Do Automatic Savings Policies Actually Increase Savings?

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Abstract: Automatic enrollment and default contribution rate auto-escalation have become widely adopted in retirement savings plans on the belief that these nudges increase savings. We find that previous estimates of their savings effects are overstated. Our new estimates at eight companies incorporate the facts that employee turnover is high, a large percentage of 401(k) balances leave the retirement savings system upon employment separation, and employees may opt out of the auto-escalation default. The net savings rate increase generated by automatic enrollment, default auto-escalation introduced on top of pre-existing automatic enrollment, and the simultaneous introduction of automatic enrollment and default auto-escalation is only 0.5%, 0.3%, and 0.7% of income per year, respectively, on average. Employees with positive balances under automatic policies withdraw a higher proportion of these balances upon separation, and only 37% of those with an auto-escalation default accept it at their first auto-escalation date.

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Automatic enrollment—where individuals contribute to a savings plan unless they opt out—has become a major part of retirement savings policy around the world.[1] In 2019, 40% of U.S. private industry workers and 28% of U.S. state and local government workers participating in a savings and thrift plan did so in one with automatic enrollment (Zook, 2023). In 2022, 40% of plans administered by Vanguard automatically escalated employee contributions annually by default (Vanguard, 2023). The SECURE 2.0 Act of 2022 requires most 401(k) plans established in 2025 and later to automatically enroll new employees at a default contribution rate of between 3% and 10% of income, and to by default auto-escalate their contribution rate by 1% of income per year up to at least 10% and no more than 15% of income. Ten U.S. states have passed legislation requiring employers that do not sponsor a 401(k) plan to automatically enroll employees in an Individual Retirement Account.

In this paper, we revisit the question of how effective such automatic savings policies are at increasing accumulation in the U.S. retirement savings system. Much work has found that automatic enrollment greatly increases the fraction of employees who contribute to the 401(k) and modestly increases average contribution rates (Madrian and Shea, 2001; Choi et al., 2002, 2004; Beshears et al., 2008; Choukhmane, 2023; Derbie, Mackie, and Mortenson, 2023). Thaler and Benartzi (2004) find that employees who opt into automatic escalation eventually experience large increases in their contribution rates, and Benartzi, Peleg, and Thaler (2012) report that the take-up rate of automatic escalation increases from about 25% to about 85% when it is made the default. Our paper's results indicate that these previous estimates overstate how much retirement savings is being created by automatic policies.

We study eight firms that introduced automatic enrollment, default automatic escalation in a context where automatic enrollment was already present, or automatic enrollment and default automatic escalation simultaneously. The automatic policies applied only to employees hired from a certain date onward, so we identify their effect by comparing 35,609 employees hired shortly after the policy introductions to 31,635 employees hired shortly before. We incorporate three methodological advances that have never been simultaneously present in prior studies.

First, our treatment effect estimates are averaged over both high-turnover and low-turnover employees. Many new hires leave the firm after only a short tenure; in 2022, about 4% of the U.S.

[1] Italy, Lithuania, New Zealand, Poland, Turkey, and the United Kingdom have implemented automatic enrollment at the national level (OECD, 2021).

nonfarm labor force left its current job each month, according to the Job Openings and Labor Force Turnover Survey. Most previous analyses of automatic policies only estimate long-horizon effects conditional on remaining at the firm through the horizon being examined. Employees who attain long tenures at a firm may have different treatment responses than employees who leave quickly. And automatic escalation, by construction, only acts upon employees who remain at the firm for a sufficiently long time.

Second, we account for the fact that employees withdraw a large percentage of their 401(k) balances upon employment separation, either voluntarily or because employers are allowed by law to compel a complete liquidation of account balances below $1,000. Argento, Bryant, and Sabelhaus (2015) report that for every dollar contributed to defined contribution retirement accounts in 2010 by those under the age of 55, 40 cents leaked out to this group as a pre-retirement withdrawal in the same year. Because automatic savings policies have the strongest positive contribution effect on those with weak savings motives, much of the positive asset accumulation induced by automatic policies during the employment spell may be withdrawn upon termination.

Third, we credit auto-escalation only for contribution increases that actually occur. Except in one 401(k) plan, Benartzi, Peleg, and Thaler (2012) are only able to measure take-up of default auto-escalation by observing whether employees have opted out *before* their first scheduled contribution increase. They mostly do not observe whether employees' contribution rates indeed increase on the auto-escalation date.

To eliminate noise induced by the realization of asset returns, our main outcome variable is based on how much individuals would have accumulated in the retirement savings system during their tenure at the observed job if retirement account returns were a constant 5% per year, given their contribution rate path and the relationship between withdrawals and 401(k) balances at separation. We convert this net-of-withdrawals accumulation amount (which includes vested employer contributions) into the constant contribution rate with no withdrawals that would have resulted in the same net accumulation by the end of the employee's tenure. This conversion essentially annualizes each employee's net savings rate over his entire tenure, making the outcome variable comparable across employees who remain at the company for different lengths of time. The average of these equivalent constant contribution rates across employees in a cohort can be interpreted as the steady-state annualized cohort-average contribution rate if separating employees exactly repeat their contribution, withdrawal, and attrition behavior in every subsequent job spell.

The difference in this average between treatment and control cohorts is the steady-state treatment effect on annualized net retirement savings rates.

Our headline result is that previous estimates of how much automatic savings policies increase retirement savings accumulation are upwardly biased. The average increase in the equivalent constant contribution rate across the three automatic enrollment companies—with default contribution rates of 2%, 4%, and 5%—is only 0.5% of income per year. Default automatic escalation that is added on top of pre-existing automatic enrollment raises the equivalent constant contribution rate by an average of 0.3% of income per year. Introducing both automatic enrollment (at a default contribution rate of 2% or 3%) and default automatic escalation simultaneously increases the equivalent constant contribution rate by 0.7% of income per year, on average. These long-run treatment effects are on average 71% smaller than estimates from a naïve extrapolation of short-term treatment effects that ignores leakage, and 54% smaller than treatment effects sans leakage estimated at 60 months of tenure only among individuals still employed at the company at that time.

The savings effects are modest for three primary reasons. First, many individuals not subject to automatic policies actively choose positive contribution rates that erode much of the savings gap that would exist if individuals were perfectly passive. Second, acceptance of auto-escalation is surprisingly low when it is the default. Across our five default auto-escalation firms, the average fraction of employees who are still at the firm and subject to the default who escalate on the first escalation date is only 37%, and acceptance of the escalation default decays further at each subsequent escalation date.[2] Third, almost half of 401(k) balances are cashed out upon departure from the firm, and the employee turnover rate is high. Controlling for balance at separation, there is little difference in cash leakage rates between those who are and are not subject to automatic policies. However, individuals in treated cohorts are more likely to separate with small positive 401(k) balances, so the average leakage rate in treated cohorts is higher than in the control cohorts.

[2] Burke, Hung, and Luoto (2017) estimate in a large sample of 401(k) plans that 59% of employees with default auto-escalation deviate from the completely passive contribution rate path during their sample period. However, this percentage does not directly measure acceptance of auto-escalation, since choosing a non-default contribution rate does not necessarily turn off auto-escalation; on the escalation date, the contribution rate would increase from whatever the current contribution rate is for somebody with auto-escalation in effect.

Our paper is related to others that have looked for crowding out of savings nudges at unnudged margins. Most closely related is Derbie, Mackie, and Mortenson (2023), who estimate in a large sample of firms that some of automatic enrollment's savings effect is undone by subsequent withdrawals, but there is no crowd-out via reduced retirement contributions by the employee's spouse. We are distinguished from Derbie, Mackie, and Mortenson (2023) in that (1) we analyze auto-escalation in addition to automatic enrollment, (2) we are able to observe employer contributions, which are an important component of 401(k) wealth accumulation, and (3) we estimate longer-run treatment effects using cohorts hired close together in time, whereas they need to compare cohorts hired five years apart from each other, heightening concerns about confounding calendar time effects.

Choukhmane (2023) finds that automatic enrollment in the current job's pension causes workers to become less likely to save in their next job's pension if they have to opt into it, but there is no such dynamic crowd-out if the next pension also uses automatic enrollment. Beshears et al. (2022) report no statistically significant effect of automatic enrollment on financial distress, credit scores, or debt excluding auto and mortgage debt. Using a much larger sample, Beshears et al. (2023) find that 25% of the pension savings created by automatic enrollment is offset by increased unsecured debt—an estimate that is within the 95% confidence interval of the corresponding Beshears et al. (2022) estimate—and automatically enrolled individuals become more likely to have a mortgage, but they are not more likely to be in financial distress. Blumenstock, Callen, and Ghani (2018) find that automatic enrollment does not crowd out other savings, although their estimates are imprecise. Chetty et al. (2014) estimate that compulsory pension savings has only small crowd-out effects on non-pension saving.

Outside of the retirement saving context, Medina (2021) shows that credit card payment reminders increase checking account overdraft fees, while Guttman-Kenney et al. (2023) find that shrouding the option to automatically make only the minimum monthly credit card payment has no effect on debt reduction because of offsetting consumer responses. Medina and Pagel (2023) find that a text message encouraging savings is successful at increasing saving among a subset of recipients while not increasing borrowing. Brown, Grozicki, and Medina (2023) report that limiting the marketing of credit cards to college students increases their student loan balances.

The paper proceeds as follows. Section I describes how we select our sample of firms and the nature of our data. Section II discusses how we choose our control and treatment cohorts and construct the equivalent constant contribution rate outcome variable. Section III describes the results of our estimations. Section IV concludes. The Appendix shows that any cash leakage from the 401(k) occurs shortly after job separation, which justifies the way we treat cash leakage in constructing our outcome variable.

I. Firm selection and data description

Our 401(k) administrative data come from Alight, a company whose services include providing defined contribution pension recordkeeping services for employers. Within a universe of approximately 200 firms, we identified 86 instances of automatic savings policies that were implemented between January 1, 2003 and January 1, 2011.[3] We then imposed the following requirements: (a) the policy only affected employees hired after the policy was introduced, (b) additional automatic savings policies were not introduced during the five years after cohort hire over which we measure outcomes, (c) data on contribution rate elections were available during the study period, and (d) employee attrition rates were similar between the control cohort hired before the introduction of the automatic policy and the treatment cohort hired after the introduction. We describe later how we chose the control and treatment cohorts at each company, and the criterion we used to determine whether their attrition rates were similar enough for inclusion in the study. The above conditions restricted our sample to eight automatic policy implementations.

Table 1 describes the 401(k) plan features at each firm in our sample. We divide our firms into those that introduced automatic enrollment only (“automatic enrollment firms”), those that introduced default automatic escalation in a context where they were already automatically enrolling employees (“automatic escalation firms”), and those that introduced automatic enrollment and default automatic escalation simultaneously (“automatic enrollment and escalation firms”). The initial default contribution rates are mostly low—in the range of 2 to 3% of income—but the automatic enrollment sample includes defaults of 4 and 5%, and one of the automatic escalation firms introduced escalation on top of a pre-existing 6% default. At firms that introduce default automatic escalation, contribution rates automatically increase by 1% of income per year

[3] We identified the introduction of automatic savings policies from a survey of Alight clients conducted in 2010 and 2019, by reading plan documents that span a range of years that differs for each company, and by searching for large discontinuous increases in the number of employees at a certain contribution rate, which is indicative of a new contribution rate default or automatic escalation. If there are automatic savings policies that we failed to identify from the administrative data because their effect was more muted than what the literature has previously documented, then our sample is biased towards more “successful” policies.

until a maximum that varies from 6% to 15% of income across firms. At Firms D and E, the first automatic escalation date did not occur within the first year of tenure for some employees. At Firm D, even though employees hired from January 2011 onwards were subject to automatic escalation, the first automatic escalation date was not until April 2012. At Firm E, automatic escalation occurs every January, but only for employees with at least six months of tenure, so employees hired from July to December do not automatically escalate until their second year of tenure.

All of our firms match employee contributions. Two of the automatic enrollment and escalation firms increased the generosity of their match rates in the middle of the study period, and one increased the match threshold (the level beyond which contributions are no longer matched) for a small number of its employees. Three firms feature immediate vesting of employer contributions—employees forfeit none of their employer contributions no matter when they leave the company—but most companies require a certain length of tenure before employer contributions are fully vested.

We have two types of administrative data. The first data set is a series of cross-sections at year-end for each firm in our study. Each cross-section contains employee-level information on birth date, hire date, gender, salary, and job termination date. It also contains year-end 401(k) plan balances; total dollars contributed to the plan (separately for employer and employee contributions) during the year; and for each withdrawal, the date of the transaction, the total dollars withdrawn, and the total dollars rolled over to an outside retirement account. The second data set contains monthly 401(k) contribution rate elections for each employee. These contribution rates are chosen as a fraction of salary and can be changed by employees at any time.

II. Methodology

A. Cohort construction

We identify the effect of automatic policies by comparing two hire cohorts within each company. The treatment cohort was subject to an automatic policy because it was hired after the policy introduction date, whereas the control cohort was never subject to the automatic policy.

At each firm, we choose cohorts using a process that aims to select cohort pairs with comparable attrition over time, similar demographics at baseline, and a large sample size. Potential treatment cohorts are employees hired in the 90, 180, 270, or 365 days after the automatic policy was implemented. Potential control cohorts are employees hired in the 90, 180, 270, or 365 day window that either begins one year before policy implementation ("seasonally matched") or ends one day before policy implementation ("adjacent"). We only consider cohort pairs where the treatment and control cohorts have the same hire window width, which means that we have seven candidate treatment-control cohort pairs for each firm. Using a seasonally matched control cohort eliminates differences between the treatment and control cohorts caused by hiring seasonalities. Using an adjacent control cohort reduces the calendar-time difference between when the two cohorts were hired, minimizing differences arising from any monotonic calendar-time trend in the types of employees the company is hiring.

Our measure of attrition imbalance is the average of the absolute difference in cumulative attrition between the treatment and control cohorts over the first 60 months of tenure:

$$\frac{1}{60}\sum_{\tau=1}^{60}\left|a_{treat,\tau} - a_{control,\tau}\right| \tag{1}$$

where $a_{c,\tau}$ is the percent of hire cohort c that has left the firm by the end of tenure month τ. Our measure of demographic imbalance is the equal-weighted average of three differences between the treatment and control cohorts: differences in mean salary divided by its standard deviation (pooled across both cohorts), mean age divided by its pooled standard deviation, and percent female. Sample size is the number of employees across both cohorts at baseline.

We exclude cohort pairs with attrition imbalance above 2.5%. Two firms (not included in Table 1) are dropped based on this criterion, as attrition imbalance exceeds 2.5% for all their candidate cohort pairs. For the remaining firms, we start with the cohort pair p that has the largest sample size. Let p' be an alternative cohort pair, $A_{p,p'}$ the percentage change in attrition imbalance obtained by moving from p to p', $D_{p,p'}$ the corresponding percentage change in demographic imbalance, and $S_{p,p'}$ the corresponding percentage change in sample size. Let P be the set of all p' for which $-A_{p,p'} - D_{p,p'} + S_{p,p'}$ is positive (i.e., an overall improvement). If P is not an empty set, we select the p' in P with the largest sample size. We repeat the process, comparing the newly selected pair only to pairs with smaller sample sizes, until no further improvements are possible or we have selected the pair with the smallest sample size.

Table 2 shows the characteristics of the treatment and control cohorts at each firm. Despite our procedure to achieve demographic balance between the treatment and control cohorts, there are a few statistically significant imbalances that remain, although the differences tend to be economically small. Our focus is on the biases that are created by ignoring employee turnover, leakage, and low auto-escalation uptake. Controlling for differences in observables in a regression has little qualitative impact on these biases. In addition, Firms A and E have statistically significant differences in employee attrition at the five-year mark. However, the economic magnitude of the difference at Firm A is quite small. In Appendix Figure 1, which shows the cumulative attrition rates with respect to tenure for treatment and control cohorts at each firm in our sample, we see that the attrition differences between the two cohorts at Firm E are nearly non-existent until the last few months of tenure.[4]

B. Outcome variable construction

Our goal is to estimate the effect of automatic savings policies on retirement wealth accumulation. One way to do this is to compare retirement account balances of the treatment cohort to retirement account balances of the control cohort at equivalent times since hire. However, such a comparison would yield a highly imprecise estimate because each cohort will have experienced different capital gains at each tenure time due to random fluctuations in the capital markets. In addition, the comparison would be confounded by the fact that the introduction of automatic enrollment is accompanied not only by a change in the default contribution rate, but an introduction of an asset allocation default that almost always differs from the average asset allocation chosen by participants not under automatic enrollment. We are seeking to estimate the effect of automatic savings policies purged of the effects of noise in capital gains and differential asset allocations.

Therefore, we instead choose to analyze as our outcome variable a synthetic measure of retirement wealth accumulation achieved under a hypothetical scenario where employees' investment returns are a constant 5% per year. We map synthetic retirement wealth accumulation for each employee over the course of his tenure at the firm to the constant contribution rate that would result in the same final synthetic balance at employment separation if annual returns are always 5% and no withdrawals are ever made. Estimating the treatment effect on this equivalent constant contribution rate allows us to interpret the treatment effect as the annualized increase in savings rates that would result from applying a given automatic policy forever (including at future

[4] Appendix Figure 1 also shows cumulative attrition at the two firms we dropped due to the attrition differences between all candidate treatment and control cohort pairs being too large. The pattern of the differences is qualitatively different from those at the firms we retained in the study.

jobs) instead of applying the control policy forever, under the assumption that an employee who departs a sample firm will have the same 401(k) features and exactly repeat her contribution, withdrawal, and attrition behaviors in every subsequent job spell.

In this subsection, we describe the construction of the most comprehensive version of the outcome variable. When we discuss our results in Section III, we will present treatment effects of automatic policies not just on this most-comprehensive outcome variable, but also on outcome variables that exclude or modify some of the steps in the outcome's construction.

Contribution accumulation

We begin with employee contribution rate elections, which are observed monthly and expressed as a percent of salary.[5] We assume that contributions are deposited at the very end of each month. Because of gaps in our data on salary and unnormalized dollars contributed to the 401(k), our approach cumulates contribution rates over time without adjustment for salary growth.

While individual i is employed at the firm, her synthetic balance b at the end of month t follows the law of motion

$$b_{i,t} = 1.05^{1/12} b_{i,t-1} + c_{i,t}\left(1 + m_{i,t} v_i\right)/12 \quad (2)$$

where $c_{i,t}$ is i's contribution rate at t, $m_{i,t}$ is the percent of i's contribution at t that is matched by the employer, and v_i is the percent of the employer match that is vested at the end of the employee's tenure at the company.[6] We divide the monthly contribution rate by 12, which makes our synthetic balance variable normed by annual salary.

Contribution rates are sometimes missing in our data. If contribution rates are missing for every month of an employee's tenure and the employee remained at the firm for at least six months, then we drop the employee from the sample. This criterion excludes 1% or fewer of the employees hired in the two-year window centered on the policy introduction date at six firms, but excludes 3% and 7% of such employees at firms E and F, respectively. For employees who are always

[5] Although Choi et al. (2009) find that 401(k) contribution rates increase in response to personally experienced 401(k) returns that are high on average with low variance, our calculations do not adjust contribution rates to account for how they would change in response to experiencing returns that are a constant 5%.

[6] Firms E, G, and H offer different matches to different employees, but our data do not explicitly identify which match structure an employee has. We infer each employee's match structure from the ratio of employee contribution dollars to employer contribution dollars within a calendar year, combined with the path of contribution rate elections during that calendar year. We assign the most common match structure within the company to those whose dollar contribution data are missing.

missing contribution rates and leave the firm within six months, we impute a zero contribution rate for their entire tenure. For the remaining employees, if their missing contribution rates are at the beginning or end of their tenure, we impute the closest non-missing contribution rate to those observations. Otherwise, we use linear interpolation to fill in missing observations. The bottom of Table 2 shows that we impute 3% or fewer contribution rate observations at all firms except Firm G, which has the greatest percentage of missing observations (11.1%) because contribution rate data are unavailable for two calendar years during the study period.

Correcting for changes in the employer match structure over time

At firms G and H, the employer match became significantly more generous in the years after each firm implemented its automatic policy. Because the control cohort was hired prior to the treatment cohort, these match changes occur later in tenure time for the control cohort than for the treatment cohort. In order to approximate what the two cohorts' retirement accumulation would have been had both of them experienced the same path of 401(k) match structures in tenure time, we calculate synthetic balances for the control cohort under the assumption that any change in the match structure occurred twelve months earlier in calendar time than its actual date for its members.[7] More generous matches are likely to increase average employee contributions (Choi, 2015), but we do not alter the contribution rates chosen by employees when doing our calculations. This biases us towards estimating a more positive treatment effect at firms G and H.

Withdrawals at employment termination

Upon terminating employment at a firm, the employee's money in the 401(k) sponsored by that firm becomes withdrawable at any time for any reason. Employees have to pay ordinary income tax on any withdrawals that are not rolled over within 60 days to another 401(k) or an Individual Retirement Account (IRA). Employees younger than 59½ also usually have to pay a penalty equal to 10% of the withdrawal unless the money is rolled over. Individuals can request

[7] The match threshold increased only for some employees at firms G and H, but our data do not explicitly identify which employees received this increase. We can only adjust the match threshold that applies to control employees one year before the actual match threshold change for those whom we observe contributing at or above the new match threshold after the actual match threshold change. In the final year we observe an employee, we assume that she has her last known match structure.

that the 401(k) administrator directly roll over the withdrawal into another retirement account, in which case the withdrawn amount never passes through the individual's bank account.

We observe in our data the amount within each calendar year that is directly rolled over and the amount that is paid out to the individual. We do not observe when individuals who have had a withdrawal paid out to them subsequently roll over the withdrawal on their own. Such instances are likely to be rare because it is more convenient to execute a direct rollover, and direct rollovers are not subject to tax withholding.

The threshold of $1,000 of balances at separation is significant because during our sample period, if the employee's 401(k) balance at separation was less than $1,000, the employer could—without the employee's consent—completely liquidate the employee's 401(k) account and send the proceeds to the employee via check. Although the individual can in principle roll over the cash withdrawal herself, small-dollar withdrawals from retirement accounts are in practice especially unlikely to be rolled over (Poterba, Venti, and Wise, 2001; Argento, Bryant, and Sabelhaus, 2015). Therefore, whether one accumulates balances greater then $1,000 before leaving the firm plays a large role in determining whether one's 401(k) dollars stay within the retirement savings system. Balances between $1,000 and $5,000 could also be unilaterally moved out of the 401(k) by the employer, but unless the employee chose otherwise, these balances had to be rolled over into an IRA of the employer's choice. Balances above $5,000 had to be retained in the employer's 401(k) indefinitely until the employee chose to withdraw them.[8]

Figure 1 shows how the cash leakage rate in the year of employment separation varies with estimated 401(k) balance at separation. We estimate 401(k) balance at separation as the sum of cash withdrawal dollars during the separation year, rollover dollars during the separation year, and 401(k) balance on December 31 of the separation year. The cash leakage rate is cash withdrawal dollars during the calendar year of separation divided by estimated 401(k) balance at separation.[9] The sample pools data on all treatment and control cohort employees at all firms (except for Firm G, which is missing withdrawals data) who terminate employment in a July, August, or September within our sample period. We restrict the termination months because compulsory cash distributions are typically enacted three to four months after an employee's separation date (see

[8] The $5,000 boundary was increased to $7,000 starting in 2024 by the SECURE 2.0 Act.
[9] If there were no capital gains or losses from employment separation to year-end, this ratio would exactly equal cash withdrawal dollars divided by 401(k) balance at separation.

Appendix Figure 2), we only observe 401(k) balances at each December 31, and we wish to have a fairly accurate measure of the 401(k) balances used by the firm to determine whether it can involuntarily cash out the former employee's account.

Consistent with firms' ability to compulsorily cash out balances under $1,000, we see in Figure 1 that the cash leakage rate for such balances is almost 100%. The fact that the cash leakage rate for balances under $1,000 is not exactly 100% is due in part to measurement error created by the fact that we observe balances on a date that is not exactly the date on which the firm determined whether it can compulsorily cash the employee out. In addition, some employees with such small balances may proactively request that they be rolled over. Cash leakage rates drop discretely to around 50% right above the $1,000 balance threshold, and then continue to decline gradually.

Because of the critical role of balance at separation in determining cash leakage, we reduce a separating employee's synthetic balances in the retirement savings system by a cash leakage percentage that depends on the employee's synthetic balance at separation. The Appendix presents evidence that any cash withdrawals from the 401(k) tend to be made immediately; the cumulative cash leakage rate approaches its asymptote soon after separation. Therefore, when an individual separates, we immediately reduce her synthetic balance by a cash leakage percentage and do not impose subsequent reductions. We treat rollover balances as remaining in the retirement savings system indefinitely, as our data contain no information on what happens to these balances after they leave the sponsoring employer's 401(k).

We compute the average leakage rate in the year of separation within each of the balance bins shown in Table 3, separately for the treatment and control cohorts. (In fact, conditional on balance size, leakage rates are similar between the cohorts and usually statistically indistinguishable from each other.) We reduce each separating employee's synthetic 401(k) balance by the leakage rate in the cell that matches her cohort and synthetic balance in absolute dollars at separation.[10] Because the synthetic balances described in equation (2) are normalized by salary, we multiply normalized balance at separation by the individual's first observed salary (deflated to dollars in the control cohort's hire year). If salary data are missing for an employee's entire tenure, then we use the median first-observed salary for employees hired in the two-year window centered on the automatic policy introduction date.

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[10] Appendix Table 1 shows that variation in balance is a far more important correlate of leakage than variation in employee age.

The penultimate row of Table 2 shows that at four firms, we need to impute salary for almost nobody, but among the remaining four firms, we impute salary for between 6% and 18% of employees. The number of employees missing all salary observations is larger (shown in the last row), but we do not need to impute salaries for many of them because they never have non-zero contribution rates. The fact that these never-contributing employees are missing salaries does reduce our sample size later when we run regressions that control for salary.

Simulating future outcomes for employees who do not separate by 60 months of hire

For individuals who do not separate from the job by 60 months of tenure (i.e., the end of our observation period), we simulate their future separation date using the empirically observed, firm-specific monthly rate at which control and treatment employees leave the firm from tenure months 49 to 60.[11] We randomly assign these remaining employees to leave each month beyond tenure month 60 at this separation rate (which is the same for both control and treatment employees within a firm) until every employee has separated from the firm. Using a similar approach, we simulate at firms with auto-escalation whether a remaining employee who is below the auto-escalation cap enters or leaves auto-escalation each year.[12] We impose a cash leakage rate upon simulated employment separation using the same procedure as previously described. Finally, to make simulation noise negligible, we run 100 simulations for each individual who does not separate from employment by 60 months of tenure, and we consider the average outcome across these 100 simulations to be the "observation" for this individual.

Conversion to equivalent constant contribution rate

The final outcome of interest for each individual is the constant monthly contribution rate c^* during his tenure at the firm that would result in the same final post-leakage synthetic balance at employment separation (computed from actually observed data or partially simulated data), b_T.

[11] We estimate this rate as the number of employees who depart the firm in that period divided by the total number of employee-months in which employees were working at the firm in that period.

[12] At firms where the difference between the initial default contribution rate and the auto-escalation cap is less than 5%, let T equal this difference (in integer units). At other firms, let $T = 5$. Define a "prompt" as a date on which one could potentially auto-escalate. We estimate separately for each firm, for employees below the auto-escalation cap, the probability of not auto-escalating on your Tth prompt conditional on having auto-escalated on your $(T-1)$th prompt, and the probability of auto-escalating on your Tth prompt conditional on not auto-escalating on your $(T-1)$th prompt. At Firm G, due to missing contribution rate data, we use the conditional probability of entry or exit at the sixth prompt for the control cohort. Note that we do not directly observe whether an employee is enrolled in auto-escalation; we infer this status from whether her contribution rate rises by 1% at the prompt.

Using the equations $b_1 = c^*$ and $b_t = 1.05^{1/12} b_{t-1} + c^*$, we get the expression for the "equivalent constant contribution rate":

$$c^* = \frac{1 - 1.05^{1/12}}{1 - 1.05^{T/12}} b_T \tag{9}$$

III. Results

The first row of Table 4 shows treatment effects on equivalent constant contribution rates that are naively extrapolated from contribution rates observed up to one year of tenure, ignoring leakage. This approach parallels how early pioneering papers in this literature (Madrian and Shea, 2001; Benartzi, Peleg, and Thaler, 2012) were interpreted (e.g., Benartzi and Thaler, 2013). The month 12 contribution rate of any individual still employed at the company at month 12 is assumed to remain in effect until month 60. If the firm implemented auto-escalation and the employee has increased his contribution rate by 1% on the first escalation date[13], we assume that he will continue auto-escalating on schedule through month 60 or until the auto-escalation cap is hit. Employees who don't auto-escalate on the first escalation date are kept off auto-escalation through month 60. We assume that all matching contributions are 100% vested. Constant equivalent constant contribution rates are computed using contribution rates (actually observed until month 12 and extrapolated after month 12) either until employment separation (for those who leave the firm before month 12) or month 60.

Using this extrapolated measure, we estimate that at the three automatic enrollment firms (A, B, and C), the automatic policy increases the equivalent constant contribution rate by 1.2%, 1.6%, and 1.0% of income per period. (Recall that these estimates include the effect of the employer match.) Figure 2 shows that at Firm A, the percent of active employees (i.e., those still employed at the firm) at the automatic enrollment default increases by 68 percentage points for the treatment cohort relative to the control cohort in tenure month 4 (the first month of 401(k) eligibility). However, the impact on 401(k) accumulation is muted because Firm A's default contribution rate is a low 2%, and by tenure month 4, about 20% of the cohort has already left the firm and thus is never subject to automatic enrollment. At Firms B and C, the increase in those contributing at the automatic enrollment default right after the opt-out deadline is significant but

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[13] For employees at Firms D and E whose first escalation date does not occur until their second year of tenure, we use whether they escalated on that first date—even though it occurs after tenure month 12—as the basis for our extrapolation.

smaller—29 and 51 percentage points, respectively—because the default equals the match threshold, which many employees choose as a contribution rate anyway in an opt-in regime, and participation rates under the opt-in regime are higher at these firms than at Firm A (see Figure 3). By the time automatic enrollment has fully kicked in at Firm C (month 3), about a quarter of the treatment cohort has left the firm. Attrition is much lower at Firm B (about 5% by month 4), and thus plays only a minor role in attenuating the treatment effect.

Auto-escalation alone added on top of pre-existing automatic enrollment at Firms D and E has a smaller effect of 0.9% of income per year. The largest effect sizes are at Firms F, G, and H, which simultaneously introduced both automatic enrollment and auto-escalation: 2.9%, 3.7%, and 1.8% of income per year.

Why is the effect size so anemic at Firms D and E? The first two graphs in Figure 4 show the take-up of auto-escalation at these firms. Because we do not directly observe an individual's auto-escalation election, we count an individual as having taken up auto-escalation if her contribution rate increases by 1% on the escalation date. This method will generate some false positives, but we can get a sense of the false positive rate by using the same method on the control cohort. Only 49% of the Firm D treatment cohort and 32% of the Firm E treatment cohort that is still actively employed on the first escalation date accept the auto-escalation default. These percentages are much higher than for the control cohort (3% and 0%, respectively), but far below the 85% acceptance rates reported by Benartzi, Peleg, and Thaler (2012).

The especially large effect sizes at the autoenrollment and escalation firms F and G are to a great extent due to the fact that the control cohorts at those firms have very low participation rates (see Figure 3), so the automatic enrollment default has a big positive impact on average contribution rates (see Figure 5). Take-up of the auto-escalation default on the first escalation date is similar to the rates at Firms D and E—37% at Firm F and 63% at Firm G. On the other hand, take-up of auto-escalation at Firm H is a minuscule 6%.

The second row of Table 4 contains treatment effect estimates in the sample of employees that remain at the firm for at least 60 months, again assuming that matches are 100% vested and there is no leakage. Behavior beyond month 60 is simulated as described in Section II.B. Except at Firm C, the estimates are all smaller than the treatment effects extrapolated from the first 12 months. Figure 5 gives insight into why. At two of the three automatic escalation firms (A and B), the control cohort raises its contribution rate more quickly than the treatment cohort, a phenomenon that has also been documented by Choi et al. (2004) and Choukhmane (2023). At the default auto-escalation firms, the treatment cohort raises its contribution rate over time, but the control cohort raises its contribution rate almost as quickly. Indeed, acceptance of the auto-escalation default erodes considerably with each passing escalation date (see Figure 4). In sum, the assumption that the gap in contribution rates between treatment and control cohorts will remain constant at automatic enrollment firms and grow at auto-escalation firms turns out to be false.

In the third row of Table 4, we show the treatment effect estimate for the entire sample of employees. We still assume no leakage but use the employee's final vesting percentage for the employer match instead of 100%. (The vesting adjustment barely makes a difference in practice.) We also simulate behavior beyond month 60 for those who are still at the firm at month 60. Although these treatment effects are smaller than the extrapolated treatment effects at all companies except for Firm B, there is no clear pattern in their size relative to the treatment effect among employees who remain at the firm until month 60.

The fourth row of Table 4 includes leakage in the treatment effect estimate for all employees. Relative to when we ignore leakage, the treatment effect shrinks by 59% on average. The average treatment effect is 0.5% of income per period among the autoenrollment firms, 0.3% of income per period among the auto-escalation firms, and 0.7% of income per period among the autoenrollment and autoescalation firms.

Table 5 shows the average cash leakage rate that is applied to members of each cohort at each firm. Averaging across cohorts and firms equally, the leakage rate is 42%. If every individual's accumulation were shrunk by 42% at separation, the net-of-leakage treatment effect would mechanically shrink relative to the no-leakage treatment effect by 42%. In reality, the treatment effect shrinks by more because at every firm, the average leakage rate of the treatment cohort exceeds the control cohort. We previously saw in Table 3 that conditional on balance at separation, the leakage rates do not differ greatly between the treatment and control cohorts. Therefore, the fact that the cash leakage rate is higher for the treatment cohort indicates that treatment individuals are more likely than the control cohort to accumulate small positive balances, which then leak at a higher rate.

The final two rows of Table 4 show treatment effects for all employees with and without leakage, controlling for gender, a quadratic function of age at hire, gender, and the log of first observed real salary. The samples in these rows differ from those in the prior rows because some employees are missing salary data. Even after controlling for observable differences between cohorts, the average shrinkage in the treatment effect when going from ignoring leakage to accounting for leakage is 47%.[14] The average treatment effect net of leakage is 0.6% of income per period among the autoenrollment firms, 0.6% of income per period among the auto-escalation firms, and 0.8% of income per period among the autoenrollment and auto-escalation firms.

IV. Conclusion

Automatic savings policies have been widely adopted in part because of the strength of the empirical evidence that they increase retirement savings accumulation. We show that the strength of the evidence has been overstated. Although we do find that automatic savings policies have a positive impact on savings, the effects are modest after taking into account the steeper increase in savings over time by those who are not subject to automatic policies, high employee turnover rates, the high rate of cash leakage upon job separation, and the low acceptance of automatic escalation defaults. Automatic savings policies are highly cost-effective from an impact to cost ratio perspective (Benartzi et al., 2017). But if policymakers wish to effect large changes in savings rates, compulsory savings may be a more effective tool (Chetty et al., 2014; Beshears et al., 2023).

Appendix

Appendix Figure 1 shows that the cumulative cash leakage rate swiftly approaches its asymptote after job separation among treatment and control cohort employees in all firms except Firm G who separate in a July, August, or September.

We construct the cumulative cash leakage rate variable as follows in order to bound its value between 0% (if no cash withdrawals are taken) and 100% (if the entire 401(k) balance is taken as a cash withdrawal) despite the fact that withdrawals can happen at various dates, there are capital gains and losses between those dates, and we only observe 401(k) balances at each year-end.

Define the cash leakage rate of individual i on day $d \in \{0, 1, 2, \ldots, 365\}$ of year y as

[14] At Firm D, the treatment effect after controlling for observables increases slightly when moving from the no-leakage to the with-leakage estimates. This surprising result is driven by the fact that Firm D employees who are missing salary data are overwhelmingly people in the treatment cohort with high leakage rates because they are employed at Firm D for only three months on average. Restricting the sample to employees with salary data but not regression-adjusting, the no-leakage treatment effect is 0.69% and the with-leakage treatment effect is 0.75%.

$$l_{idy} = \frac{w_{idy}}{B_{iy} + R_{iy} + W_{iy}} \tag{A1}$$

where w_{idy} is the dollars disbursed as a cash withdrawal to i on day d of year y, B_{iy} is i's (actual, not synthetic) 401(k) balance at the end of year y, R_{iy} is the total dollars withdrawn by i as rollovers in year y, and W_{iy} is the total dollars of cash withdrawals by i in year y. The denominator $B_{iy} + R_{iy} + W_{iy}$ is what i's 401(k) balance would be at the end of y if there were no leakage or capital gains during y. Similarly, define the rollover rate as

$$r_{idy} = \frac{\rho_{idy}}{B_{iy} + R_{iy} + W_{iy}} \tag{A2}$$

where ρ_{idy} is the dollars disbursed as a rollover to i on day d of year y.

In the calendar year of separation, i's cumulative cash leakage rate from his day of separation s through day $d \geq s$ is

$$\mathcal{L}_{idy} = \sum_{t=s}^{d} l_{ity} \tag{A3}$$

and the cumulative rollover rate is

$$\mathcal{R}_{idy} = \sum_{t=s}^{d} r_{ity} \tag{A4}$$

In subsequent years, the cumulative cash leakage rate is defined as a weighted sum of the cumulative cash leakage rate at the end of the prior calendar year and the sum of the daily cash leakage rate in the current calendar year:

$$\mathcal{L}_{idy} = \mathcal{L}_{i,365,y-1} + \left(1 - \mathcal{L}_{i,365,y-1} - \mathcal{R}_{i,365,y-1}\right) \sum_{t=1}^{d} l_{ity} \tag{A5}$$

The value of $\mathcal{L}_{idy}$ is bounded between 0% and 100%. The cumulative rollover rate is similarly defined as

$$\mathcal{R}_{idy} = \mathcal{R}_{i,365,y-1} + \left(1 - \mathcal{L}_{i,365,y-1} - \mathcal{R}_{i,365,y-1}\right) \sum_{t=1}^{d} r_{ity} \tag{A6}$$

If there are never any capital gains or losses, then $\mathcal{L}_{idy}$ equals the sum of cash distribution dollars from separation to day d of year y divided by 401(k) balances at separation. If there are capital gains or losses and withdrawals always happen at calendar year-ends, then $\mathcal{L}_{i,365,y}$ is the amount that withdrawn cash would have been worth if left to appreciate inside the 401(k) until the end of y divided by what the 401(k) balance would have been at the end of y in the absence of withdrawals and rollovers.

To see why the above claims are true, let year 0 be the calendar year of job separation. Cumulative cash leakage in year 1 is

$$\mathcal{L}_{id1} = \mathcal{L}_{i,365,0} + \left(1 - \mathcal{L}_{i,365,0} - \mathcal{R}_{i,365,0}\right) \sum_{t=1}^{d} l_{it1} \tag{A7}$$

$$= \frac{W_{i0}}{B_{i0} + R_{i0} + W_{i0}} + \frac{B_{i0}}{B_{i0} + R_{i0} + W_{i0}} \sum_{t=1}^{d} \frac{w_{id1}}{B_{i1} + R_{i1} + W_{i1}} \tag{A8}$$

If there are no capital gains, then $B_{i0} = B_{i1} + R_{i1} + W_{i1}$, so (A2) is equal to

$$\frac{W_{i0} + \sum_{t=1}^{d} w_{it1}}{B_{i0} + R_{i0} + W_{i0}} \tag{A9}$$

which is the sum of all cash withdrawals from separation to day d of year 1 divided by 401(k) balance at separation.

If 401(k) balances at the end of year 0 have a gross return of g_1 during year 1 and transactions during years 0 and 1 only occur at the very end of the year, then $B_{i0}g_1 = B_{i1} + W_{i1} + R_{i1}$. In this case, the expression from (A2) for $\mathcal{L}_{i,365,1}$ is equal to

$$\frac{W_{i0}g_1 + W_{i1}}{(B_{i0} + R_{i0} + W_{i0})g_1} \tag{A10}$$

which is what cash withdrawals from separation to the end of year 1 would be worth at the end of year 1 if they had been left in the 401(k) divided by what the 401(k) would have been worth at the end of year 1 in the absence of cash withdrawals and rollovers.

The expressions for cumulative cash leakage in subsequent years have the same interpretations. In the absence of capital gains, the expression for $y > 1$ is

$$\frac{\sum_{v=0}^{y} W_{iv} + \sum_{t=1}^{d} w_{it1}}{B_{i0} + R_{i0} + W_{i0}} \tag{A11}$$

and with capital gains is

$$\frac{\sum_{v=0}^{y-1} W_{iv}\left(\prod_{q=v+1}^{y} g_q\right) + W_y}{(B_{i0} + R_{i0} + W_{i0}) \prod_{v=1}^{y} g_v} \tag{A12}$$

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Table 1. 401(k) plan features

Firm	Policy intro date	Default contribution rate	Employer match and nonelective contributions	Employer contribution vesting schedule
Panel A: Automatic enrollment firms				
A	Jul 1, 2005	2%	100% of first 4% of income contributed	100% immediately
B	Apr 1, 2008	4%	100% of first 4% of income contributed	100% immediately
C	Jun 2, 2006	5%	75% of first 2% of income contributed, 50% of next 3% of income contributed	0% before tenure year 4, 100% at tenure year 4
Panel B: Automatic escalation firms				
D	Jan 1, 2011	6% initial, escalate up to 15%	100% of first 6% of income contributed	100% immediately
E	Jan 1, 2011	3% initial, escalate up to 10%	100% of first 6% of income contributed for most employees. 50% or 75% match rate for ~15% of employees in 2010-2011, ~5% in 2012-2017	0% before tenure year 2, 40% at tenure year 2, 60% at tenure year 3, 80% at tenure year 4, 100% at tenure year 5
Panel C: Automatic enrollment and escalation firms				
F	Jan 1, 2010	2% initial, escalate up to 6%	100% of first 6% of income contributed	0% before tenure year 4, 100% at tenure year 4
G	Jan 1, 2006	3% initial, escalate up to 15%	*Before Apr 1, 2008*: 50% of first 3% of income contributed *Starting Apr 1, 2008*: 100% of first 3% of income contributed*	0% before tenure year 4, 100% at tenure year 4
H	Jul 1, 2007	3% initial, escalate up to 6%	*Before Apr 1, 2009*: 80% of first 6% of income contributed if tenure < 5 years. 100% of first 6% of income contributed if tenure ≥ 5 years *Starting Apr 1, 2009*: 100% of first 6% of income contributed, plus 1% of income nonelective contribution. About 5% of employees have 7% match threshold.	0% immediately, 20% at tenure year 1, 40% at tenure year 2, 60% at tenure year 3, 80% at tenure year 4, 100% at tenure year 5

* A small number of Firm G employees had a match threshold between 4% and 9% during the sample period. Most significantly, a match threshold of 4% applied to about 5% of employees in 2010 and about 10% in 2011.

Table 2. Hire cohort characteristics

The top two sections show hire dates (in days relative to the firm's automatic policy introduction), average characteristics, attrition rates, and employee counts in the control and treatment cohorts. Salary (deflated to the control cohort's hire year dollars) is measured at hire when available; otherwise, it is the employee's first observed salary if one is available, or the median first-observed salary of everybody in the firm hired within 365 days before or after the policy implementation date. The penultimate section shows *p*-values from tests of equality across the two cohorts. The final section shows the fraction of employee-months where contribution rates are imputed and the fraction of employees whose salary is imputed to be the firm-wide median salary.

	Autoenrollment firms			Auto-escalation firms		Autoenrollment and escalion firms		
	Firm A	Firm B	Firm C	Firm D	Firm E	Firm F	Firm G	Firm H
Control cohort								
Hire dates	[-365, -1]	[-365, -186]	[-270, -1]	[-180, -1]	[-365, -1]	[-365, -1]	[-365, -276]	[-270, -1]
Age at hire	29.8	32.1	35.2	35.6	37.2	33.0	35.6	34.1
Female	65.5%	50.4%	32.2%	20.4%	23.5%	60.7%	77.7%	39.1%
Salary	$27,586	$57,307	$46,680	$40,290	$59,671	$32,295	$32,149	$59,069
5-year attrition	82.0%	55.5%	82.5%	51.9%	67.7%	78.0%	74.7%	43.0%
Employees	13,275	411	171	1,293	5,137	1,697	8,695	956
Treatment cohort								
Hire dates	[0, 364]	[0, 179]	[0, 269]	[0, 179]	[0, 364]	[0, 364]	[0, 89]	[0, 269]
Age at hire	29.7	31.3	36.0	34.8	37.2	32.0	35.5	33.9
Female	66.8%	44.1%	32.0%	19.1%	23.0%	58.9%	78.4%	37.0%
Salary	$27,247	$61,831	$40,159	$39,732	$64,706	$34,215	$34,992	$58,712
5-year attrition	80.1%	50.6%	87.6%	53.8%	62.4%	79.9%	74.6%	44.7%
Employees	13,691	263	194	1,761	7,029	2,388	9,377	906
Tests of equality between cohorts								
Age at hire	0.599	0.286	0.451	0.062	0.786	0.005	0.762	0.622
Female	0.018	0.113	0.967	0.360	0.483	0.254	0.317	0.341
Salary	0.251	0.150	0.059	0.561	< 0.001	0.304	< 0.001	0.817
5-year attrition	< 0.001	0.214	0.170	0.289	< 0.001	0.127	0.847	0.458
Imputed or missing observations								
Contrib. imputed	3.6%	0.2%	0.0%	0.1%	1.4%	0.8%	11.1%	0.6%
Salary imputed	3.7%	4.5%	17.5%	7.2%	6.6%	6.4%	14.3%	5.6%
Salary never observed	25.1%	10.7%	33.2%	9.7%	15.2%	16.7%	45.2%	11.2%

Table 3. Cash leakage rates by 401(k) balance at employment separation

This figure shows the cash leakage rate in the year of employment separation by 401(k) balance at separation. The sample is treatment or control cohort employees who separated in July, August, or September at all firms except Firm G. Standard errors are shown in parentheses.

Balance at separation	Treatment cohort	Control cohort	*p*-value of difference
\$0 - \$999	0.948	0.910	0.044
	(0.005)	(0.018)	
\$1,000 - \$4,999	0.473	0.434	0.220
	(0.016)	(0.027)	
\$5,000 - \$9,999	0.447	0.511	0.193
	(0.028)	(0.040)	
\$10,000 - \$19,999	0.333	0.337	0.942
	(0.027)	(0.039)	
≥ \$20,000	0.147	0.115	0.196
	(0.016)	(0.018)	

Table 4. Automatic policy treatment effects on equivalent constant contribution rates

Standard errors are shown in parentheses.

	Autoenrollment firms			Auto-escalation firms		Autoenrollment and escalation firms		
	Firm A	Firm B	Firm C	Firm D	Firm E	Firm F	Firm G	Firm H
Extrapolation from all employees at 12 months, assume 100% vesting, no leakage	1.23 (0.05)	1.56 (0.51)	1.01 (0.65)	0.93 (0.18)	0.92 (0.11)	2.85 (0.16)	3.73 (0.06)	1.75 (0.27)
Employees active at 60 months, assume 100% vesting, no leakage	0.77 (0.13)	1.31 (0.66)	2.82 (0.96)	0.43 (0.25)	0.59 (0.16)	2.24 (0.33)	2.91 (0.11)	0.67 (0.32)
All employees, no leakage	0.82 (0.05)	1.70 (0.51)	0.61 (0.51)	0.52 (0.18)	0.65 (0.10)	1.49 (0.16)	2.19 (0.06)	1.12 (0.28)
All employees, with leakage	0.24 (0.04)	1.09 (0.44)	0.02 (0.36)	0.20 (0.17)	0.45 (0.09)	0.44 (0.13)	1.02 (0.05)	0.51 (0.25)
All employees, no leakage, with regression controls	1.01 (0.05)	1.39 (0.50)	1.60 (0.57)	0.61 (0.17)	0.74 (0.10)	1.86 (0.16)	2.50 (0.08)	0.79 (0.27)
All employees, with leakage and regression controls	0.31 (0.04)	0.70 (0.42)	0.68 (0.45)	0.69 (0.15)	0.55 (0.10)	0.80 (0.13)	1.47 (0.07)	0.11 (0.24)
Control cohort average equivalent constant contribution rate with leakage	1.30 (0.03)	5.72 (0.26)	2.35 (0.26)	7.74 (0.13)	5.31 (0.07)	2.45 (0.10)	1.44 (0.03)	6.61 (0.17)

Table 5. Average cash leakage rates by cohort

This table shows the average cash leakage rate applied upon separation by cohort. Standard errors are shown in parentheses.

	Autoenrollment firms			Auto-escalation firms		Autoenrollment and escalation firms		
	Firm A	Firm B	Firm C	Firm D	Firm E	Firm F	Firm G	Firm H
Treatment cohort	0.658	0.288	0.612	0.331	0.346	0.627	0.542	0.296
	(0.003)	(0.012)	(0.025)	(0.005)	(0.003)	(0.007)	(0.004)	(0.008)
Control cohort	0.484	0.272	0.517	0.318	0.343	0.455	0.398	0.231
	(0.004)	(0.011)	(0.026)	(0.007)	(0.004)	(0.010)	(0.005)	(0.008)
Difference	0.174	0.016	0.095	0.012	0.003	0.172	0.144	0.065
	(0.005)	(0.016)	(0.036)	(0.009)	(0.005)	(0.012)	(0.006)	(0.011)

Figure 1. Cash leakage rate in year of employment separation, by 401(k) balance at separation

This figure shows the cash leakage rate in the year of employment separation by 401(k) balance at separation. The sample is treatment and control cohort employees who separated in July, August, or September at all firms except Firm G. The horizontal position of each data point indicates the center of its balance bin. Whiskers denote 95% confidence intervals.

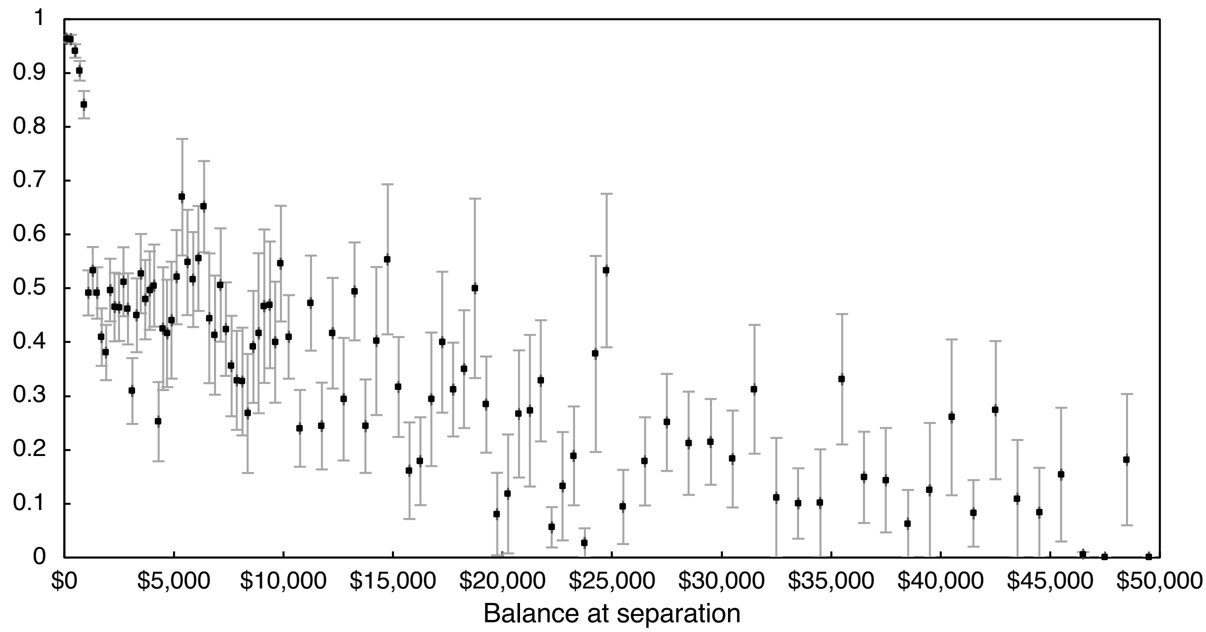


Figure 2. Percent of active employees who are on default contribution path

These graphs show the percent of active (i.e., still employed at the firm) employees whose contribution rate matches the contribution rate of a completely passive employee in the automatic enrollment regime.

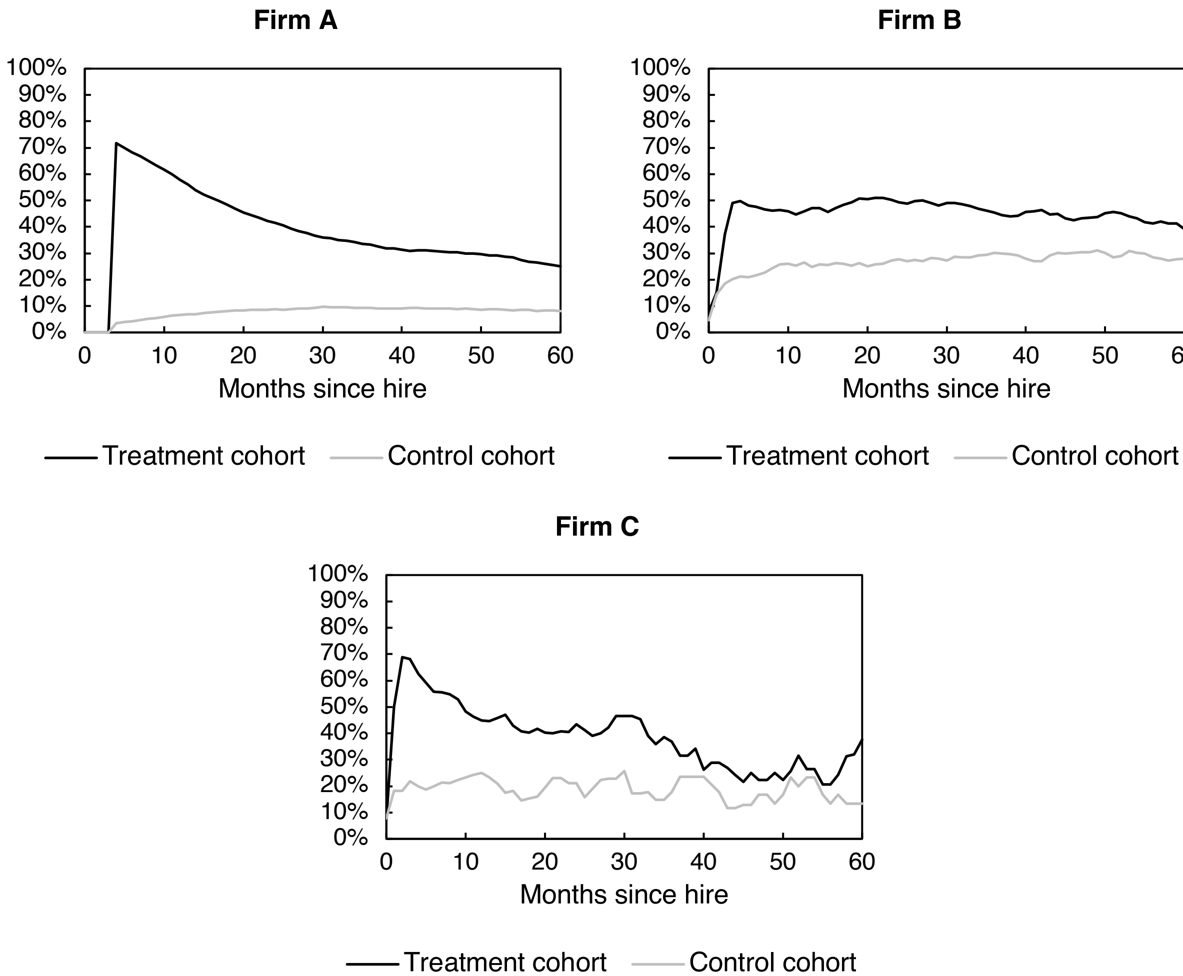
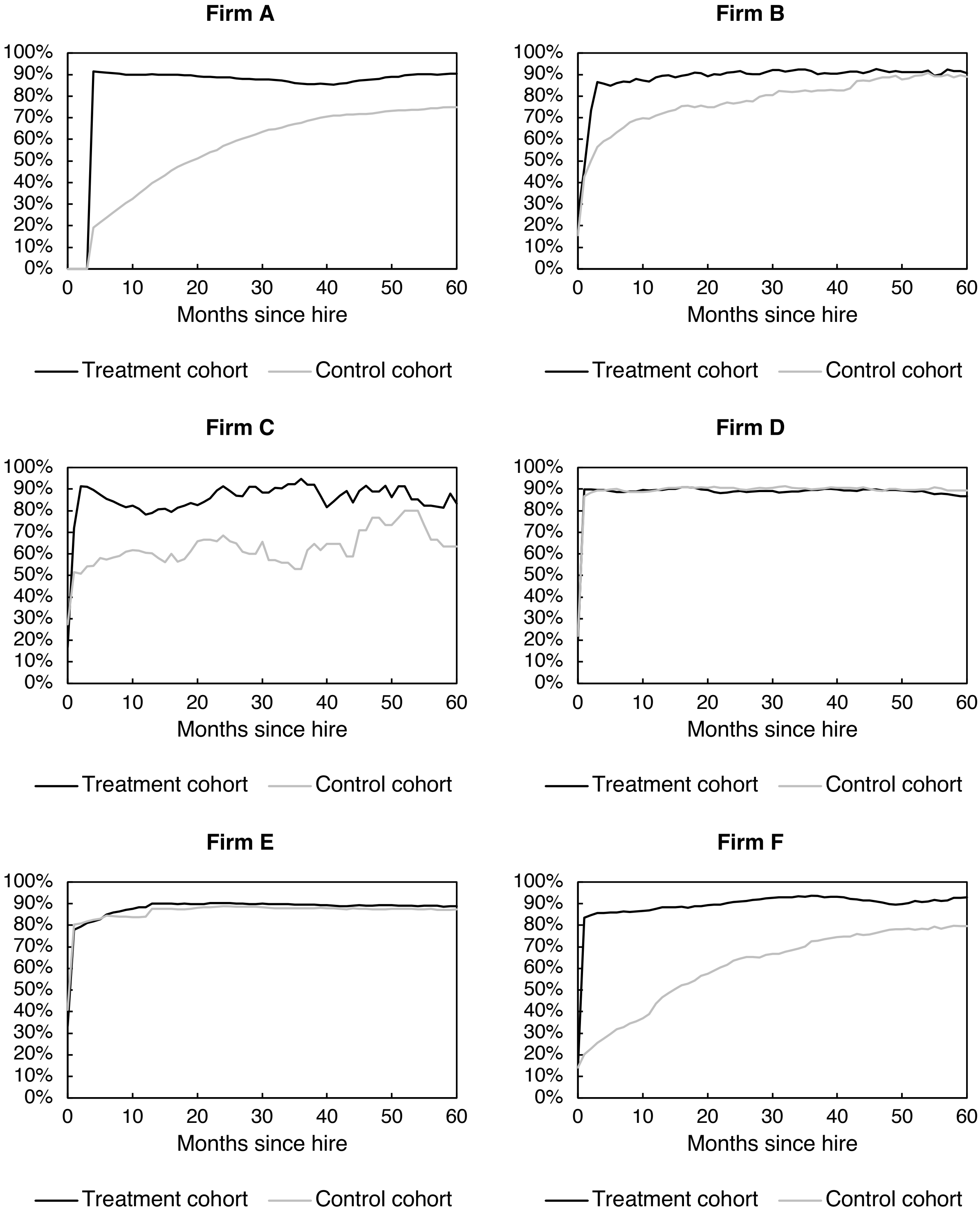
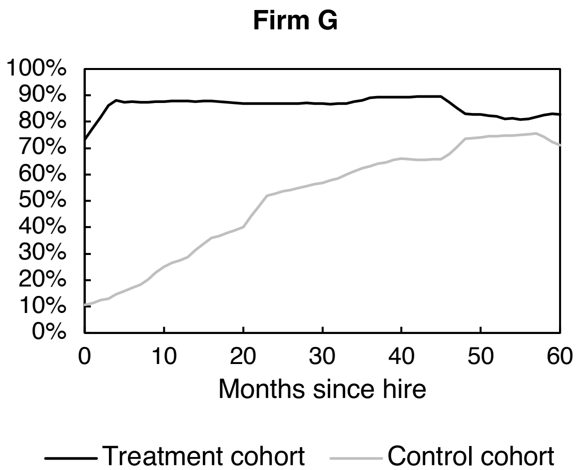


Figure 3. Percent of active employees with a positive contribution rate




Firm G
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
10
20
30
40
50
60
Months since hire
Treatment cohort
Control cohort

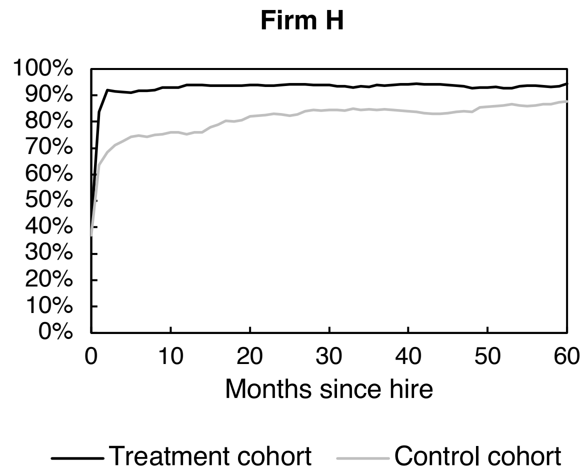

Firm H
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
10
20
30
40
50
60
Months since hire
Treatment cohort
Control cohort

Figure 4. Percent of active employees who auto-escalate

At Firm G, take-up of auto-escalation is not measured in years 3 and 4 for the treatment cohort and years 4 and 5 for the control cohort because we are missing all 2009 contribution rates.

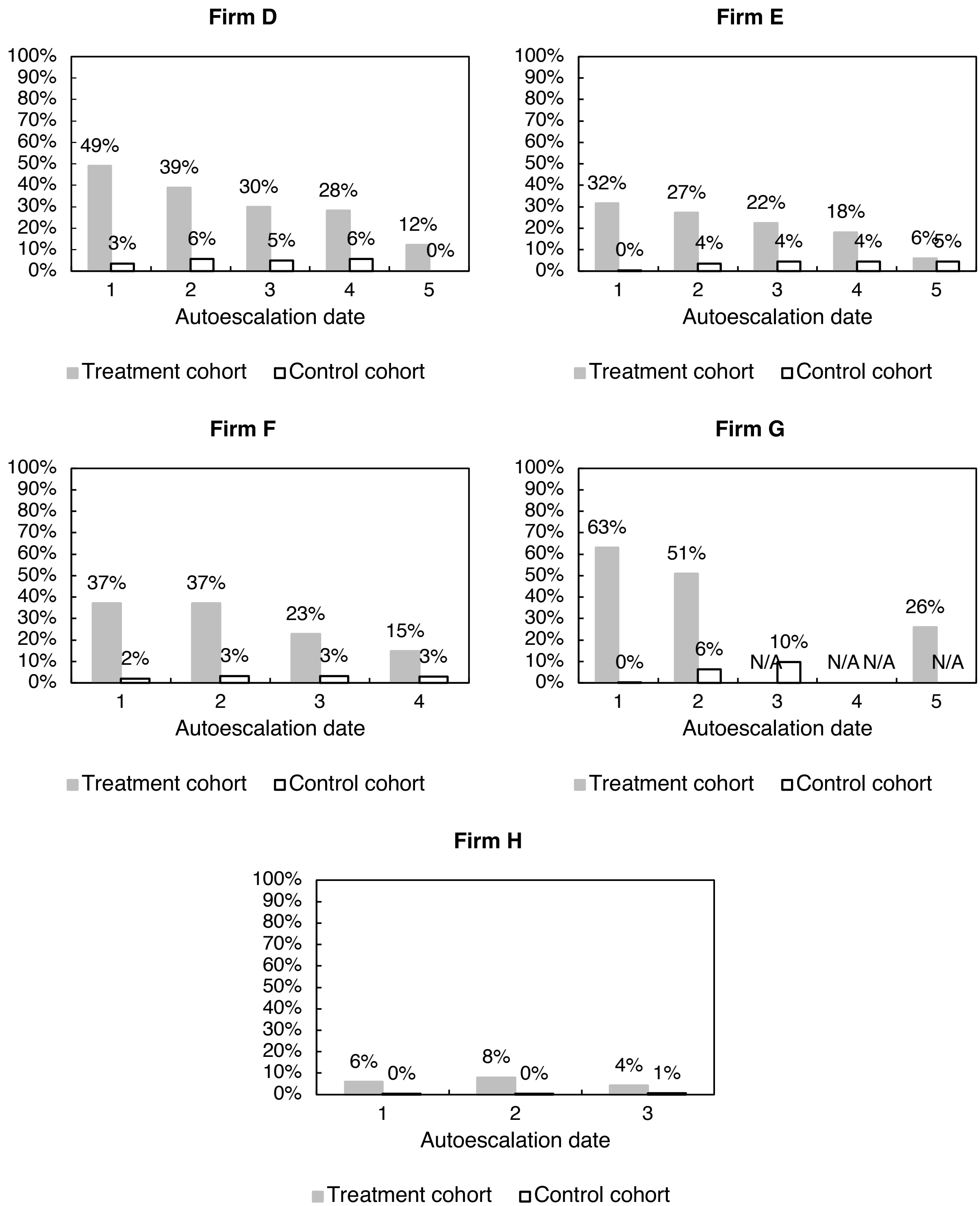
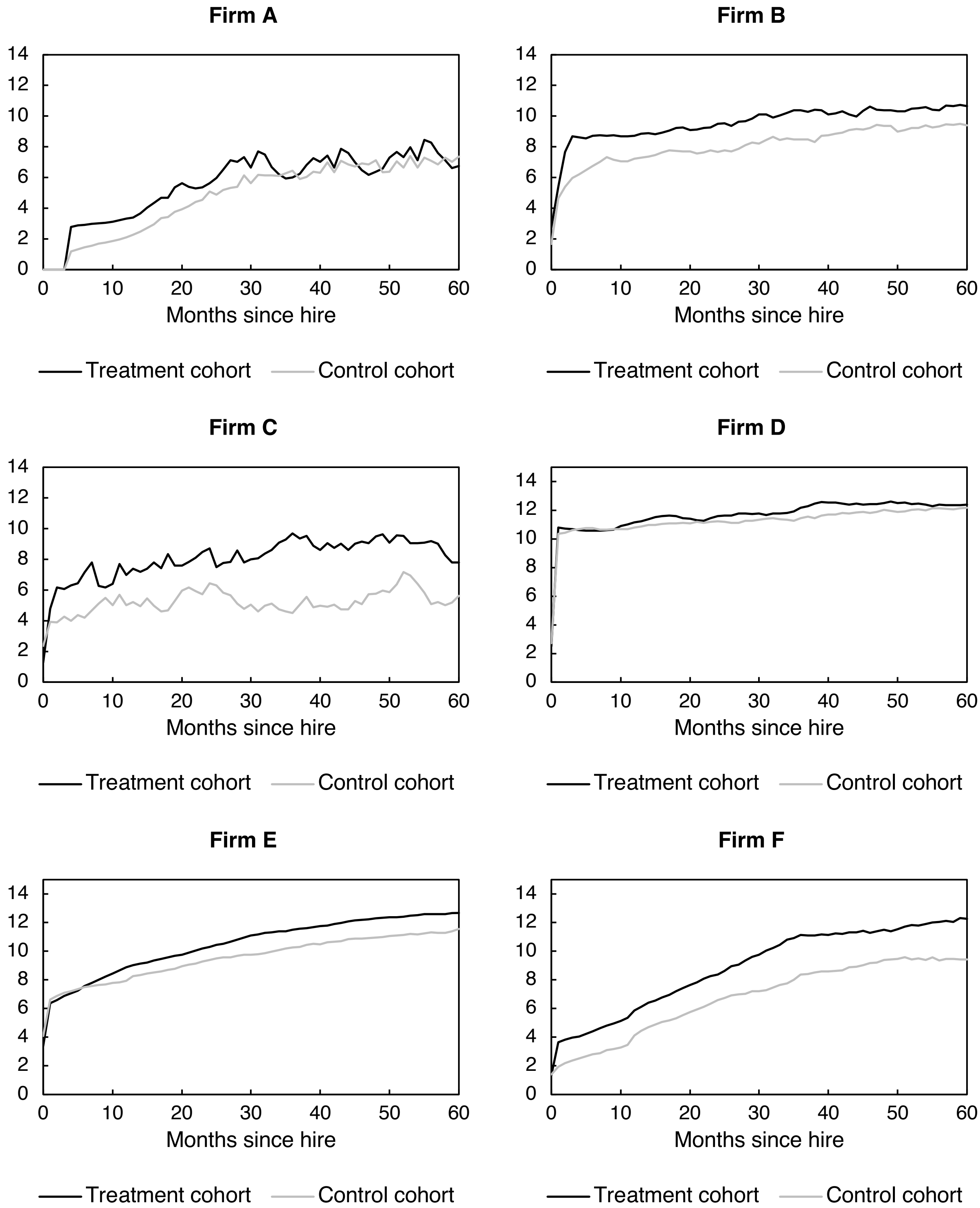
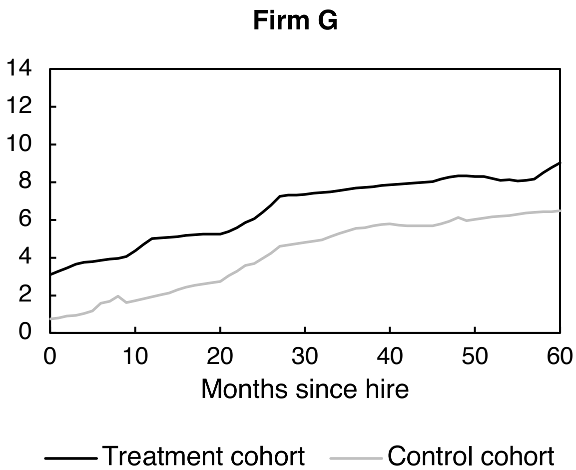
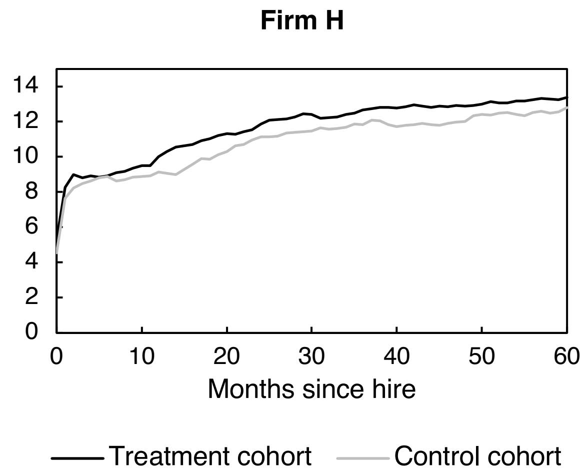


Figure 5. Average employee plus match contribution rate among active employees




Firm G
14
12
10
8
6
4
2
0
0
10
20
30
40
50
60
Months since hire
Treatment cohort
Control cohort


Firm H
14
12
10
8
6
4
2
0
0
10
20
30
40
50
60
Months since hire
Treatment cohort
Control cohort

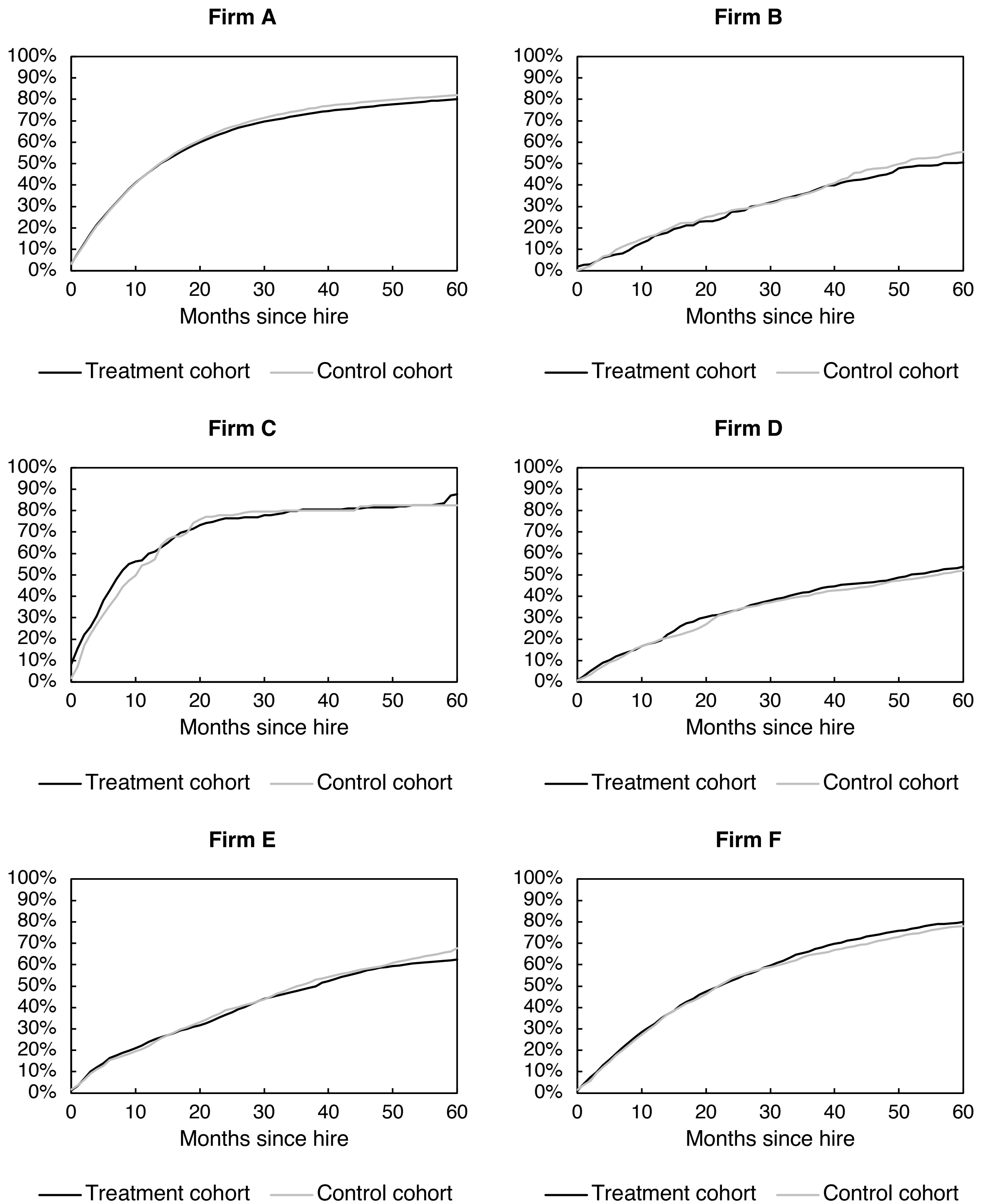
Appendix Table 1. Cash leakage rates by age and 401(k) balance at employment separation

This figure shows the cash leakage rate in the year of employment separation by 401(k) balance and age at separation. The sample is treatment and control cohort employees who separated in July, August, or September at all firms except Firm G. Standard errors are shown in parentheses.

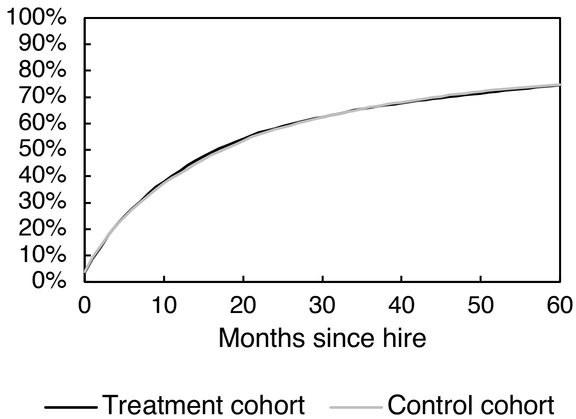
	Age				
Balance at separation	0 – 24	25 – 29	30 – 34	35 – 44	≥ 45
\$0 - \$999	0.942	0.952	0.949	0.924	0.959
	(0.006)	(0.010)	(0.014)	(0.015)	(0.014)
\$1,000 - \$4,999	0.456	0.540	0.491	0.400	0.431
	(0.023)	(0.031)	(0.043)	(0.033)	(0.036)
\$5,000 - \$9,999	0.360	0.421	0.506	0.592	0.472
	(0.045)	(0.051)	(0.066)	(0.047)	(0.053)
\$10,000 - \$19,999	0.282	0.358	0.402	0.339	0.316
	(0.045)	(0.050)	(0.060)	(0.052)	(0.044)
≥ \$20,000	0.097	0.114	0.161	0.169	0.125
	(0.026)	(0.029)	(0.037)	(0.027)	(0.021)

Appendix Figure 1. Cumulative attrition rates by time since hire

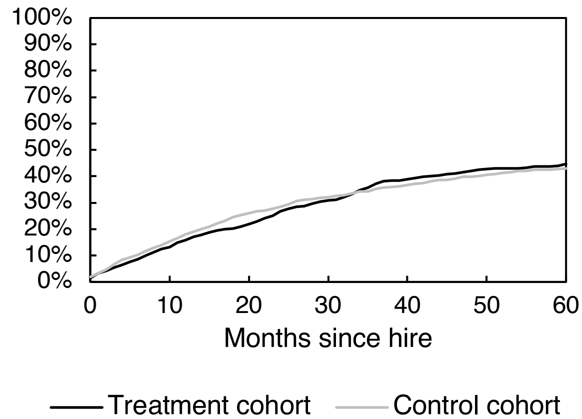
The treatment cohort for firms I and J are employees hired in the 365 days following the automatic policy implementation. The control cohort for firms I and J are employees hired in the 365 days prior to the automatic policy implementation



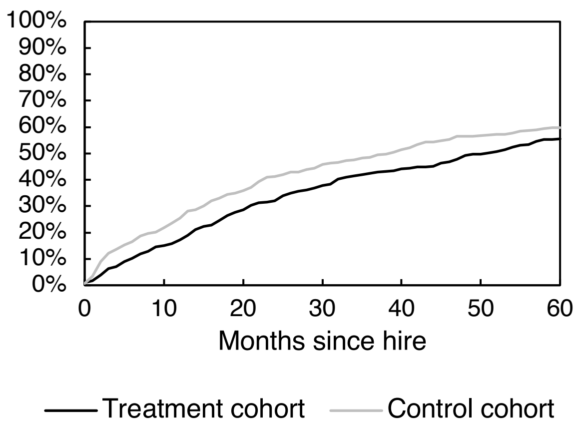
Firm G



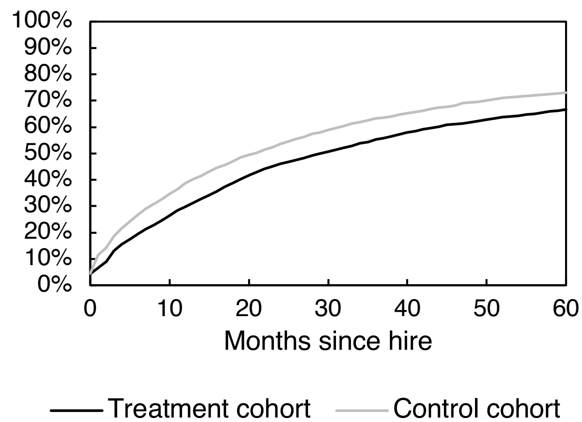
Firm H



Firm I (dropped from study)



Firm J (dropped from study)



Appendix Figure 2. Cumulative cash leakage and rollover rate by days since employment separation

This figure shows, separately for those with 401(k) balances at employment separation below or above \$1,000, the cumulative cash leakage rate and cumulative rollover rate by days since separation. The sample is treatment and control cohort employees who separated in July, August, or September at all firms except Firm G.