000)	(0.000)	(0.003)	(0.5)	(0.002)	(0.012)
Tenure 7 to 12	0.022**	0.011**	-0.002	0.3	0.002	0.014
	(0.000)	(0.000)	(0.004)	(0.7)	(0.003)	(0.018)
Tenure 13 to 18	0.031**	0.015**	-0.002	0.6	0.006	0.027
	(0.001)	(0.001)	(0.005)	(0.8)	(0.004)	(0.023)
Tenure 19 to 24	0.039**	0.020**	-0.004	0.4	0.006	0.015
	(0.001)	(0.001)	(0.006)	(0.9)	(0.004)	(0.026)
Tenure 25 to 30	0.046**	0.023**	0.001	0.1	0.010	0.029
	(0.001)	(0.001)	(0.007)	(1.0)	(0.005)	(0.029)
Tenure 31 to 36	0.051**	0.025**	0.004	-0.1	0.015**	0.050
	(0.002)	(0.001)	(0.008)	(1.1)	(0.005)	(0.032)
Tenure 37 to 42	0.056**	0.027**	0.007	0.5	0.016**	0.054
	(0.002)	(0.002)	(0.008)	(1.1)	(0.006)	(0.035)
Tenure 43 to 48	0.058**	0.026**	0.009	0.2	0.020**	0.074
	(0.002)	(0.002)	(0.009)	(1.2)	(0.006)	(0.038)
Tenure 49 to 53	0.060**	0.026**	0.003	1.4	0.017*	0.095*
	(0.003)	(0.003)	(0.010)	(1.4)	(0.007)	(0.043)
N	398,393	398,393	809,414	670,254	809,414	809,414

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

Table 3. Automatic enrollment effects on D1 subcomponents

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

	HELOC revolving	Non-HELOC revolving	Other installment loans	Second mortgages	Student loans	External collections	Residual debt
Tenure ≤ -18	0.005 (0.002)	0.001 (0.002)	-0.003 (0.003)	0.004 (0.003)	-0.005 (0.003)	-0.001 (0.001)	0.001 (0.001)
Tenure -17 to -12	0.001 (0.002)	0.000 (0.001)	-0.003 (0.002)	0.000 (0.002)	-0.003 (0.002)	-0.001 (0.001)	0.000 (0.000)
Tenure -11 to -6	-0.001 (0.001)	0.001 (0.001)	-0.003* (0.001)	0.000 (0.001)	-0.001 (0.001)	-0.001* (0.000)	0.000 (0.000)
Tenure 1 to 6	0.000 (0.001)	0.000 (0.001)	-0.003* (0.002)	0.002 (0.001)	0.002 (0.001)	0.000 (0.000)	0.000 (0.000)
Tenure 7 to 12	0.001 (0.002)	-0.001 (0.001)	-0.006* (0.003)	0.002 (0.002)	0.001 (0.002)	0.000 (0.001)	0.001 (0.000)
Tenure 13 to 18	0.000 (0.002)	0.001 (0.002)	-0.006 (0.003)	0.002 (0.002)	0.001 (0.002)	-0.001 (0.001)	0.001 (0.001)
Tenure 19 to 24	0.000 (0.002)	0.002 (0.002)	-0.005 (0.004)	0.000 (0.003)	-0.001 (0.003)	0.000 (0.001)	0.001 (0.001)
Tenure 25 to 30	-0.001 (0.003)	0.003 (0.003)	-0.005 (0.004)	0.002 (0.003)	0.001 (0.003)	0.000 (0.001)	0.001* (0.001)
Tenure 31 to 36	-0.002 (0.003)	0.004 (0.003)	-0.002 (0.004)	0.002 (0.003)	0.001 (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 37 to 42	-0.001 (0.003)	0.004 (0.003)	-0.004 (0.004)	0.004 (0.004)	0.004 (0.004)	-0.001 (0.001)	0.002* (0.001)
Tenure 43 to 48	-0.002 (0.004)	0.005 (0.003)	-0.002 (0.004)	0.003 (0.004)	0.004 (0.005)	-0.002 (0.001)	0.002** (0.001)
Tenure 49 to 53	-0.001 (0.004)	0.004 (0.004)	-0.006 (0.005)	0.005 (0.004)	0.000 (0.006)	0.000 (0.001)	0.002 (0.001)
N	809,414	809,414	809,414	809,414	809,414	809,414	809,414

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

Table 4. Automatic enrollment effect on debt and net wealth

The first three columns report coefficients from regressions estimated according to equation (6), where the dependent variable is in the column heading. D1 is debt excluding auto loans and first mortgages, D2 is auto loans plus D1, and D3 is first mortgages plus D2. The coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. The next three columns report treatment effect estimates on three measures of net wealth accumulation that are obtained by subtracting treatment effects on debt from the treatment effects on cumulative total TSP contributions reported in Table 2. NW1, NW2, and NW3 subtract D1, D2, or D3, respectively. All dependent variables are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in the debt regressions.

	D1	D2	D3	NW1	NW2	NW3
Tenure ≤ -18	0.002 (0.006)	0.002 (0.007)	0.010 (0.022)	--	--	--
Tenure -17 to -12	-0.005 (0.004)	-0.006 (0.005)	-0.022 (0.017)	--	--	--
Tenure -11 to -6	-0.005 (0.003)	-0.004 (0.003)	-0.021 (0.012)	--	--	--
Tenure 1 to 6	0.001 (0.003)	0.002 (0.004)	0.024 (0.013)	0.010** (0.003)	0.009** (0.004)	-0.014 (0.013)
Tenure 7 to 12	-0.002 (0.004)	0.000 (0.005)	0.015 (0.019)	0.024** (0.004)	0.021** (0.005)	0.007 (0.019)
Tenure 13 to 18	-0.002 (0.005)	0.004 (0.007)	0.031 (0.024)	0.033** (0.005)	0.027** (0.007)	0.000 (0.023)
Tenure 19 to 24	-0.004 (0.006)	0.003 (0.008)	0.017 (0.027)	0.042** (0.006)	0.036** (0.008)	0.021 (0.027)
Tenure 25 to 30	0.001 (0.007)	0.010 (0.009)	0.040 (0.031)	0.045** (0.007)	0.035** (0.009)	0.006 (0.030)
Tenure 31 to 36	0.004 (0.008)	0.018 (0.010)	0.069* (0.034)	0.047** (0.008)	0.033** (0.009)	-0.018 (0.033)
Tenure 37 to 42	0.007 (0.008)	0.023* (0.011)	0.076* (0.038)	0.049** (0.008)	0.033** (0.011)	-0.020 (0.036)
Tenure 43 to 48	0.009 (0.009)	0.028* (0.011)	0.102* (0.041)	0.049** (0.009)	0.029** (0.012)	-0.044 (0.040)
Tenure 49 to 53	0.003 (0.010)	0.020 (0.013)	0.114* (0.047)	0.057** (0.011)	0.040** (0.014)	-0.054 (0.047)
N	809,414	809,414	809,414	--	--	--

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

Table 5. Automatic enrollment effect on subpopulations at 43-48 months of tenure

Each cell except those in the rows labeled NW1-NW3 contains a coefficient from its own separate regression representing the treatment effect of automatic enrollment on the variable indicated in the row label at 43-48 months of tenure for the group in the column header. The contribution regressions are estimated according to equation (1), and the credit regressions are estimated according to equation (6). The cells in the NW1-NW3 rows show the difference between the automatic enrollment effect on cumulative total TSP contributions and its effect on D1-D3, respectively. D1 is debt excluding auto loans and first mortgages, D2 is auto loans plus D1, and D3 is first mortgages plus D2. All dependent variables except for Vantage credit score are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses.

	Salary < $34K	Age < 30	High school only	Baseline Vantage < 620	Black	Hispanic
Cumulative total	0.094**	0.057**	0.071**	0.084**	0.090**	0.085**
TSP contributions	(0.006)	(0.005)	(0.003)	(0.004)	(0.007)	(0.011)
Cumulative employee	0.042**	0.026**	0.033**	0.040**	0.044**	0.043**
TSP contributions	(0.005)	(0.004)	(0.003)	(0.003)	(0.005)	(0.009)
Auto loans and	0.049*	0.021	0.037**	0.037*	-0.002	0.007
leases	(0.022)	(0.014)	(0.011)	(0.018)	(0.022)	(0.035)
First mortgages	0.181	-0.058	0.170**	0.010	0.031	0.100
	(0.117)	(0.086)	(0.059)	(0.091)	(0.122)	(0.197)
D1 (debt excl. auto	0.000	-0.015	0.021	0.048	-0.007	0.045
and first mortgages)	(0.031)	(0.017)	(0.014)	(0.029)	(0.033)	(0.043)
D2	0.049	0.006	0.058**	0.085*	-0.009	0.052
	(0.040)	(0.024)	(0.018)	(0.036)	(0.041)	(0.056)
D3	0.229	-0.052	0.228**	0.095	0.022	0.152
	(0.129)	(0.092)	(0.065)	(0.105)	(0.135)	(0.214)
NW1	0.094**	0.072**	0.050**	0.036	0.097**	0.039
	(0.030)	(0.018)	(0.015)	(0.028)	(0.033)	(0.044)
NW2	0.045	0.050*	0.013	-0.001	0.099*	0.032
	(0.039)	(0.024)	(0.019)	(0.036)	(0.041)	(0.056)
NW3	-0.136	0.109	-0.157**	-0.011	0.068	-0.068
	(0.128)	(0.092)	(0.062)	(0.105)	(0.137)	(0.211)
Vantage credit	2.2	-4.0	0.5	4.6	-0.5	1.5
Score	(3.4)	(2.9)	(1.9)	(3.1)	(4.1)	(7.4)
# of employees at 43-48 months	5,882	7,358	15,576	6,572	4,009	1,448

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

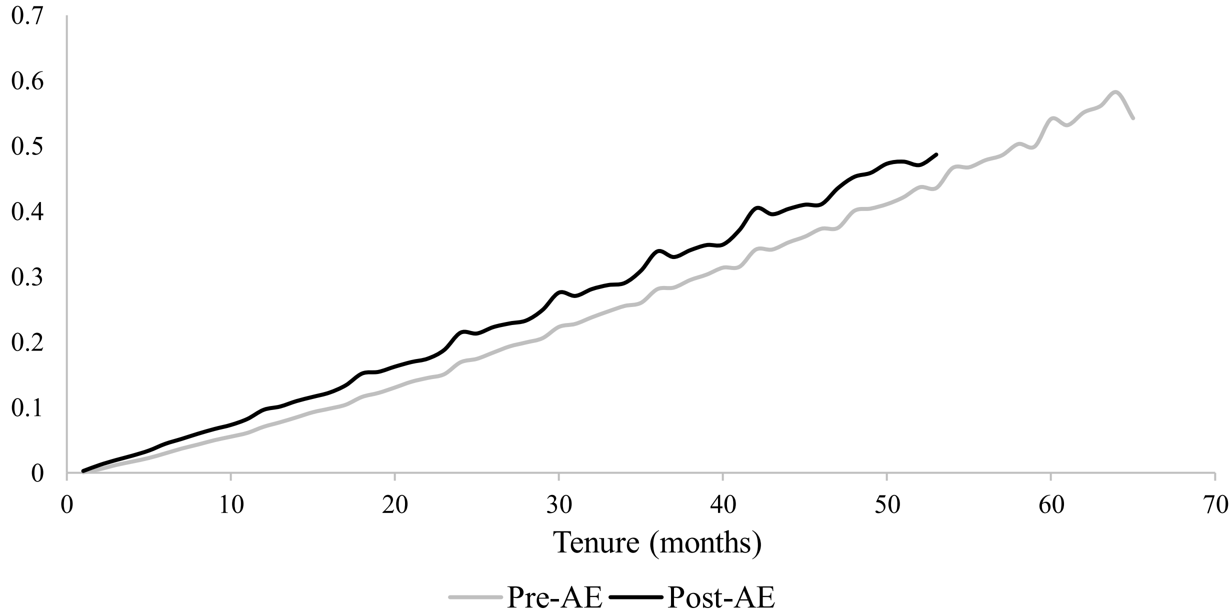


Figure 1. Cumulative total TSP contributions to annualized first-year pay ratio. Every point in the graphed series corresponds to observations in June and December, with number of paychecks scaled to make them comparable at each tenure level across cohorts. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The sample at each tenure level consists of all those employed by the Army at that time.

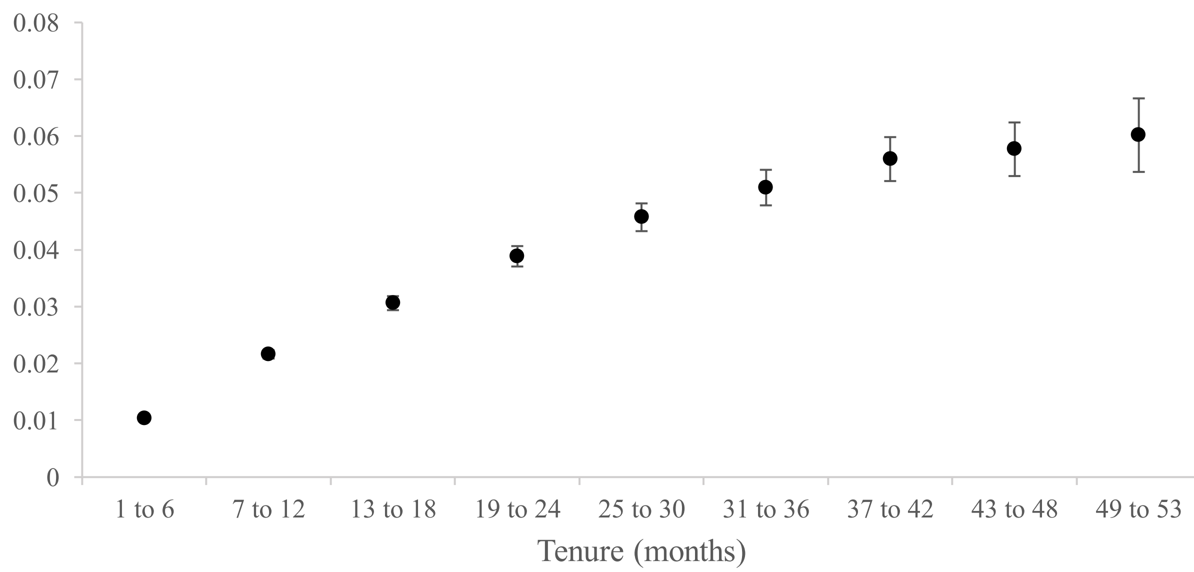


Figure 2. Automatic enrollment treatment effect on cumulative total TSP contributions to annualized first-year pay ratio. The estimates come from the regression in Table 2. Point estimates and 95% confidence intervals are shown.

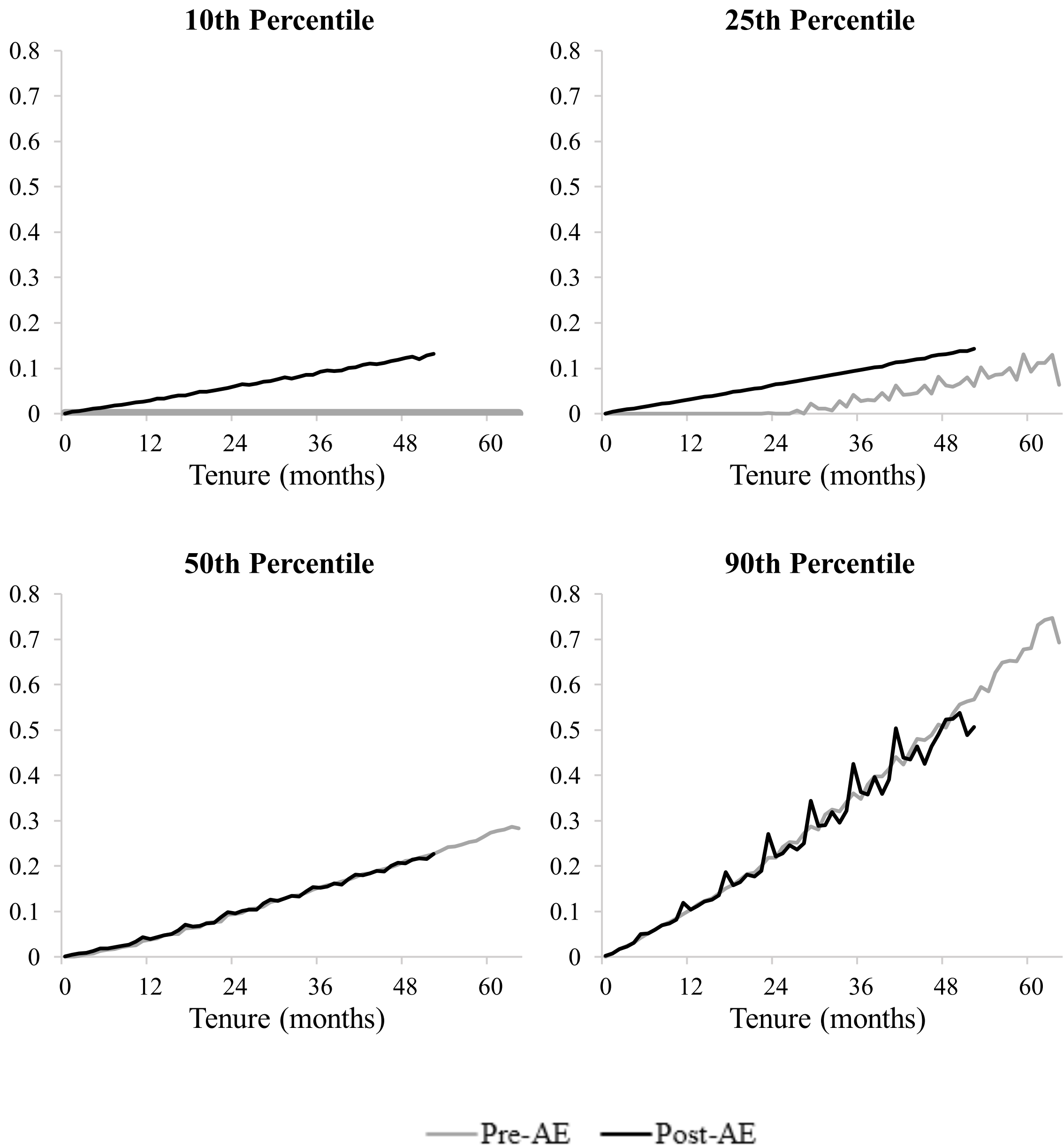


Figure 3. Cumulative employee TSP contributions to annualized first-year pay ratios at 10th, 25th, 50th, and 90th percentiles. Every point in the graphed series corresponds to observations in June and December, with number of paychecks scaled to make them comparable at each tenure level across cohorts. 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 those employed by the Army at that time.

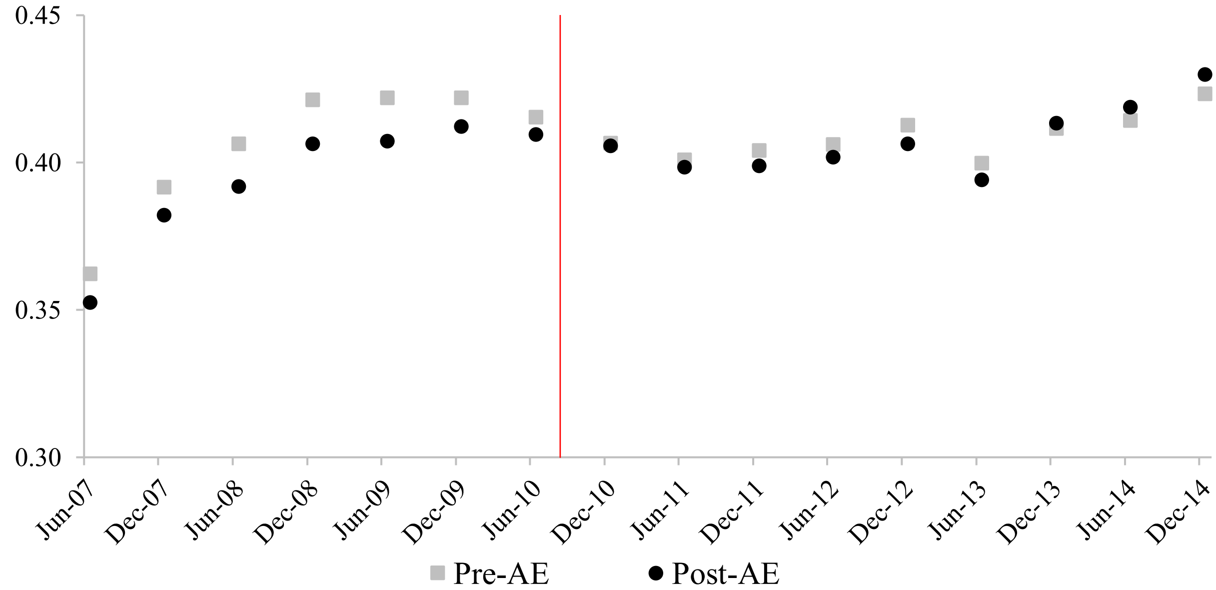


Figure 4. Debt balance excluding auto debt and first mortgages normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment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

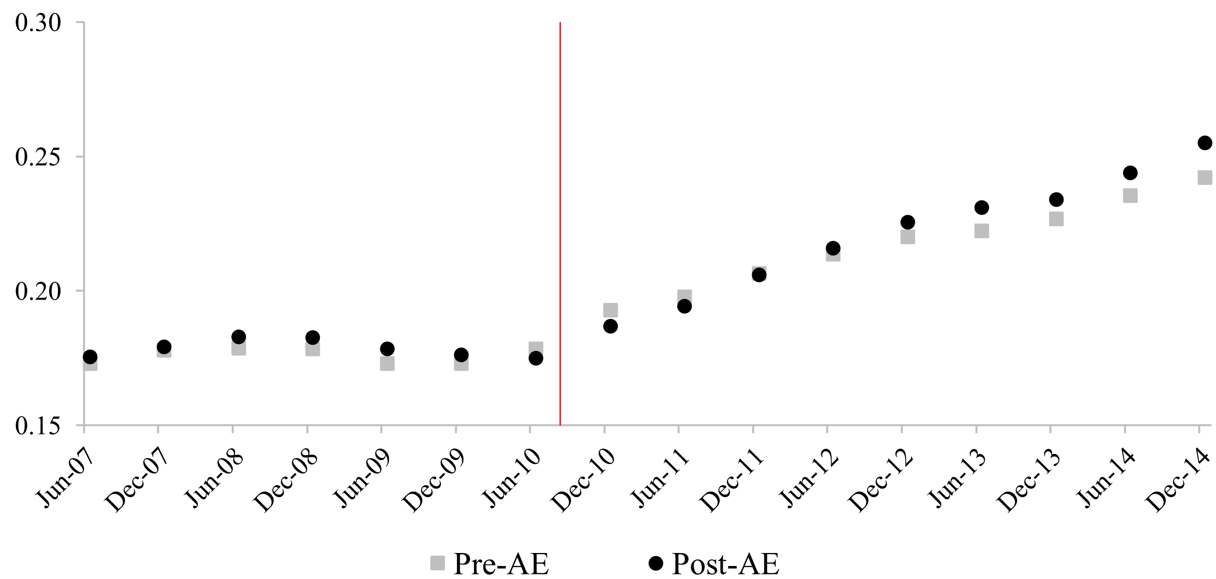


Figure 5. Auto loan and lease balance normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for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

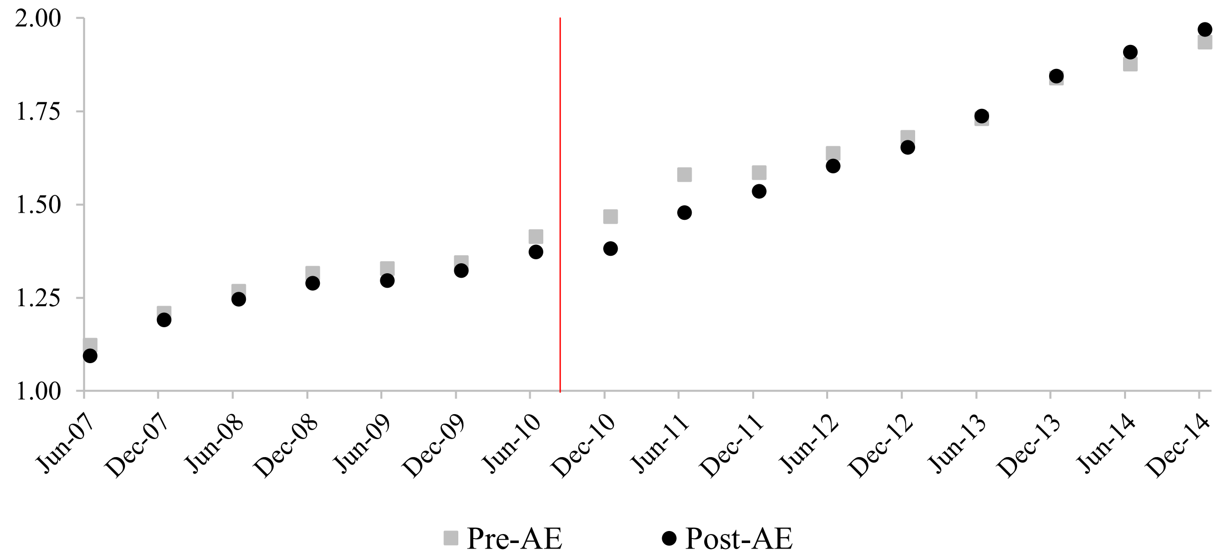


Figure 6. First mortgage balance normalized by annualized first-year pay at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires.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

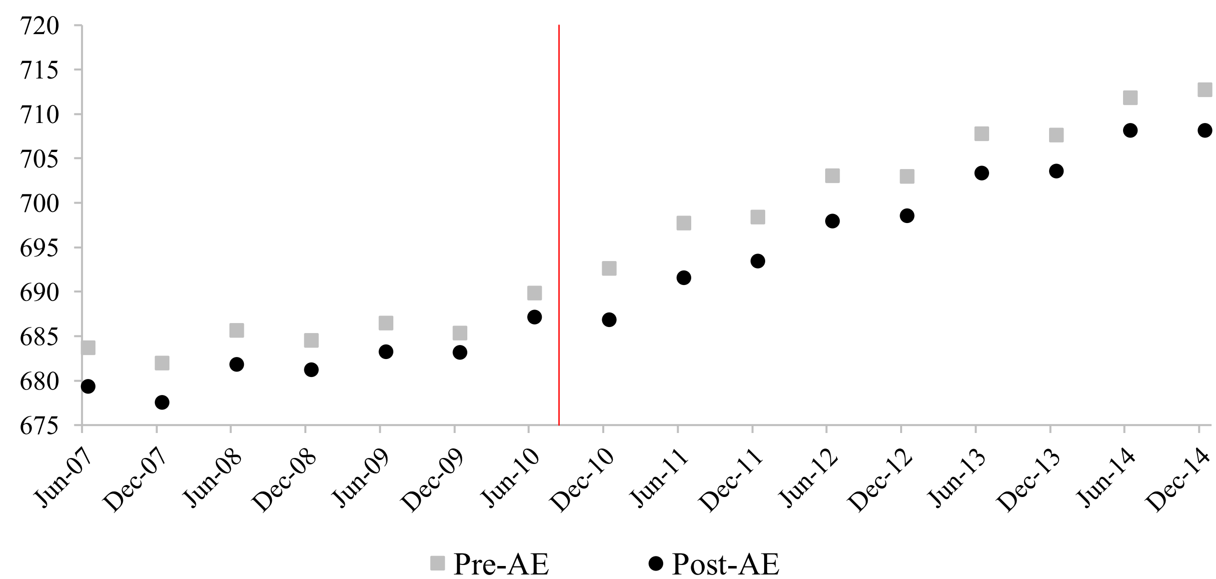


Figure 7. Vantage score at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. The vertical line indicates when automatic enrollment was introduced for new hires. People are dropped from the sample once they have left the Army.

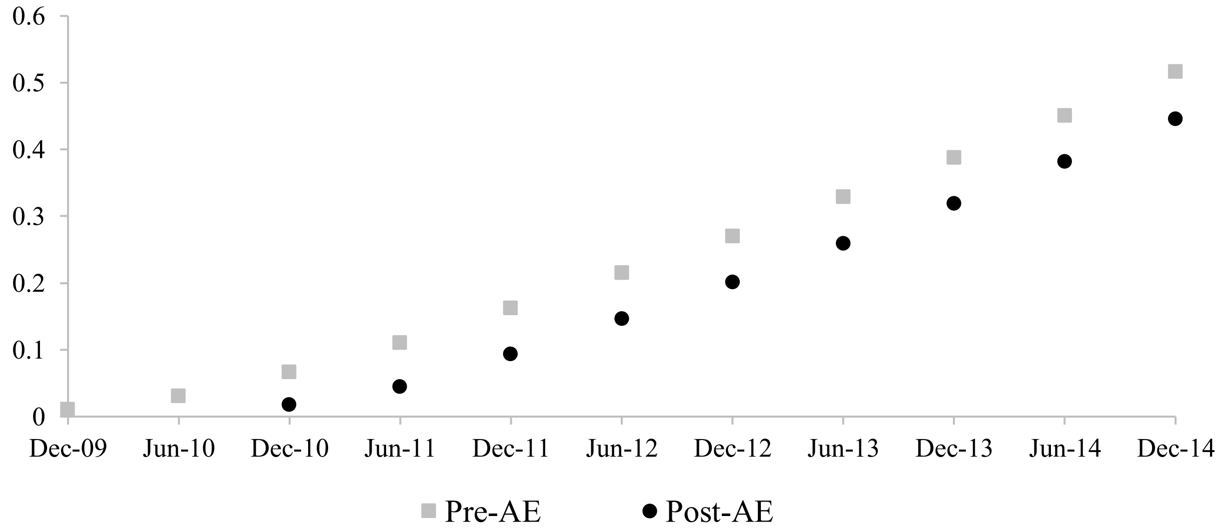


Figure 8. Cumulative total TSP contributions to annualized first-year pay ratio at each calendar date. The pre-AE cohort consists of August 2009 – July 2010 hires, and the post-AE cohort consists of August 2010 – July 2011 hires. People are dropped from the sample once they have left the Army. Contributions are not scaled based on the number of paychecks received to date.

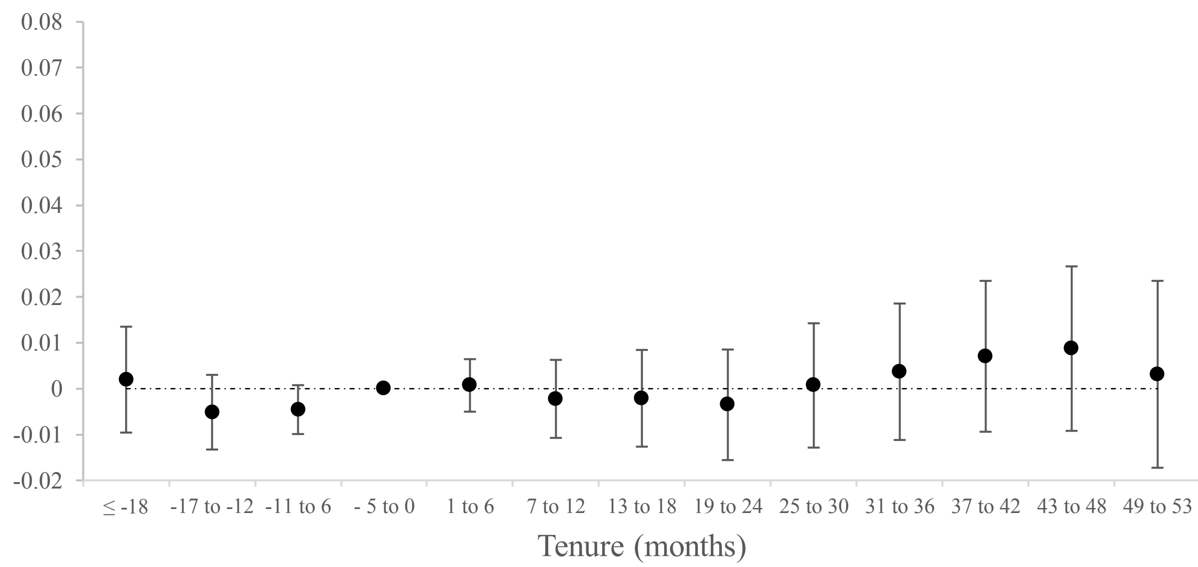


Figure 9. Automatic enrollment effect on debt balance excluding first mortgages and auto loans normalized by annualized first-year pay. The estimates come from the regression in Table 2. Point estimates and 95% confidence intervals are shown.

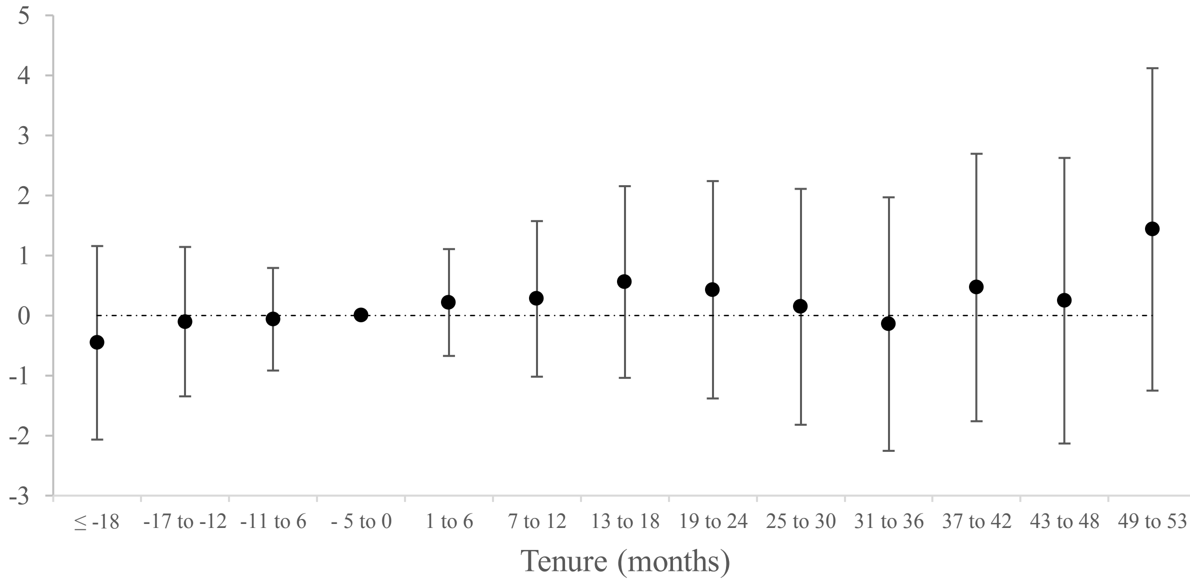


Figure 10. Automatic enrollment effect on Vantage score. The estimates come from the regression in Table 2. Point estimates and 95% confidence intervals are shown.

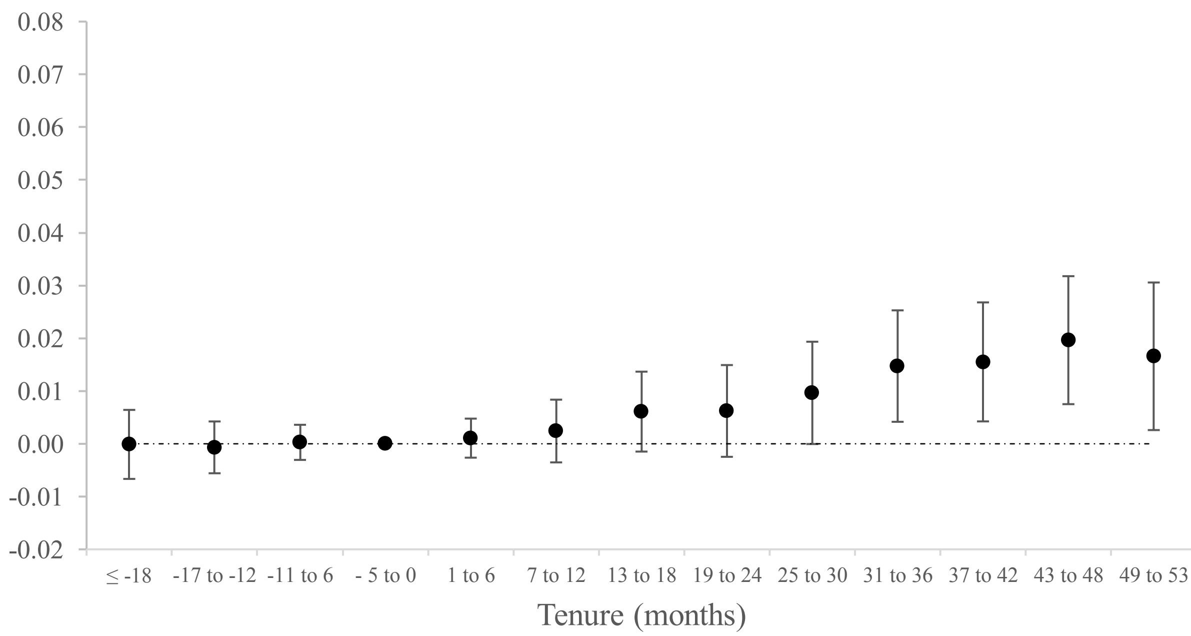


Figure 11. Automatic enrollment effect on auto loan balance normalized by first-year annualized pay. The estimates come from the regression in Table 2. Point estimates and 95% confidence intervals are shown.

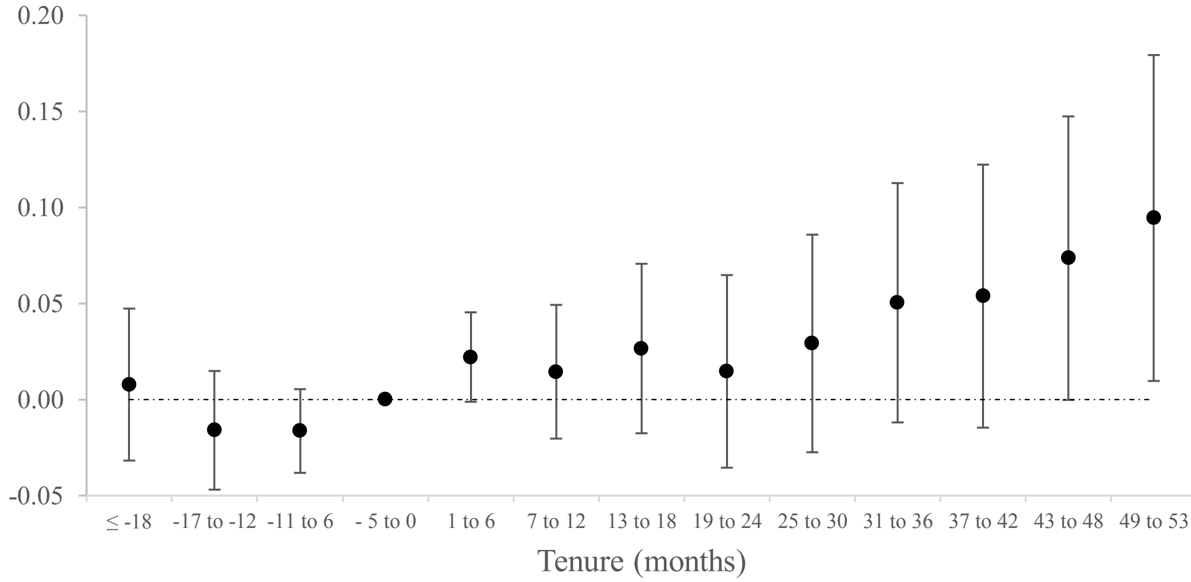


Figure 12. Automatic enrollment effect on first mortgage balance normalized by first-year annualized pay. The estimates come from the regression in Table 2. Point estimates and 95% confidence intervals are shown.

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

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

December 6, 2017

In this appendix, we estimate the automatic enrollment effect on TSP contributions and debt using a regression discontinuity design with hire month as the assignment variable. In our baseline estimate, we find that automatic enrollment causes cumulative TSP contributions to increase by 5.3% of first-year pay at 47 months of tenure. At the same horizon, we detect no statistically significant automatic enrollment effect on debt excluding auto loans and first mortgages (D1) or auto loans, and a statistically significant increase in first mortgage balances of 13.9% of first-year pay. We also estimate that automatic enrollment causes a negligible change in Vantage score. The confidence intervals of all these estimates include the point estimates in the main text.

We prefer the empirical methodology in the main text for two reasons. First, the main text's methodology yields smaller standard errors than the regression discontinuity design. Second, when we subject the regression discontinuity design to placebo tests by estimating effects of automatic enrollment on debt *prior* to hire, we find some significant results three years prior to hire, which might indicate that even after controlling for observables, employees hired in the month before the implementation of automatic enrollment differ unobservably from employees hired in the month of the implementation.

A.I. Methodology

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

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

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

Our data contain the employee's month of hire but not the day of hire, so we assume that employees were hired in the middle of each month. For example, August 2010 hires are coded as having $hiremonth_i = 0.5$, and July 2010 hires are coded as having $hiremonth_i = -0.5$. The coefficient of interest is β_1, the extrapolated difference in y between those hired an instant before August 1, 2010 versus those hired at the very beginning of August 1, 2010. Because all outcomes in the regression are measured as of the same calendar date, there is no need to control separately for calendar time effects, and on this calendar date, those hired an instant before August 1, 2010 have the same tenure as those hired at the beginning of August 1, 2010, so there is no need to control separately for tenure effects. We present our results under several bandwidths (4, 8, and 12 months on each side of the hire date threshold) to illustrate that they are not particularly sensitive to bandwidth choice. Due to the small number of running variable values, we do not cluster standard errors by the assignment variable.

In order for β_1 to consistently estimate the treatment effect of automatic enrollment, month of hire around August 2010 must be as-if randomized, conditional on observables. Since employees can easily implement the automatic enrollment defaults themselves, there is little incentive to self-sort across the August 2010 hire threshold, so as-if conditional randomization is plausible. Appendix Table 1 shows that controlling for observables is important. Compared to the cohort hired during the month before the implementation of automatic enrollment, the cohort hired during the first month of automatic enrollment is lower-income, less educated, and less likely to hold a professional position. The magnitudes of the differences are bigger for the one-month cohorts than for the one-year cohorts analyzed in the main text, suggesting that the month-to-month variation is smoothed out when averaging over more months.

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

A.II. Results

Appendix Table 2 presents the results from the regression discontinuity analysis. When describing the results, we focus on those obtained from the 12-month bandwidth, but the findings are qualitatively similar using the other bandwidths.

At 47 months of tenure (corresponding to the 43-48 month tenure bucket that is our preferred long-run horizon in the main text), automatic enrollment increases cumulative total TSP contributions by 5.4% of first-year income (95% confidence interval = [4.2%, 6.6%]. There is no significant effect on debt excluding auto loans and first mortgages (D1) at the same horizon; the point estimate is 1.1% of first-year income (95% confidence interval = [-1.1%, 3.3%]). There is statistically significant increase in auto debt at 35 months of tenure of 1.6% of income, but the statistical significance disappears at other horizons, and the point estimate at 47 months is 1.1% (95% confidence interval = [-0.5%, 2.7%]). First mortgage debt shows a significant increase starting at 17 months, and by 47 months, automatic enrollment increases first mortgage debt balances by 13.9% of first-year income (95% confidence interval = [4.9%, 22.9%]). There is an economically negligible effect on Vantage score at 47 months of 0.3 points (95% confidence interval = [-2.6, 3.2]. All of these confidence intervals contain the point estimates in Table 2 of the main text.

Appendix Figures 1-5 present visual analogues of the above analysis using the 12-month bandwidth. The vertical axes represent the residual values $\tilde{y}_{it}$ from regressions of the outcome variable y_{it} on the covariates X_i. The fitted lines are from regressions of the form

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

The data points plotted are the average residualized value of the outcome for people with that hire month.[1]

If our identifying assumptions are valid, we should estimate no effect of automatic enrollment on outcomes *prior* to hire. Appendix Table 3 shows the results of these placebo tests. There are no estimates on outcomes at tenure month -13, since this tenure corresponds to June 2009, the baseline date from which we compute differences. We also do not use bandwidths of 8 and 12 months for tenure month -7 (January 2010), since the wider bandwidths cause both individuals hired and not hired as of January 2010 to be included in the pre-AE cohort sample, and the assumption of local linearity may not hold across a sample of both hired and not-yet hired individuals.

[1] The b coefficients are close but not identical to the β coefficients in Appendix Table 2. Per the Frisch-Waugh-Lovell Theorem, we could produce identical estimates by residualizing the regressors in a similar way, but at the cost of visual clarity.

We find no significant pre-hire effects on first mortgage debt and credit score through tenure month -37. For auto debt, there is a significant positive effect of 4.2% of first-year income at tenure month -25, but only when using a 12-month bandwidth. More concerning are the pre-hire estimates for D1. There are no significant effects through tenure month -25, but at tenure month -31, the regressions estimate a significant 1.6% to 2.4% across the three bandwidths. At tenure month -37, the 8-month and 12-month bandwidths estimate a significant 1.8% and 1.5% effect, respectively. On the one hand, these significant differences appear only at a point fairly distant in the past, and with *t*-statistics hovering around 2, their statistical significance is not overwhelming given the large number of tests we have run in Appendix Table 3. On the other hand, the fact that there are any significant placebo results at all casts some doubt on the validity of the regression discontinuity specification.

Appendix Table 1. Comparison of employees hired in month before versus month of automatic enrollment implementation

	Pre-AE (Jul '10 hires)	Post-AE (Aug '10 hires)	Difference	*p*-value of difference
Avg. starting salary	$56,981	$53,787	-$3,194	0.000
Avg. age at hire	39.0	38.9	0.0	0.900
Male	63.2%	65.2%	2.0%	0.113
White	50.9%	54.7%	3.7%	0.005
Black	11.8%	12.1%	0.3%	0.695
Hispanic	3.2%	4.3%	1.1%	0.031
Asian	2.9%	4.2%	1.3%	0.010
Missing race	30.4%	23.7%	-6.7%	0.000
High school only	42.1%	47.4%	5.3%	0.000
Some college, no degree	12.3%	12.6%	0.3%	0.732
Associates degree	5.1%	5.1%	0.1%	0.907
Bachelor's degree	21.8%	18.1%	-3.6%	0.001
Graduate degree	17.7%	15.8%	-1.9%	0.054
Unknown education	1.1%	1.0%	-0.1%	0.591
STEM college major	14.1%	11.3%	-2.8%	0.001
Business college major	11.8%	11.2%	-0.6%	0.461
Other college major	21.4%	18.2%	-3.2%	0.002
Administrative position	29.5%	31.5%	2.1%	0.092
Blue collar position	8.6%	7.5%	-1.1%	0.117
Clerical position	7.6%	6.9%	-0.8%	0.271
Professional position	25.5%	19.3%	-6.2%	0.000
Technical position	16.6%	16.2%	-0.4%	0.652
Has June or December credit report in 6 months preceding	82.8%	83.2%	0.4%	0.677
Avg. Vantage Score for credit report in 6 months preceding hire, conditional on having Vantage Score	689.3	688.0	-1.3	0.643
N	2,432	3,414		

Appendix Table 2. Automatic enrollment effects on TSP contributions and debt changes since June 2009

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

		Tenure (months)								
	Bandwidth	5	11	17	23	29	35	41	47	53
Cumulative total TSP contributions	4 months	0.012**	0.012**	0.027**	0.028**	0.035**	0.038**	0.040**	0.043**	0.042**
		(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)	(0.007)	(0.008)	(0.010)
	8 months	0.013**	0.023**	0.032**	0.034**	0.044**	0.047**	0.053**	0.058**	0.059**
		(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
	12 months	0.012**	0.021**	0.030**	0.032**	0.041**	0.044**	0.049**	0.054**	0.054**
		(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)
Debt excluding auto, first mortgage (D1)	4 months	0.007	-0.008	-0.005	0.005	0.016	0.003	0.019	0.027	0.028
		(0.011)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.018)	(0.019)	(0.021)
	8 months	0.005	-0.005	-0.001	0.002	0.002	0.005	0.008	0.014	0.007
		(0.008)	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)	(0.013)	(0.013)	(0.014)
	12 months	0.006	-0.004	-0.004	0.001	-0.001	0.004	0.007	0.011	0.010
		(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.011)	(0.011)	(0.012)
Auto debt	4 months	0.014	0.010	0.008	0.006	0.017	0.024*	0.009	0.011	0.006
		(0.007)	(0.008)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	(0.013)	(0.014)
	8 months	0.010	0.012	0.007	0.009	0.017*	0.019*	0.011	0.010	0.006
		(0.005)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
	12 months	0.004	0.010*	0.007	0.008	0.012	0.016*	0.009	0.011	0.010
		(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
First mortgage debt	4 months	0.064	0.064	0.177**	0.180**	0.048	0.115	0.113	0.190*	0.113
		(0.045)	(0.050)	(0.058)	(0.061)	(0.067)	(0.071)	(0.077)	(0.079)	(0.084)
	8 months	0.067*	0.080*	0.142**	0.134**	0.091	0.119*	0.095	0.177**	0.124*
		(0.032)	(0.035)	(0.040)	(0.043)	(0.047)	(0.050)	(0.054)	(0.056)	(0.059)
	12 months	0.033	0.053	0.102**	0.089**	0.084*	0.109**	0.065	0.139**	0.089
		(0.026)	(0.028)	(0.033)	(0.035)	(0.038)	(0.041)	(0.044)	(0.046)	(0.048)
Vantage credit score	4 months	1.7	1.1	0.7	0.6	2.3	0.3	0.2	-0.9	1.2
		(1.7)	(1.8)	(2.0)	(2.1)	(2.3)	(2.4)	(2.5)	(2.6)	(2.8)
	8 months	-0.7	0.6	1.1	-0.2	0.7	0.5	1.1	1.6	2.1
		(1.2)	(1.3)	(1.4)	(1.5)	(1.6)	(1.7)	(1.8)	(1.8)	(1.9)
	12 months	-1.3	0.4	0.3	-0.5	-0.1	-0.0	-0.7	0.3	0.0
		(1.0)	(1.0)	(1.2)	(1.2)	(1.3)	(1.4)	(1.5)	(1.5)	(1.6)

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

Appendix Table 3. Automatic enrollment effects on debt changes relative to June 2009, prior to hire

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

		Tenure (months)					
		-37	-31	-25	-19	-13	-7
Debt excluding auto, first mortgage (D1)	4 months	0.015	0.024*	0.014	0.012	--	-0.005
		(0.013)	(0.012)	(0.009)	(0.008)		(0.008)
	8 months	0.018*	0.019*	0.011	0.008	--	--
		(0.009)	(0.008)	(0.006)	(0.005)		
	12 months	0.015*	0.016*	0.010	0.005	--	--
		(0.007)	(0.007)	(0.005)	(0.004)		
Auto debt	4 months	-0.052	-0.036	-0.060	-0.044	--	0.021
		(0.045)	(0.041)	(0.035)	(0.029)		(0.032)
	8 months	0.033	0.048	0.023	0.001	--	--
		(0.032)	(0.028)	(0.024)	(0.019)		
	12 months	0.048	0.046	0.042*	0.013	--	--
		(0.026)	(0.023)	(0.019)	(0.016)		
First mortgage debt	4 months	-0.008	-0.010	-0.005	-0.004	--	0.003
		(0.007)	(0.006)	(0.005)	(0.004)		(0.004)
	8 months	0.000	-0.003	-0.003	-0.003	--	--
		(0.005)	(0.004)	(0.004)	(0.003)		
	12 months	0.002	-0.001	-0.001	-0.003	--	--
		(0.004)	(0.004)	(0.003)	(0.002)		
Vantage credit score	4 months	-1.01	-1.83	-1.42	-0.14	--	1.09
		(1.85)	(1.71)	(1.47)	(1.17)		(1.17)
	8 months	-1.28	-0.98	-0.69	0.02	--	--
		(1.30)	(1.20)	(1.03)	(0.82)		
	12 months	-1.03	-0.67	-0.18	0.46	--	--
		(1.07)	(0.99)	(0.85)	(0.67)		

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

Appendix Table 4. Automatic enrollment effects on contributions and debt components in a constant sample

Each column reports coefficients from a regression whose dependent variable is in the column heading. The contribution regressions are estimated according to equation (1), the credit regressions are estimated according to equation (6), and the coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. All dependent variables except for Vantage credit score are normalized by first-year annualized salary. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in each regression. The sample contains only people who remain employed at 43 to 48 months.

	Cumulative total TSP contributions	Cumulative employee TSP contributions	Debt excluding auto, first mortgage (D1)	Vantage credit score	Auto debt	First mortgage debt
Tenure	--	--	0.008	-1.6	-0.005	-0.008
≤ -18			(0.007)	(1.0)	(0.004)	(0.026)
Tenure	--	--	-0.002	-1.0	-0.005	-0.036
-17 to -12			(0.005)	(0.8)	(0.003)	(0.020)
Tenure	--	--	-0.003	-0.5	-0.001	-0.015
-11 to -6			(0.004)	(0.6)	(0.002)	(0.014)
Tenure	0.009**	0.004**	0.002	0.5	0.002	0.029
1 to 6	(0.000)	(0.000)	(0.004)	(0.6)	(0.002)	(0.015)
Tenure	0.019**	0.009**	-0.001	0.2	0.005	0.027
7 to 12	(0.000)	(0.000)	(0.005)	(0.8)	(0.004)	(0.022)
Tenure	0.027**	0.013**	0.002	0.8	0.007	0.049
13 to 18	(0.001)	(0.001)	(0.007)	(1.0)	(0.005)	(0.027)
Tenure	0.034**	0.016**	0.002	0.6	0.005	0.015
19 to 24	(0.001)	(0.001)	(0.007)	(1.1)	(0.005)	(0.031)
Tenure	0.041**	0.019**	0.006	0.0	0.007	0.030
25 to 30	(0.001)	(0.001)	(0.008)	(1.2)	(0.006)	(0.034)
Tenure	0.046**	0.021**	0.010	-0.7	0.009	0.053
31 to 36	(0.002)	(0.001)	(0.009)	(1.2)	(0.006)	(0.037)
Tenure	0.051**	0.023**	0.015	-0.2	0.010	0.052
37 to 42	(0.002)	(0.002)	(0.009)	(1.3)	(0.006)	(0.039)
Tenure	0.058**	0.026**	0.016	-0.7	0.011	0.075
43 to 48	(0.002)	(0.002)	(0.010)	(1.3)	(0.007)	(0.042)
Tenure	0.064**	0.029**	0.011	0.3	0.010	0.107*
49 to 53	(0.003)	(0.003)	(0.011)	(1.5)	(0.008)	(0.047)
N	316,446	316,446	579,200	482,068	579,200	579,200

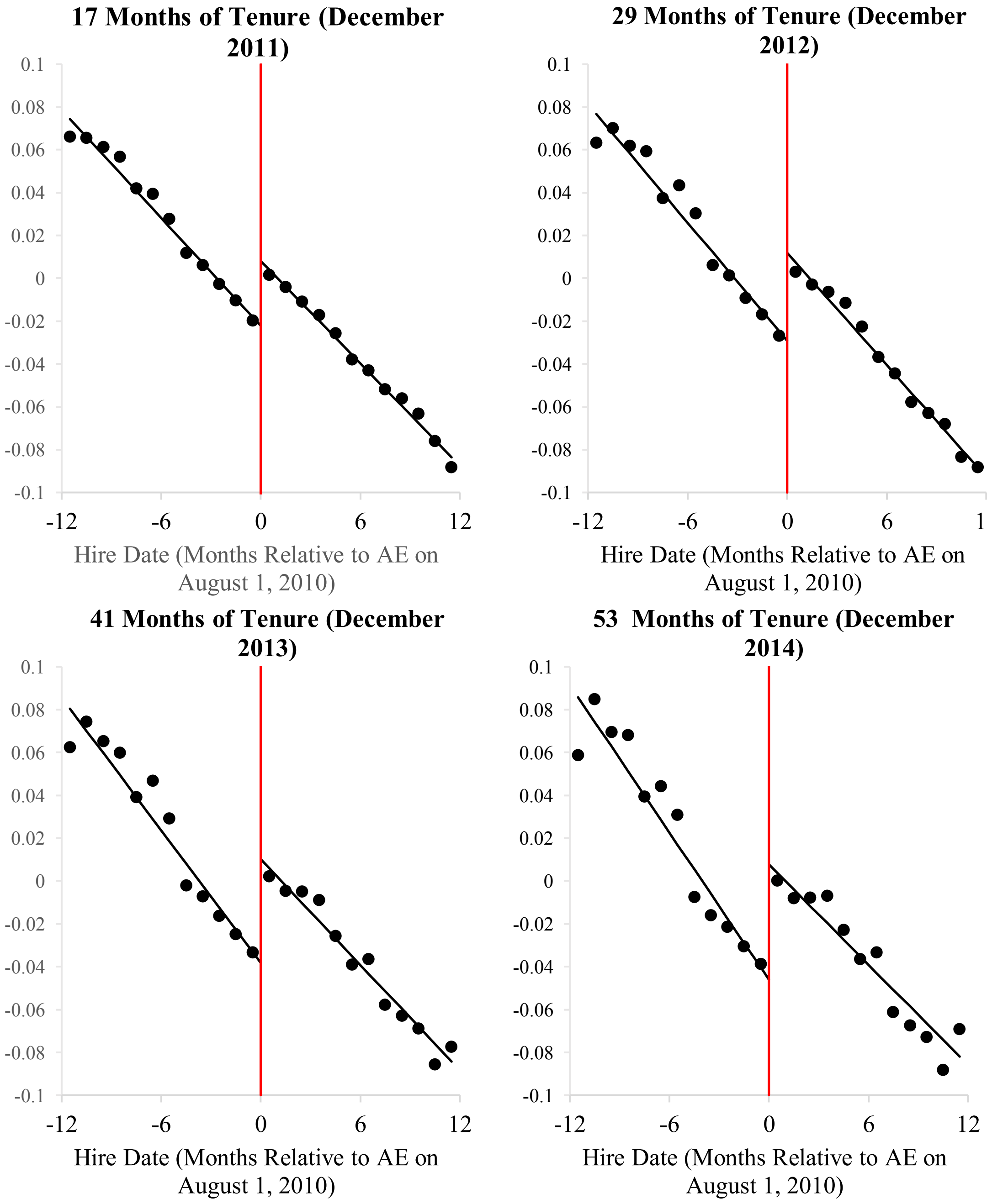
* Significant at 5% level. ** Significant at 1% level.

Appendix Table 5. Automatic enrollment effects on D1 subcomponents on a constant sample

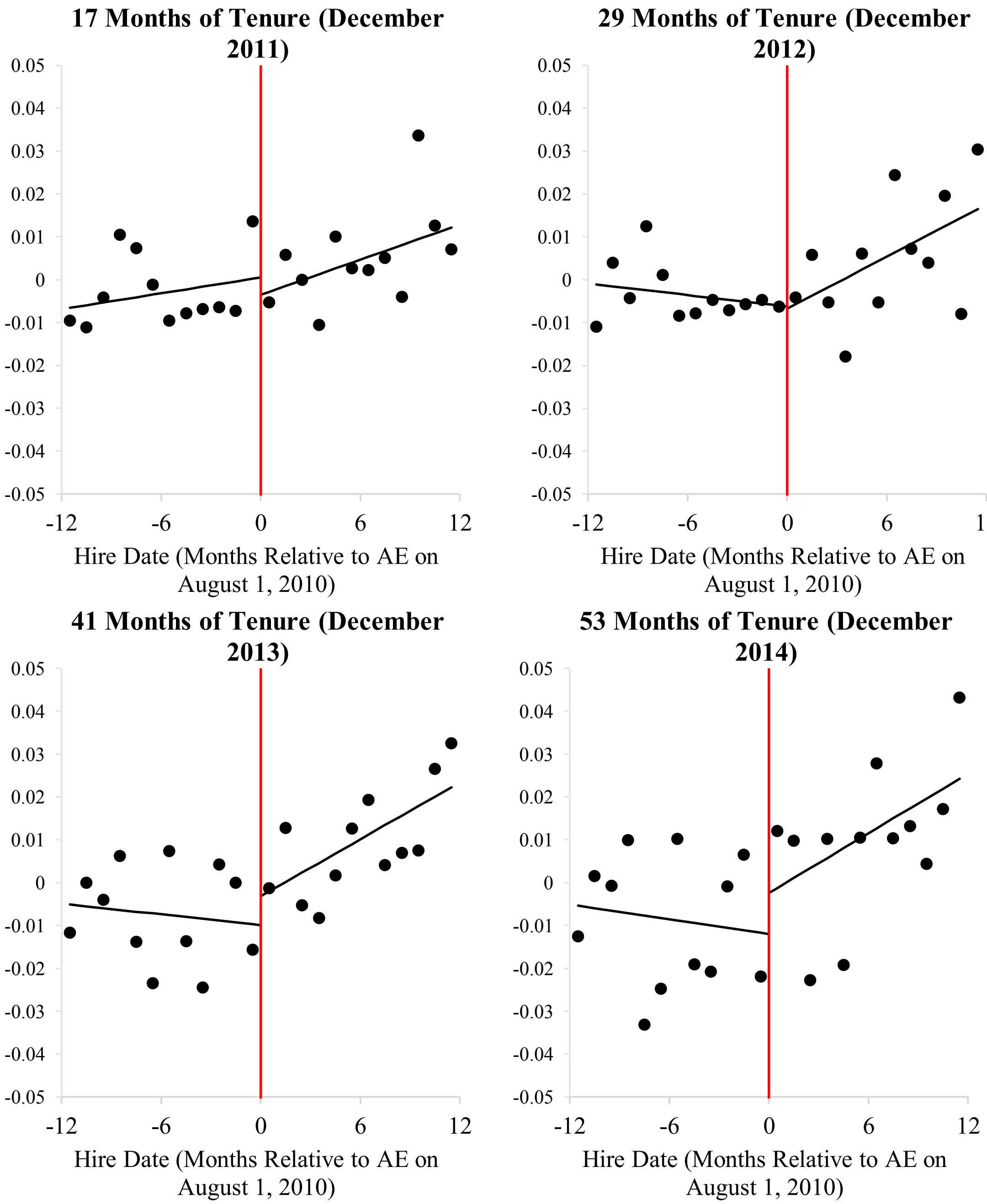
Each column reports coefficients from a regression estimated according to equation (6) whose dependent variable is in the column heading. All dependent variables are normalized by first-year annualized salary. The coefficients correspond to the treatment effect of automatic enrollment at the tenure months indicated. Standard errors clustered at the employee level are in parentheses. The last row shows the number of person-months in each regression. The sample contains only people who remain employed at 43 to 48 months.

	HELOC revolving	Non-HELOC revolving	Other installment loans	Second mortgages	Student loans	External collections	Residual debt
Tenure	0.002	0.004	0.000	0.007*	-0.004	-0.001	0.000
≤ -18	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Tenure	-0.001	0.002	-0.002	0.002	-0.003	-0.001	0.000
-17 to -12	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.000)
Tenure	-0.001	0.001	-0.003	0.000	-0.001	0.000	0.000
-11 to -6	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Tenure	0.000	0.000	-0.001	0.001	0.001	0.000	0.000
1 to 6	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.000)	(0.000)
Tenure	0.000	0.002	-0.004	0.000	0.001	0.000	0.000
7 to 12	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.001)	(0.000)
Tenure	0.000	0.004	-0.004	0.001	0.001	0.000	0.000
13 to 18	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Tenure	-0.001	0.005	-0.003	0.001	0.000	0.000	0.001
19 to 24	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
Tenure	-0.002	0.005	-0.003	0.002	0.002	0.000	0.001
25 to 30	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.001)	(0.001)
Tenure	-0.003	0.005	0.001	0.002	0.004	0.000	0.001
31 to 36	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.001)	(0.001)
Tenure	-0.001	0.006	-0.002	0.005	0.007	0.000	0.001
37 to 42	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.001)	(0.001)
Tenure	-0.002	0.007	-0.002	0.005	0.007	0.000	0.001
43 to 48	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.001)	(0.001)
Tenure	-0.001	0.007	-0.006	0.007	0.003	0.000	0.001
49 to 53	(0.004)	(0.004)	(0.005)	(0.004)	(0.006)	(0.001)	(0.001)
N	579,200	579,200	579,200	579,200	579,200	579,200	579,200

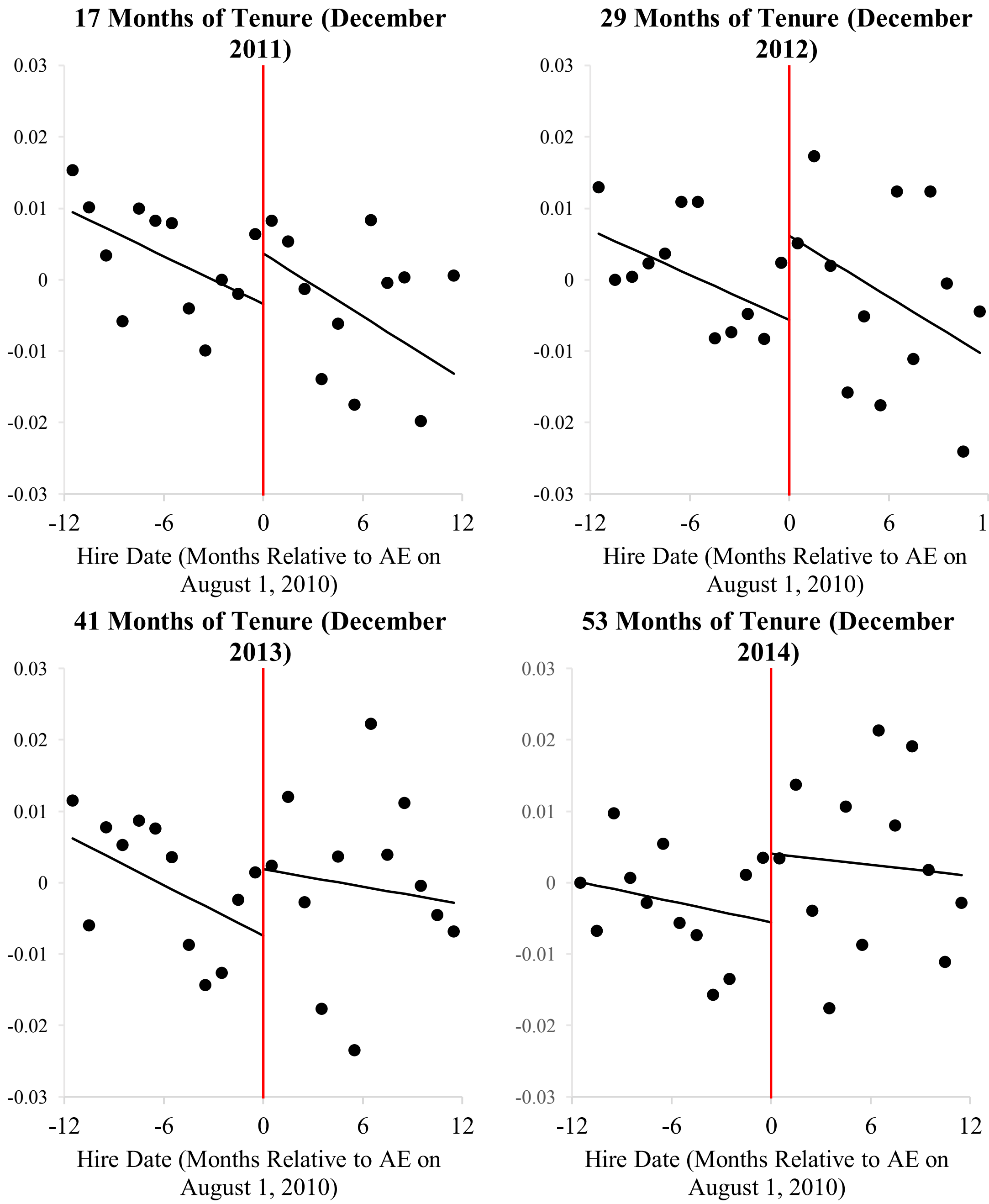
* Significant at 5% level. ** Significant at 1% level.



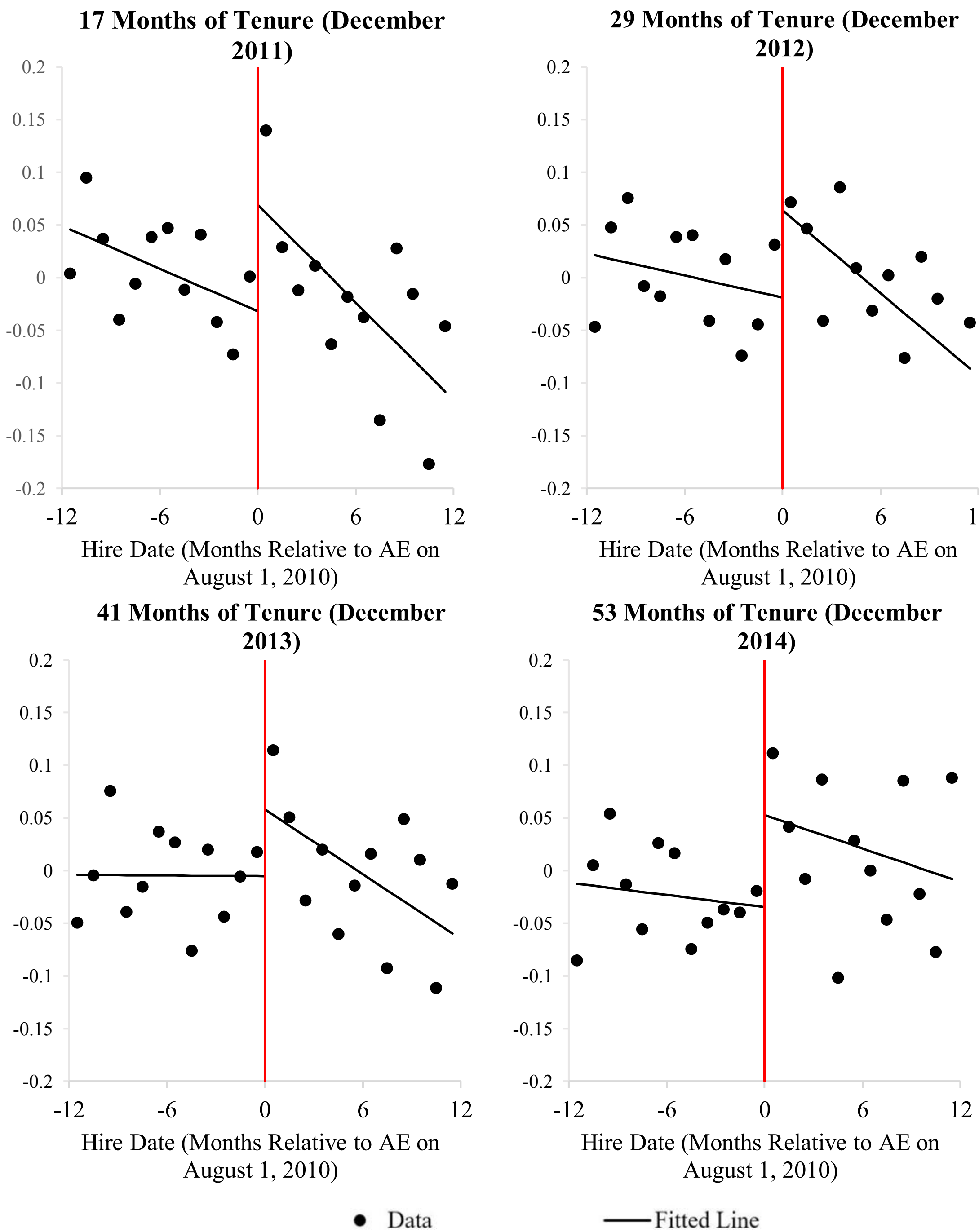
Appendix Figure 1. Automatic enrollment effect on total TSP contributions to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



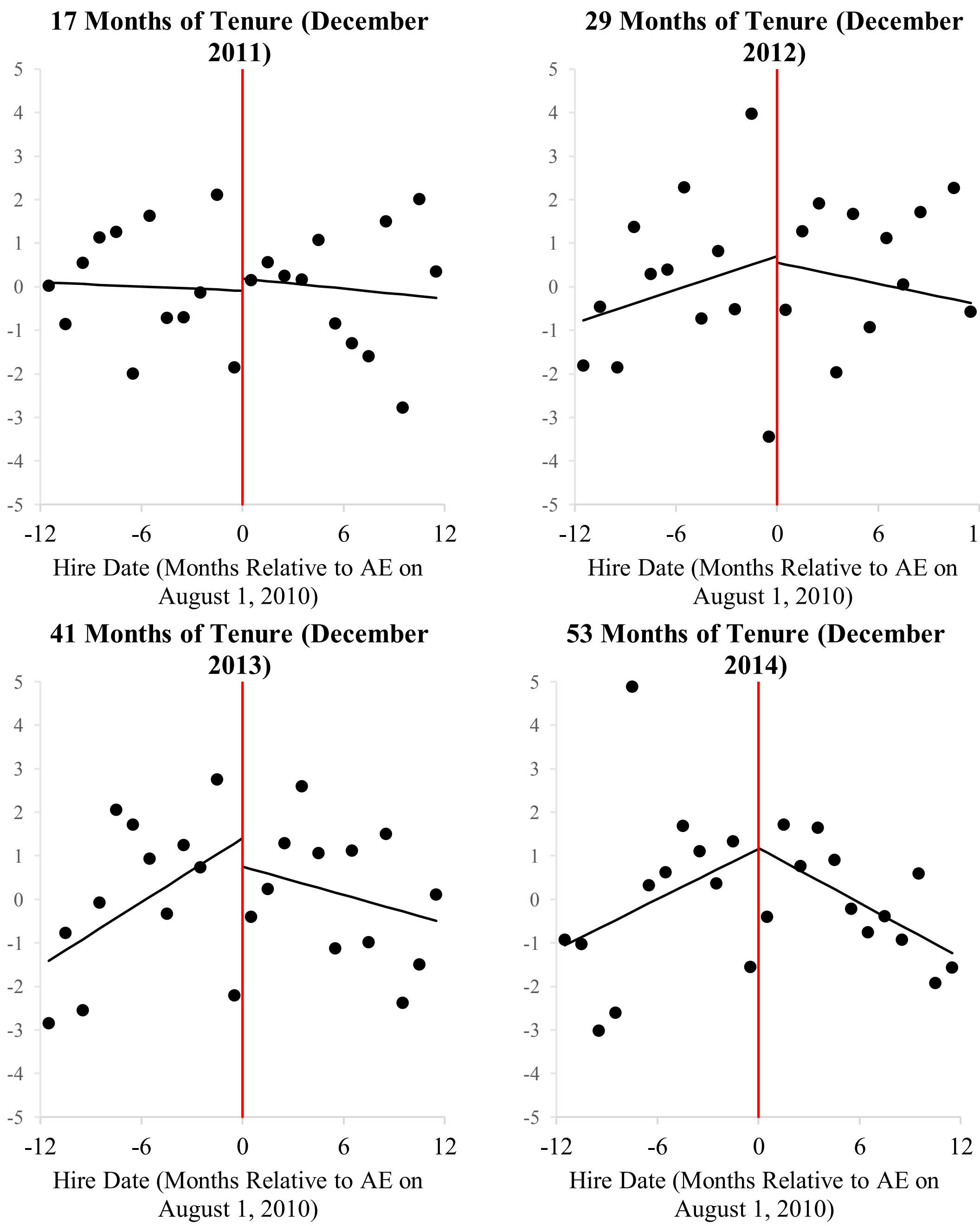
Appendix Figure 2. Automatic enrollment effect on change since June 2009 in debt excluding auto and first mortgage debt (D1) to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired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



Appendix Figure 3. Automatic enrollment effect on change since June 2009 in auto debt to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart’s title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



Appendix Figure 4. Automatic enrollment effect on change since June 2009 in first mortgage debt to first-year pay, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).



Appendix Figure 5. Automatic enrollment effect on change since June 2009 in Vantage score, 12-month bandwidth. The plotted data points are average residualized values of the outcome variable measured at the date in each chart's title for those hired in the month indicated in the horizontal axis. The lines are fitted lines from the regression in equation (A.2).