Online Appendix for

Efficiency in Household Decision Making:

Evidence from the Retirement Savings of U.S. Couples

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A Data Appendix

This appendix discusses i) the Form 5500 data collection procedure, ii) how we form a crosswalk from the Form 5500 EINs to W-2 EINs, iii) how we calculate foregone match, and iv) how we measure education and parental income.

A.1 Form 5500 Data Collection

Under the Employee Retirement Income Security Act (1974) and the Internal Revenue Code, every retirement plan in the U.S. is obliged to submit an annual 'Report of Employee Benefit Plan' (Form 5500) to the federal government. This form satisfies reporting requirements that plans have to each of the IRS, the Department of Labor and the Pension Benefit Guaranty Corporation. For plans with 100 participants are more, this return must be accompanied by an auditor's report which contains, among much else, a *narrative* description of the retirement plan. For Defined Contribution plans, this description of the plan contains details on the matching schedule (if any), vesting schedule (if any) and auto-features (if any).

All Form 5500 filings since 2003 are publicly available from the Department of Labor.[36] Our process for converting these narrative descriptions into a usable data-set is described below: steps 1 to 3 are automatable; the bulk of the effort is in steps 4 and 5, which involved the hand-coding (and extensive checking) of the data.

1. Step 1 was to download the entire data set: there are are up to half a million retirement plans each year from 2003-2018, and each report can be up to 100 pages in length.

[36] `https://www.dol.gov/agencies/ebsa/about-ebsa/our-activities/public-disclosure/foia/form-5500-datasets`.

2. Step 2 was to form a sample of plans in which to codify the plans. Our sample consists of 6,201 plans, comprised of the largest 4,730 plans, where the plans are ordered according to the mean number of active participants over the period 2003 to 2018 and a random sample of remaining plans (1,471 additional plans).[37]

3. Step 3 was to identify that portion of the text in which the narrative description of the plan starts. This almost always starts with the heading 'Description of Plan' or 'Description of the Plan'. The pages containing the relevant information were extracted from the (much longer) auditor's report. To facilitate the subsequent steps, which involve manually identifying the relevant passages, we highlighted relevant terms (e.g., 'matching', 'vesting', 'auto-enrollment', 'default' etc.).

4. Step 4 was for the files to be read and the relevant text extracted and recorded. The data was codified using a standardized numerical coding system. This was completed by undergraduate Research Assistants[38] for the largest 500 firms, and by external contractors for the remainder of our sample, with queries from the external contractors on individual files answered by the authors and the local research assistants.

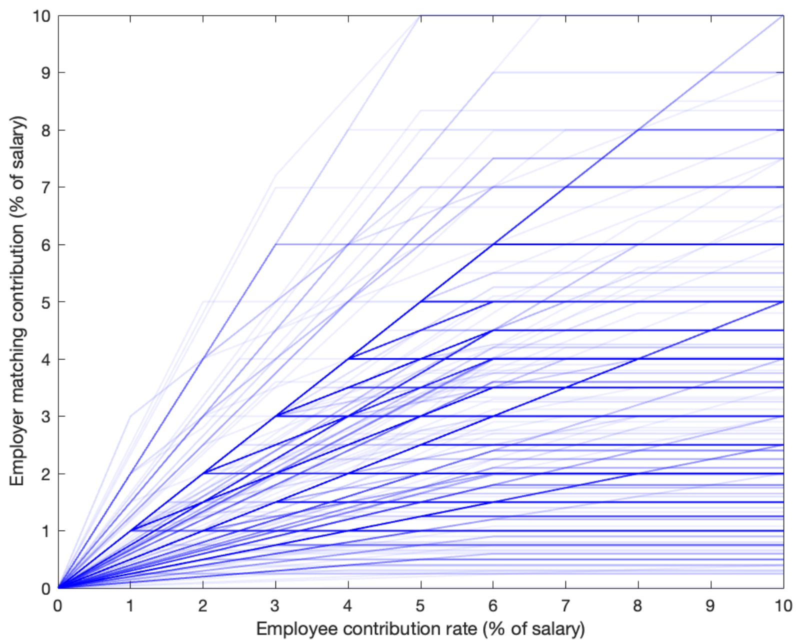
5. Step 5 involved checking and quality control. Any unusual entries were flagged for manual checking. This identified plans where the plan parameters were unusual (very large match rates, for example) or when plan features were coded as changed in one year and reverting the previous year – while in some cases this turns out to be a genuine change and subsequent reversion, this provides a useful check on individual years being miscoded.

The resulting data set (Choukhmane et al. (2025)) is an employer data set with data on 70,282 plan year observations on 6,201 plans. In 60% of these cases, the match schedule was amenable to codification. The remaining 40% of cases involved the plan being too complicated for codification at scale or involved match schedules that differed by class of employee, which would prevent us

[37] The sampling structure was designed to combine a capacity to analyze the behavior of a large number of employees (facilitated by our prioritizing large firms), with the ability to use this data set more generally to work with a representative sample of plans (facilitated by a random sampling of the remaining firms).

[38] We are grateful to Jun-Davinci Choi, Alessa Kim-Panero, Rosa Kleinman and Charlotte Townley who provided excellent research assistance throughout the period over which we collected this data. We are also grateful to Keelan Beirne, Rachel Bitustky, Jasper Feinberg, Albert Gong, Melissa Kim, Maddie Nagle, Liana Wang, Clara Lew-Smith and Kelly Wei who provided excellent research assistance during parts of our period of data collection.

Figure A1: Matching Schedules

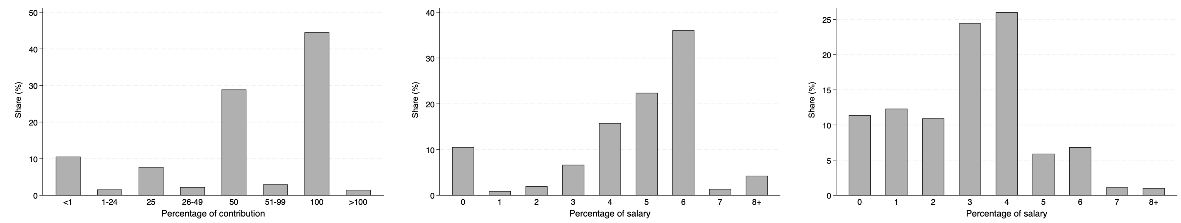


Notes: The sample is all employer match schedules for plans observed in 2015. Each line represents a match schedule and the depth of shade represents the frequency of the match schedule.

from making a clean link between employee behavior and employer plan details. Such plans are not used in our analysis.

Figures A1 plots *all* the matching schedules in our data for 2015, with the intensity of the shading in proportion to how frequently that schedule is observed in our data. To further illustrate the variation in match schedules, Figure A2 summarizes the heterogeneity in match schedules by showing the cross-sectional distribution of three summary measures of match schedules in 2015. These are: i) the 'Match Rate on First Dollar'– the matched contributions that employees receive on their first dollar of contributions, ii) the 'Matching Cap' – the proportion of the employees' salary above which no more matching contributions are offered and iii) the 'Maximum Employer Match' – the matched contribution that the employer makes if the employee fully exploits their match.

Figure A2: Heterogeneity in Match Schedules



(a) Match on first dollar (b) Matching cap (c) Maximum match

Notes: The sample for this figure is all employer match schedules for plans in 2015. Panel (a) summarizes the rate at which employers match the first dollar of employee contributions. Panel (b) summarizes the distribution of the matching cap – the level of employee contribution at which employer contributions are maximized. Panel (c) summarizes the maximum employer match – the employer contribution that would be made on behalf of employees who are fully exploiting their employer match (that is, employees who are contributing at least the level of the matching cap).

A.2 Linking employees to DC plans

The hand-coded plan data described above needs to be linked to data on participant behavior, which we take from tax data, primarily Form W-2 (Internal Revenue Service, 2024; Alexander et al., 2024).

While both Form 5500 and Form W-2 include an Employer Identification Number (EIN), a given employer may (and often does) use a different EIN on their Form 5500 and their Forms W-2. For example, the firm might use the parent company's EIN on their Form 5500, while some subsidiary (or disregarded entity) issues Form W-2.[39]

To overcome this issue, we make use of links implied by Form 8955-SSA, which pension plans file with the IRS. Form 8955-SSA is, effectively, a list of separating employees that have accrued pension benefits that remain in the plan.[40] Importantly for us, firms predominantly use the same EIN on Form 8955-SSA as they do on Form 5500 since they are both filed at the level of the retirement plan. We have access to Forms 8955-SSA filed in 2015; when constructing our panel, we assume that the links identified between the employee EIN and the retirement plan EIN identified by this process are stable across years.

[39]Determining a comprehensive mapping from W-2 EINs to the EIN of the parent company is an arduous process that typically requires substantial hand-coding (Dobridge et al., 2019). Furthermore, this approach might not be appropriate in our setting: a corporate group might have a different plan for employers of different subsidiaries.

[40]Both DC and DB plans file Form 8955-SSA. In our procedure, we restrict attention to Form 8955-SSA observations that indicate a positive DC account balance.

We proceed as follows. Let j denote the EIN as filed on Form 5500 and Form 8955-SSA and let i denote an employee reported on Form 8955-SSA (with $j(i)$ being employee i's plan, as indicated by Form 8955-SSA).[41] Let k denote a given W-2 EIN. We are looking for pairs jk where we can be confident that a given employee working at k is eligible for plan j. First, we identify ik links: that is, for each i in the Form 8955-SSA data, we find all the W-2 EINs $k(i)$ that i separated from at some point between 2014 and 2016.[42] Second, for each j and k, we compute $Pr(j = j(i)|k = k(i), i \in S_{8955})$, where S_{8955} denotes the set of individuals in the Form 8955-SSA data. We define a valid "match" as follows. If the W-2 EIN and the Form 5500 EIN are identical (i.e., if $j = k$), we treat this jj pair as a presumptive match, and delete this match only if $Pr(j = j(i)|k = k(i), i \in S_{8955}) < 0.5$. We impose a higher standard when $j \neq k$: we require $Pr(j = j(i)|k = k(i), i \in S_{8955}) \geq 0.9$ and that there are at least 5 individuals with $k = k(i)$. That is, a link jk is a pair of EINs where separating employees of k that leave their money in their former employer's plan are predominantly doing so in plan j.

We do not require the conditional probability to be exactly one since a given employee might separate from multiple jobs during our measurement period. For instance, person i might separate from two firms k and k', with DC plans j and j' respectively. It is possible that, upon separation, she rolls over the j DC account into an IRA and so we do not observe her in the the Form 8955-SSA data, but she leaves the j' account untouched, meaning that we observe an ij' link but not an ij link – that is, we observe only one of the two true links. This fact pattern would tend to cause $Pr(j = j(i)|k = k(i), i \in S_{8955})$ to be less than one despite j and k being a true match. For this reason, we use the threshold of 0.9. Among our matches, the average conditional probability is 96.4%.

As a final further backstop to ensure that we are matching employees to the correct plan, we compute the total amount of employee contributions in the tax data and we estimate the total number of eligible participants.[43] We then compare these calculations from the tax data to their analogues reported on Form 5500. We drop all plans where either tax moment (estimated number eligible or calculated total contributions) exceeds its analogue on Form 5500. This restriction drops

[41] $j(i)$ could be a multi-valued set.

[42] We restrict to ik links where i made at least $5,000 in DC contributions to k at some point prior to 2015.

[43] For the latter calculation, we restrict to employees age 21 or greater and with at least two prior calendar years of positive earnings with the firm.

cases where employees may in fact be eligible for other DC plans that we do not observe.

Our final dataset in 2015 contains approximately 40% of the plans we initially attempted to code, or 34% weighted by participant count (39% if we include the federal government). 62% of dropped plans (63% of participants) are dropped before attempting to link to the employer due to, for instance, the existence of more than one plan at a given employer. The remaining drops occur due to failures to match to the IRS data.

Once we restrict to our final set of plans, and the W-2 EINs k that correspond to them, we construct our sample. Formally, the unit of observation is the couple i in year t, made up of individuals i_A and i_B, with two employers (i.e., as defined by the Form 5500 EIN) $j(i_A, t)$ and $j(i_B, t)$, where both $j(i_A, t)$ and $j(i_B, t)$ are in our Form 5500 dataset. We allow for the couple to work at the same firm, i.e., $j(i_A, t)$ is allowed to equal $j(i_B, t)$. In the rare event that a given couple has multiple combinations of $j(i_A, t)$ and $j(i_B, t)$ (that is, when at least one member of the couple participates for more than one firm in our dataset in a given year), they appear in our data as separate observations. Our baseline sample of 184,600 observations is comprised of 184,000 unique couples.

A.3 Calculating Foregone Match

For all of the plans that we consider, the employer's matching contributions are a function of the employee's contributions expressed as a proportion of pay. For example, a plan may match employee contributions dollar-for-dollar, up to the first 5 percent of pay. In this case, the employer match, $m(s; Y)$, is equal to $\min(0.05, \frac{s}{Y}) \times Y$, where Y is pay and s is employee contributions.[44] Let α denote $\frac{s}{Y}$ and let $\tilde{m}(\alpha)$ denote the term in $m(\cdot)$ that multiplies Y. This means that matching contributions $m(s, Y)$ are equal to $\tilde{m}(\alpha) \times Y$. Every plan that we code satisfies the property that $\tilde{m}(\alpha)$ is weakly concave – i.e., marginal match rates are weakly decreasing. Additionally, every plan satisfies the property that $\frac{\partial \tilde{m}}{\partial \alpha} = 0$ for large enough α – that is, there is a point (the "match cap") at which marginal contributions are no longer matched.

We observe employee contributions s directly. However, we do not perfectly observe Y and thus we cannot perfectly calculate α. In the administrative tax data, we observe wages; that is, taxable (Form W-2, box 1) wages plus pre-tax DC contributions. We denote this quantity as Y^{obs}.

[44] In Section 2.2, we suppressed the dependence on Y, as we treat Y as fixed.

This differs slightly from Y because Y is computed before subtracting certain tax-preferred payroll deductions, including employee contributions to employer-sponsored health insurance (ESI) and Flexible Savings Accounts (FSAs) – neither of which we reliably observe in the tax data – while Y^{obs} is calculated after subtracting those items.[45] Therefore, we must make a decision on how to translate Y^{obs} into Y.

If we merely assumed that $Y^{obs} = Y$, this measurement error could cause us to calculate erroneously that a couple was forgoing some match when in fact it was not. For example, suppose a given couple is comprised of members a and b, each of whom earned $Y = \$100,000$ and faces a simple matching schedule where the first 5% of pay is matched dollar for dollar. Suppose that a contributes 5% of their pay and b contributes nothing. This is an allocation on the Pareto frontier; while they are not fully exploiting their match, there is no allocation of their existing savings that would increase their match.

But suppose further that a pays a \$5,000 ESI premium, meaning that $Y^{obs} = \$95,000$ for a. We would observe, in this case, that a is contributing (slightly) more than 5% of her pay, while b is contributing nothing. This would no longer be on the Pareto frontier, since a would be estimated to be contributing in a region with a zero marginal match rate, while b has unfilled match (implying a positive marginal match rate). Thus, under the most naive approach, we would deem this to be a foregone match, even though in reality it was not.

To avoid this sort of erroneous conclusion, we proceed using two conservative assumptions. Recall equation (3), repeated below, which defines the foregone match as the optimal match the couple could have received given their chosen aggregate saving S, less the actual match they receive given their individual saving, s^A and s^B:

$$FM = \underbrace{\Big(m^A\big(s^{*A}(S)\big) + m^B\big(s^{*B}(S)\big)\Big)}_{\text{Optimal Match}} - \underbrace{\Big(m^A\big(s^A\big) + m^B\big(s^B\big)\Big)}_{\text{Actual Match}} \tag{4}$$

We first assume, when computing the optimal match of a couple (the first underbraced term in equation (4)) that $Y = Y^{obs}$; that is, we assume no tax-preferred payroll deductions. This will lead us to underestimate Y and therefore overestimate α. Because of the weak concavity of $\tilde{m}(\alpha)$,

[45] Less commonly, Box 1 wages can include amounts that would usually not be included in Y, such as stock options and certain life insurance premiums paid by employers.

a larger α will cause us to calculate a weakly lower average rate at which employee contributions are matched.[46] Together, this will lead us to ***underestimate* the optimal match.** Second, in computing the actual match for a given couple (the second underbraced term in equation (4), we assume that $Y = Y^{obs} + ESI$, where ESI is an assumed level of tax-preferred deductions. Our baseline assumption is that $ESI = \$5,000$. While this is approximately the average level of worker contributions in 2015 for family coverage (see Exhibit A in Kaiser Family Foundation (2015)), it is likely to be higher than the average payment paid by our sample, and so have the effect (on average) of leading us to overestimate Y and thus underestimate α – with the end result being that we will tend to ***overestimate* the actual match.** The reason that $5,000 is likely to be an overestimate is because many individuals have self-only or self-plus-one coverage (rather than family coverage) and because many individuals have no coverage at all (typically because they are on their spouse's plan).

Given that foregone match is defined as the optimal match less actual match, and given our first assumption will weakly bias the former down for everyone and our second assumption will bias the latter up, on average, our approach will cause us to estimate lower incidences of foregone match than exist in reality.

Figure A3 shows the sensitivity of our results (on both our true sample and synthetic samples) varies with assumed ESI in calculating the actual match. The headline proportion is only modestly sensitive to extremely large differences in assumed ESI – the figure shows values in the range of $1,000 to $10,000, while the qualitative pattern that we document, of a substantial wedge between the share with a foregone match in the true sample and in the synthetic samples, is unchanged.

A.4 Additional detail on construction of demographic variables

This section discusses the construction of education and parental income demographics used in Figure 4.

We measure **education** as the distinct calendar years in which an individual receives Form 1098-T. Form 1098-T is required to be issued by eligible education institutions (generally, all accredited postsecondary institutions) to most students. The most substantive exception is that institutions

[46]The total employer match can be written as $\frac{\tilde{m}(\alpha)}{\alpha} \times s$. The weak concavity of $\tilde{m}(\alpha)$ means that $\frac{\tilde{m}(\alpha)}{\alpha}$ is weakly decreasing in α.

are not required to file Form 1098-T for students whose tuition is covered entirely by scholarships. Nevertheless, most institutions voluntarily file Form 1098-T for such students, as Form 1098-T can make it easier for that student to claim tax benefits. Cronin and Gray-Hancuch (2024) find that the vast majority of students receive a Form 1098-T, while the bulk of those missing a Form 1098-T attend public two-year institutions. Because we observe Form 1098-T receipt beginning in 1999, in these columns of Figure 4, we restrict attention (solely for this analysis) to couples where both spouses attained age 18 no earlier than 1999. This reduces the baseline sample size from 184,600 to 22,700; within this restricted sample, the average age is 30.3 and the share with foregone match is 17.7%.

We measure **parental income** by exploiting dependent linkages we observe beginning in 1994. Following Chetty et al. (2014), we assign each individual to the first parent (or set of two parents) that claim them as a dependent. Again following Chetty et al. (2014), we impose an age requirement in order to minimize cases of grandparents claiming a child as a dependent. In particular, for a dependent link to be "valid", we require either (a) the presence of a female claimant between the ages of 15-40 or (b) the presence of a male claimant between the ages of 15-40 and the absence of a female claimant. If a given year involves an "invalid" dependent link, we iterate forward in time until we find a valid one.

We then compute the average income of the parents (at the household level) over a five-year period, using Form 1040. For cohorts younger than 1990, we take the average from 1996-2000, the first five years of Form 1040 income data available to us. For cohorts 1990 and older, we take the average of the years when the child was ages 6 to 10. We measure income at the household (technically, tax unit) level. If an individual is matched to a single parent who subsequently gets married, income includes the income of the original parent as well as the (new) "step-parent" after that marriage takes place. If an individual is matched to married parents who subsequently divorce, we take the sum of their individual incomes from their newly separate Forms 1040. If one of those divorced parents subsequently remarries, we assign half of the new couple's income to the original parent.

In these columns of Figure 4, we restrict attention to couples where we can match both spouses to a parent. We observe dependent linkages beginning in 1994 and most children are claimed as dependents at least through age 17. As a result, we have fairly good match rates (85.5%) for those

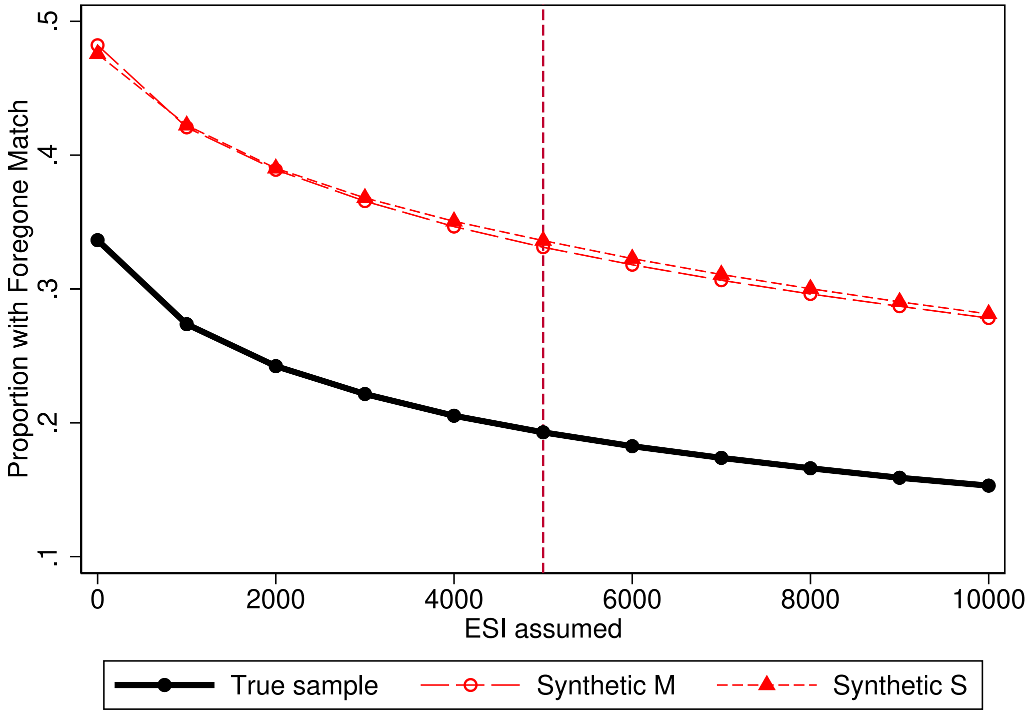


Figure A3: Sensitivity of results to ESI

couples where both spouses are in the 1977 cohort or younger and poor match rates for all other couples (9.2%).

This reduces the baseline sample size from 184,600 to 50,800; within this restricted sample, the average age in 2015 is 34.2 and the share with foregone match is 17.6%.

B Foregone match is not driven by measurement error in plan assignment

In our main empirical exercise, we infer non-coordination when the allocation of savings across spouses does not maximize employee contributions, taking as given the plans of each spouse and the total saving of the couple. If we mismeasured the incentives of each spouse's plan – i.e., if we assigned spouses the wrong match formula – we could erroneously deem a couple that is truly coordinating to be non-coordinating.[47]

In this appendix, we provide additional validation that our results are not driven by such measurement error. First, we show evidence that employee contributions bunch at the assigned match cap, which we would not expect to see if we have systematically misassigned the match cap. Second, we audited a subsample of our data (the largest 50 plans and 100 random plans) and discovered an error or ambiguity in 4.7% of plans representing 2.8% of workers. Third, we find that the probability of foregone match is fairly similar across firms, suggesting that foregone match is not driven by measurement error at a small number of firms. Fourth, we perform a simulation exercise to show that even if *all* individuals were assigned the wrong plan, non-coordination is still required to explain the foregone match that we observe.

The largest of our plans in our data is that of the federal government. We also undertake an audit of the plan features for the different branches of the federal government (identified by all the payer IDs associated with the federal government in an IRS database). An overwhelming share (over 95%) of the federal employees in our sample of federal works are employed in a small number (fewer than 10) of distinct federal payers.[48] We have manually audited and verified that employees in each of these large federal payers were eligible for the match in 2015, and we have done a similar audit for any smaller federal payer with a large incidence of foregone match.

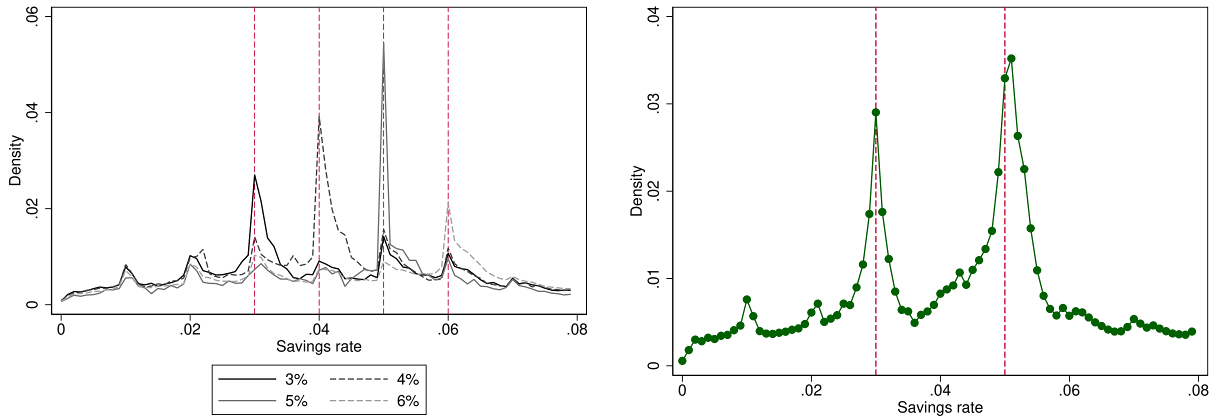
B.1 Bunching is observed at match caps

The cap on employer matching contributions introduces a kink in savers' budget set: contributions up to the cap earn a discontinuoulsy higher rate of return than contributions above the cap. Theory

[47]The potential for apparent misallocation to be observed as a result of measurement error has been documented and studied in multiple other settings– see, for example Gollin and Udry (2021) and Bils et al. (2021).

[48]These statements 'over 95%' and 'fewer than 10' are unavoidably vague for disclosure reasons.

Figure B1: Bunching at match caps



(a) Single-tier plans: caps at 3%, 4%, 5%, and 6%

(b) Federal government plan: kinks at 3% and 5%

Notes: Panel (a) plots the density of contributions as a share of wages for single-tier plans with match caps at 3%, 4%, 5%, and 6% respectively. Panel (b) plots the density of contributions as a share of wages for the two-tier federal government plan, which has kinks at 3% and 5%. In both panels, we use the full population of individuals working for each firm in 2015 and making positive contributions.

predicts that there will be an excess mass of individuals who choose allocations at convex kinks of their budget constraint (Saez (2010)). Because all the match formulas we study are (weakly) concave, every match cap represents such a kink. Therefore, if we have assigned the correct match formulas to the correct individuals, we should expect to see bunching at the match caps.

The left panel in Figure B1 shows the density of contributions as a share of wages in 2015 for single-tier plans with match caps at 3%, 4%, 5%, and 6%. For this analysis, we use the full population – not just the members of married couples included in our sample. There is some bunching at all integer percentage point values, which can be explained by the choice architecture where participants often specify their desired contribution as a proportion of salary. More interestingly, we see the largest amount of bunching at 3% for plans with a match cap at 3%, 4% for plans with a match cap at 4%, and so forth – consistent with our measurement of matching caps. The right panel in Figure B1 shows the analogous density for the two-tier plan of the federal government, with kinks at 3% and 5% of wages. We again see larger bunching at these two kink points than at other integer percentage point values.

Figure B2 presents evidence that this bunching is widespread across firms. We compute the ratio of the density in a pre-defined 'bunching region' (within 0.5 percentage points of the cap) to the density in a 'control region' (which we define as the average of the density in a bin 0.5 to 1.5

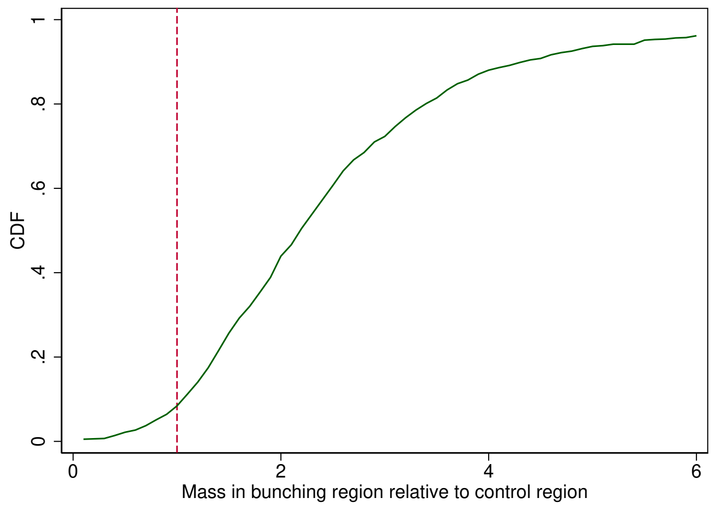


Figure B2: CDF of density in bunching region relative to density in control region

Notes: For a plan with match cap x, the bunching region is defined as $[x - 0.005, x + 0.005]$, while the control region is defined as $[x - 0.015, x - .005) \cup (x + 0.005, x + 0.015]$. The figure includes all single-tier firms in 2015, and the CDF is taken with respect to the unweighted distribution of firms.

percentage points below the cap, and the density in a bin 0.5 to 1.5 percentage points above the cap). A ratio greater than 1 indicate excess mass at the cap. Figure B2 plots the CDF of this ratio across firms with single-tier matching schedules. We find that over 91 percent of the mass is to the right of one – meaning that the vast majority of firms have an excess mass of contributors near the match cap, relative to the density nearby.

B.2 Audit

Our new data set was, as described in Section A.1, constructed by hand-coding a large number of plans at scale. A reasonable concern is that this data is contaminated with error, and that this leads to an over-estimation of the proportion of couples with foregone match.

To assess the extent to which me may have miscoded our data, we implemented an audit. We selected 150 plans for a careful recheck. For this audit, we selected the largest 50 plans, and a random 100 of the remainder plans.

Out of 150 plans, the audit uncovered 2 miscoded plans, and 5 plans which we had coded as having an unambiguous meaning, but which from a second look have been coded as 'too complicated to code at scale'. While we have corrected these specific instances of mis-coding in our data, a reasonable inference is that the proportion that these represent – 4.7% of plans, or 2.8% of workers

– have plans that are miscoded. Sections B.3 and B.4 below will help to put the importance of these rates in context for our result.

B.3 Foregone match is pervasive across employers

If our results were driven by measurement error, we would expect to find that high levels of foregone match are much more frequent in some employers (those we have erroneously coded) than others. Figure B3 plots the CDF of the share of individuals in a couple with foregone match at the firm level.[49] 58 percent of employers have foregone match rates between 15% and 40%. (Due to disclosure protections, we cannot plot the equivalent participant-weighted distribution, which would show the proportion of individuals with a foregone match.)

As a final check, we evaluate how sensitive our results are to dropping employers with a particularly high incidence of foregone match. We do two exercises, both of which involve dropping outlier employers. In the first, we identify the retirement plans where the incidence of foregone match is above the 97.2th percentile (weighted by unique individuals), corresponding to the 2.8% person-weighted miscode rate in our audit. The second approach is more conservative and involves dropping employers where the incidence of foregone match is above the 90th percentile. This involves dropping close to four times more employees than implied by our audit of matching formulas. We show the effect of these two sensitivity exercises on the estimated share of couples with foregone match (in Table B1), and on the estimated foregone match amounts (in Table B2).

Columns (1) and (2) of Table B1 repeat our baseline analysis of the proportion of couples not actively coordinating their contributions from Table 3. It shows that 19.3% of couples are observed with foregone match, which implies a share of couples not actively coordinating of between 57.4% and 58.2%, depending on whether we benchmark against the reshuffled couples or pairs of singles. Under our most conservative assumption, in which we drop employers with an incidence of foregone match above the 90th percentile, the proportion of couples with foregone match falls from 19.3% in our baseline to 16.8%. Similarly, the implied proportion not coordinating falls from between 57.4% and 58.2% in the baseline to between 52.1% and 54.7%, depending on which benchmark we

[49]This analysis is necessarily at the firm level, and not at the couple level. To achieve this, we treat each couple as having two observations – one for each employer – for this purpose. E.g., if a couple works for employers A and B and has foregone match, the foregone match is assigned both to A and B. If both members of the couple work for employer A, this is treated as two observations with foregone match, each attached to employer A.

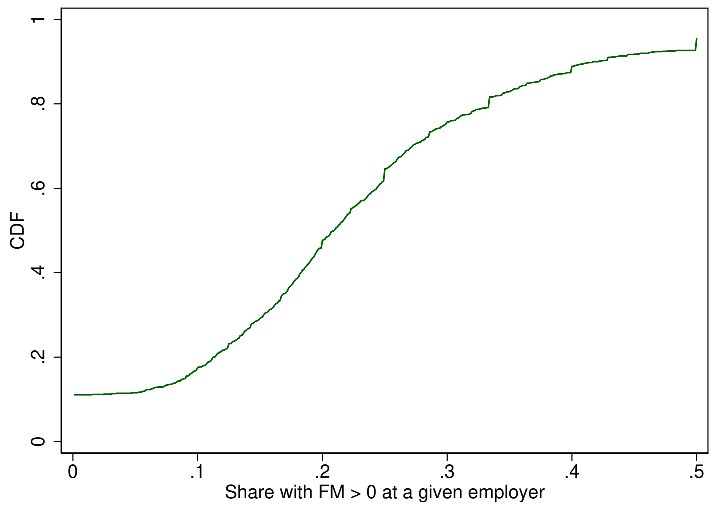


Figure B3: CDF of prevalence of foregone match at the employer level

Notes: The CDF is computed with respect to the unweighted distribution of firms.

use. The fact that these shares fall only very modestly follows directly from the fact that foregone match is pervasive across firms.

Next we turn to the foregone match amounts. The first two columns of Table B2 show the average foregone match amount in our baseline sample. Column (1) gives the quantity among those with foregone match, and so is the mean conditional on having some foregone match. Column (2) gives it among all couples (i.e. including a zero foregone match for the approximately 80% of couples with no unexploited arbitrage condition). In the baseline, these are \$757 and \$146, respectively (worth 12.6% and 2.4% of employee contributions). Both conditional and unconditional foregone match amounts are only modestly affected by our sensitivity exercises, which suggests that the foregone match amounts observed in plans with high incidence of foregone match are broadly similar to those in the broader population of retirement plans.

The lesson we draw from this analysis is that miscoding of employer matching formulas is unlikely to significantly either change the incidence and amount of foregone match estimated in our data.

B.4 A simulation exercise to bound maximal contribution of miscoding

As a final validation exercise, we compute an upper bound of the amount of foregone match that could be explained by measurement error. In this exercise, we suppose that all couples are truly

Table B1: Estimated proportion failing to actively coordinate after dropping high-FM firms

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	Baseline		Dropping top 2.8%		Dropping top 10%	
Sample of true couples:						
Proportion with foregone match	0.193		0.182		0.168	
Panel B:	Reshuffled	Pairs	Reshuffled	Pairs	Reshuffled	Pairs
Non-Coordination Benchmark:	Couples	of Singles	Couples	of Singles	Couples	Singles
Proportion with foregone match	0.331	0.336	0.319	0.330	0.307	0.322
Panel C:						
Implied share non-coord.	0.582	0.574	0.569	0.551	0.547	0.521

Notes: This table gives the sensitivity of the baseline results in Table 3 on the proportion of couples with foregone match. Columns (1) and (2) repeat our baseline results from that table. Panel A shows the proportion of true couples with foregone match; Panel B shows the proportion of couples in each of our non-coordinating; Panel C uses both sets of numbers to show the implied share of couples who are not actively coordinating. Columns (3) and (4) repeat the same analysis, dropping the 2.8% of couples with a spouse in the firms with the highest foregone match. This proportion is informed by the miscode rate in our audit. Columns (5) and (6) repeat the baseline analysis, dropping 10% of couples with a spouse in the firms with the highest foregone match.

Table B2: Mean Foregone Match, dropping high Foregone Match firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline		Dropping top 2.8%		Dropping top 10%	
	Conditional	Uncond.	Conditional	Uncond.	Conditional	Uncond.
Dollars	\$757.2	\$146.1	\$753.6	\$137.0	\$806.6	\$135.2
Share	12.6%	2.4%	12.1%	2.2%	12.1%	2.0%

Notes: This table gives the sensitivity of the baseline results in Table 2. Column (1) repeats the mean dollars per year among those with Foregone Match in our baseline sample. Column (2) adds the average across all couples (including those without positive foregone match). Columns (3) and (4) repeat the same analysis, dropping the 2.8% of couples with a spouse in the firms with the highest foregone match. This proportion is informed by the miscode rate in our audit. Columns (5) and (6) repeat the baseline analysis, dropping 10% of couples with a spouse in the firms with the highest foregone match.

coordinating, but all spouses have been assigned the incorrect plan. We show that even in this upper bound case, foregone match would be uncommon among truly coordinating couples.

We proceed in two steps. In the first step, we construct a simulated sample of coordinating couples. We take the baseline sample and assume that the match formula we have assigned to each spouse are correct. We leave untouched the observed contributions of the couples whose observed foregone match is zero. For those couples with observed foregone match, we reallocate the savings between the two spouses randomly within the set of efficient allocations. Thus, we are left with a simulated sample of couples with, by construction, zero foregone match.

In the second step, we randomly allocate match formulas to each couple, drawing uniformly from the set of plans that are included in the final sample.[50] We then recompute the foregone match under the new assignment of plans. This corresponds to the case where all couples (except for the few who happen to be randomly assigned their true plan) have been assigned the incorrect plan.

The result of this exercise is that we estimate that 13% of such couples, who are, by construction, all coordinating, would be measured to have foregone match. This upper bound (in which we have assumed that we have maximally-erred in linking individuals to their plan details) is considerably below the baseline result that 19.3% of couples have foregone match. Thus, we conclude that measurement error in plan assignment is unlikely to generate the level of non-coordination observed in the data.

The fact that estimated foregone match would be so low even if almost all formulas were miscoded is worth further discussion. This result can be seen in the context of the right panel of Figure 6. That figure shows that couples with zero foregone match tend to have saving relatively evenly split between spouses – otherwise, there is a high chance that one spouse exceeds their match cap while the other has not exhausted their match. Such an evenly-split allocation is likely to be efficient under any arbitrary plan assignnment, especially among those couples who work for the same employer (who are thus assigned the same plan).

Importantly, the contrapositive is also true: those true couples who have foregone match tend to have uneven savings. Such an allocation is likely to be inefficient even if we had assigned the

[50]For couples who work for different firms, this random assignment is done independently between the two spouses. For couples whose spouses work for the same firm, we impose that we make the same assignment for each spouse.

wrong plan. We take this as additional evidence that the foregone match we estimate could not be driven by errors in plan assignment at the firm level.

C Lifetime simulation model

To quantify the *lifetime* cost of non-coordination, we build a simple simulation model based on the empirical evidence in our panel sample of couples aged 25 to 59. The model is calibrated to match the incidence and the amount of foregone match by age and previous foregone match status in the preceding three years. We calibrate the simulation model to match three sets of inputs: (i) the transition probabilities into and out of having some foregone match (i.e., $Pr\left(FM>0\right)$), (ii) the average foregone match amount conditional on having some foregone match (i.e., $E\left(FM|FM>0\right)$), and (iii) the average investment returns of a Target Date Fund, the typical investment in employer-sponsored retirement savings plans. We estimate the probability and the conditional amount of foregone match of a couple $i$in year t in the panel sample. Our empirical model includes a second-order polynomial in the average age of the spouses (age_i), and 8 indicator variables (D^j) for each combination of the couples' foregone match status in the preceding three years (i.e., each combination of the values of FM_{t-1}, FM_{t-2}, and FM_{t-3} with couples with no foregone match in the previous 3 years as the omitted category). The estimating equations are given by:

$$Pr\left(FM_{i,t}>0\right)=\alpha+\beta_1.age_{i,t}+\beta_2.age_{i,t}^2+\sum_{j=1}^{8}\delta_J D_{i,t}^j+\epsilon_{i,t}$$

$$E\left(FM_{i,t}|FM_{i,t}>0\right)=\alpha+\beta_1.age_{i,t}+\beta_2.age_{i,t}^2+\sum_{j=1}^{8}\delta_J D_{i,t}^j+\epsilon_{i,t}$$

We report the estimates from these two regressions in Table C1. In order to initialize the model at ages 25, 26, and 27, we also estimate a modified version of the model with, respectively, no lags, a single lag, or two lags.

We calibrate the rate of return on retirement savings to match the average investment returns by age of a typical investment strategy in retirement savings plans. We assume that savings are divided across three types of assets: equities, bonds, and treasury bills. The allocation of savings

Table C1: Probability and the conditional amount of foregone match in the panel sample

	(1) $Pr\,(FM > 0)$ coef.	(s.e.)	(2) $E\,(FM \mid FM > 0)$ coef.	(s.e.)
Lagged foregone match				
- (0; 0; 1)	0.077	(0.002)	118.3	(17.3)
- (0; 1; 0)	0.132	(0.003)	-76.8	(16.2)
- (0; 1; 1)	0.108	(0.002)	-59.6	(14.3)
- (1; 0; 0)	0.543	(0.003)	243.4	(10.8)
- (1; 0; 1)	0.557	(0.007)	153.1	(17.9)
- (1; 1; 0)	0.656	(0.003)	269.1	(10.9)
- (1; 1; 1)	0.797	(0.001)	544.8	(7.5)
Mean age of spouses				
- age	0.002	(0.000)	76.9	(3.0)
- age^2	0.000	(0.000)	-0.6	(0.0)
Constant	-0.016	(0.006)	-1593.2	(66.4)

Notes: The dependent variable in column (1) is an indicator variable for a couple having some foregone match, and the coefficients are those from a linear probability model. The dependent variable in column (2) is the foregone match amount (in dollars) conditional on having a positive foregone match; the coefficients are those from an Ordinary Least Squares regression. We include 7 indicator variables for lagged foregone match status that capture each combination of the couples' foregone match status in the preceding three years. We treat couples with no foregone match in the previous 3 years as the omitted category

across these three asset classes is calibrated to match the glide path of Fidelity target date funds.[51] The average real rates of return for each of these three asset classes are chosen to match the evidence in Jordà et al. (2019).

We simulate the incidence and amount of foregone match for 100,000 couples between ages 25 and 55. We assume the foregone savings would have earned the expected return on a Target Date Fund investment until age 65. We report two metrics for the lifetime cost of non-coordination: (i) Sum of foregone dollars, which is the sum of foregone match at each age, assuming these amounts are not invested (i.e., earn a 0% real rate of return), and (ii) Foregone wealth at age 65, which is the cumulative foregone match under the assumption that foregone match amounts would have been invested in a typical Target Date Fund until age 65. In Table 2, we summarize the distribution of these two metrics in the simulated sample.

[51] Specifically, we target the allocations to equities. bonds and short-term debt by age, as of year-end 2022, of the Fidelity Freedom Funds with target retirement dates between 2005 and 2065.

Table D1: Proportion with foregone match

		(1) N	(2) Prop.
	All	424,280	24.4%
(1)	No unvested	260,520	20.0%
(2)	Baseline age restriction	339,410	22.6%
(3)	No short tenure	294,340	20.8%
	Baseline: (1), (2), and (3)	184,620	19.3%
(4)	Baseline + no Equitable Division	45,210	19.7%
(5)	Baseline + no low earnings	163,540	19.3%
(6)	Baseline + no age $\geq$ 55	149,820	18.9%
(7)	All Restrictions (4)-(6)	33,010	19.5%

Notes: The sample is the full set of couples in our linked employer-employee data in the 2015 cross-section, subject to the restriction that at least one member contributes and at least one member works for an employer that offers a match. Each of the rows labeled (1) to (3) applies only the sample restriction listed. The "baseline" row applies all restrictions (1)-(3) simultaneously; we refer to the sample surviving these restrictions as the 'baseline" sample. Rows (4)-(7) start from the baseline sample and apply additionally only the sample restriction listed. Row (7) applies all restrictions simultaneously. Column (1) gives the number of couples in the sample, and column (2) gives the proportion with a foregone match.

D Additional Tables and Figures

D.1 The incidence of foregone match

Table D1 shows the share of couples with foregone match is slightly higher when we do not impose our baseline age, tenure, and vesting sample restrictions (24.4%), and is broadly similar when we restrict the sample to couples living in community property states where divorce rules are more transparent (19.7%), or when exclude couples in which either spouse has earnings below $15,000 (19.3%). This share is also similar when we exclude couples with at least one member age 55 or older (and for whom the savings are more liquid in unemployment).

D.2 Characterizing inefficiency

Figure 1 motivated our test by focusing on a case where the efficient strategy is simple: one spouse should do all the saving. This involved drawing a sample which is small (with approximately 2,800 couples) and selected (drawn from those who save small amounts). In contrast, Table D2 broadens the analysis and studies the prevalence of inefficiency in a broader set of cases where the strategy required for efficiency differs according to the level of saving and combination of match schedules. Our aim is to show that our result of widespread inefficiency is broad-based and not restricted to certain saving levels or combinations of match plan characteristics.

We take the full set of couples where both match schedules are single-tier schedules – that is the employer match is a single match rate up to a cap, after which no further matching is provided.[52] As in Section 4.1.1, we denote the spouse with the lower and higher match rate as spouse L and H respectively, and their caps as c_L and c_H, measured in dollars. In contrast to Section 4.1.1, we include cases where the two spouses face the same match rate, in which case we deem H to be the spouse with the lower match cap (as measured in dollars).

In Table D2 we divide couples into cells across which the savings strategy required for efficiency differs. We place couples into cells defined by (across the columns) the total level of retirement saving that they do and (down the rows) the nature of the match rate that each spouse faces. The groups of retirement saving are:

(A) The 'Low Household Saving' group contains couples who have total saving that is lower than c_H, the cap of the spouse with the higher match rate. This was the restriction used to generate the sample studied in Figure 1.[53]

(B) The 'Medium Household Saving' group contains couples who have total saving that is higher than c_H but lower than than the sum of the two caps ($c_H + c_L$).

(C) The 'High Household Saving' group contains couples who save more than the sum required to fully exploit both spouses' employer match.

The groups defined by the match rates (in rows) are:

1. Spouse L's match rate is zero.

2. Spouse L's match rate is positive but below spouse H's match rate.

3. Spouse L and spouse H have the same match rate.

Figure 1 in the paper, our motivating example, can be interpreted as a close examination of cells A1 and A2. Table D2 broadens this exercise to consider all nine cells: for each of these cells, we can

[52]In Table D3, we will further broaden the sample to include plans with two different match rates over different segments.

[53]To generate implications of efficiency for Figure 1 it was not necessary to restrict to one-tier match schedules; thus, the sample in this column of Table D2 is not exactly equivalent to the sample in Figure 1.

give the savings strategy required for efficiency and test the extent to which efficiency is achieved.[54]

For cases (1) and (2), where spouses have different match rates, efficiency requires couples to first "use up" the part of the couple's match schedule with the highest match rate. This means that the first $\$c_H$ of saving should be in spouse H's account. Then, the couple should fill up the part of the couple's match schedule with the *next* highest rate – that is, savings between $\$c_H$ and $\$(c_H + c_L)$ should be in spouse L's account.[55] Finally, savings beyond $\$(c_H + c_L)$ can be allocated to either account with no consequence to efficiency.

In case (3), where both spouses have the same match, there is a larger set of allocations that is consistent with efficiency. In particular, all allocations are efficient *except* where one spouse exceeds the match cap while the other is strictly below the match cap. This is not possible for the 'Low Household Saving' group and so all allocations in cell A3 are consistent with efficiency.

Table D2 reports results for these nine cells (of which eight contain some of the sample and in seven of which there is a testable implication of efficiency). In each cell of Table D2 we give the testable implication of efficiency, report the share with foregone match in the data and in each of our synthetic samples, the average foregone match (conditional on positive foregone match) and the sample size. A substantial share of couples in all of these cells are saving inefficiently, with substantial heterogeneity across cells. That heterogeneity is, however, not just driven by heterogeneity in the incidence of coordination, but also how common it is for couples to have to coordinate to avoid forgoing some match. Intuitively, and as is seen in the rates of those inconsistent with efficiency in our synthetic samples, there is less scope for couples to forego some match when the match rates are identical or when the households save large amounts: in these cases, a wide range of allocations is consistent with efficiency. Comparing the proportions inefficient in the data to that in our synthetic samples, coordination increases the more saving there is (relative to match caps).

Table D3 further broadens the sample to include those plans where the match schedule has more

[54]For the sake of focusing on couples with the clearest incentives, we drop all couples whose column assignment would depend on whether we compute the cap with or without adding the adjustment for health insurance to wages (see Section A.3 for details). That is, the condition for being in column 1 is that $S < c_H$, where c_H is computed without adding $ESI = \$5,000$ to wages. In column 2, the condition is that $S > c_H$ (where c_H is computed after adding ESI to wages) and $S \leq c_H + c_L$ (where c_L and c_H are each computed without adding ESI to wages). Finally, in column 3, the condition is that $S > c_H + c_L$, where c_L and c_H are each computed after adding ESI to wages. This restriction drops about 14% of couples that would otherwise have been included in this table.

[55]Note that cell B1 is empty as no couple can have medium saving and be in case (1), where spouse L has no match and so $\$c_L = 0$. Therefore there are effectively eight cells containing some couples rather than nine.

Table D2: Characterizing non-coordination in the subset of couples where both spouses face simple matching formulas

		Household-level Saving (S): Spouse H + Spouse L contributions		
		(A): **Low** Saving	(B): **Medium** Saving	(C): **High** Saving
Spouse H has a higher match and spouse L has ...		$S \leq c_H$ (i.e. couple cannot save more than Sp. H's cap (c_H))	$c_H < S < c_H + c_L$ (i.e. can exploit Sp. H match but not both)	$S \geq c_H + c_L$ (i.e. can fully exploit both spouses' matches)
(1)... **no** match	For efficiency:	Sp. L should not save	n.a.	Sp. H should save ≥cap
	Reshuffled Couples:	25.3% inconsistent	n.a.	22.5% inconsistent
	Pairs of Singles:	33.5% inconsistent	n.a.	25.7% inconsistent
	Data:	**27.6% inconsistent**	n.a.	**19.5% inconsistent**
	Avg. FM:	$873	n.a	$782
	N:	590	n.a	1,370
(2)... a **lower** match rate	For efficiency:	Sp. L should not save	Sp. H should save = cap	Both should save ≥cap
	Reshuffled Couples:	52.2% inconsistent	74.5% inconsistent	26.0% inconsistent
	Pairs of Singles:	52.8% inconsistent	77.1% inconsistent	33.1% inconsistent
	Data:	**54.1% inconsistent**	**72.6% inconsistent**	**18.2% inconsistent**
	Avg. FM:	$654	$648	$837
	N:	990	1,680	3,660
(3)... the **same** match rate	For efficiency:	All allocations are efficient	Both should save <cap	Both should save ≥cap
	Reshuffled Couples:	0.0% inconsistent	33.0% inconsistent	29.0% inconsistent
	Pairs of Singles:	0.0% inconsistent	32.4% inconsistent	27.2% inconsistent
	Data:	**0.0% inconsistent**	**18.1% inconsistent**	**10.6% inconsistent**
	Avg. FM:	n.a.	$747	$882
	N:	2,590	9,470	17,200

Notes: See the text for the description of this table.

than one tier. We do not attempt to enumerate the potential situations in the manner of Table D2, as the number of such situations is substantially larger when we allow more than one tier. Instead, we simply separate our sample into the number of tiers of each spouse's match schedule. For the purpose of this table, we deem Spouse 1 to have the lower number of tiers. A majority of our sample has one or more spouses in firms where the match schedule has two (or more) tiers. This is largely driven by the size of the federal government, the largest employer in our sample, which has a two-tier plan (known as the Thrift Savings Plan). There is some heterogeneity across groups, but, echoing the lesson we took from each of Table 1 and Table D2, inefficiency is broad-based.

Table D4 decomposes non-coordination in a different manner. The table allocates couples with foregone match across four cells defined by the number of spouses that contribute (across the columns) and the number of spouses whose contributions are in excess of their match cap (across the rows). The table shows that 41% of all non-coordination occurs when one spouse contributes beyond their match cap while the other does not fully exploit their match. The remaining 65% is split across the remaining cells with shares between 15% and 25%.

Table D3: Characterizing non-coordination by Plan Type

		Spouse 2				
		No match	One tier		Two+ tiers	
	No match	Not in sample	Reshuffled Couples:	22.9% inconsistent	Reshuffled Couples:	20.0% inconsistent
			Pairs of Singles:	27.3% inconsistent	Pairs of Singles:	23.5% inconsistent
			Data:	**21.8% inconsistent**	**Data:**	**19.5% inconsistent**
			Avg. FM:	$815	Avg. FM:	$773
			N:	2,140	N:	3,130
	One tier	-	Reshuffled Couples:	36.6% inconsistent	Reshuffled Couples:	35.7% inconsistent
		-	Pairs of Singles:	30.1% inconsistent	Pairs of Singles:	40.8% inconsistent
Spouse 1		-	**Data:**	**16.2% inconsistent**	**Data:**	**29.5% inconsistent**
		-	Avg. FM:	$769	Avg. FM:	$705
		-	N:	41,530	N:	26,690
	Two+ tiers	-	-		Reshuffled Couples:	31.8% inconsistent
		-	-		Pairs of Singles:	33.6% inconsistent
		-	-		**Data:**	**17.9% inconsistent**
		-	-		Avg. FM:	$772
		-	-		N:	111,120

Notes: This table partitions the baseline sample into five cells based on the number of tiers of the match schedule for each spouse. Spouse 1 is defined to be the spouse with (weakly) fewer tiers. In each cell, we report the sample size (rounded to the nearest 10) and the share of each cell with $FM > 0$.

Table D4: Partitioning Non-coordination into Cells

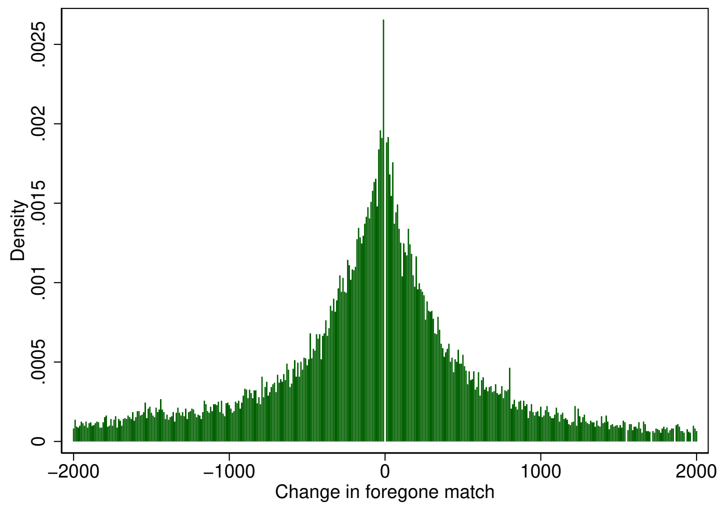
	Both spouses contribute	One spouse contributes
Neither above cap	0.254	0.147
One member above cap	0.406	0.193

Notes: This table partitions the share of couples with $FM > 0$ into four cells; that is, the four cells of the table mechanically sum to one. One dimension of the partition is whether both spouses contribute or only one contributes. The other dimension is whether neither member contributes in excess of the matching cap, or whether at least one member does. Mechanically, it is impossible to have $FM > 0$ in couples where both are contributing in excess of the cap or in which neither are making any contributions at all. This table uses the baseline sample, restricted to those with $FM > 0$.

D.3 The role of inertia

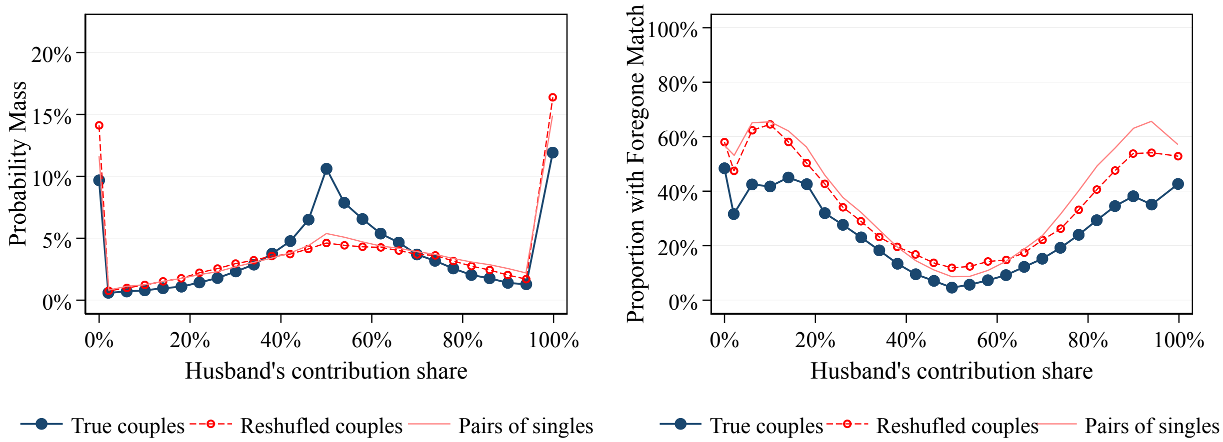
Figure D1 shows the distribution of the *change* in the quantity of foregone match by couples in which both spouses made an active change in their contribution rate (we omit a large mass at zero). This distribution has a mean of minus $11 – that is, the mean change in foregone match after an active change was very small. Further, the distribution is close to symmetric – the share of couples *reducing* the extent of their foregone match (9.4%) is only slightly larger than the share of couples *increasing* it (8.1%). This result suggests that the non-coordination we document is not driven by spouses being temporarily away from the efficient allocation due to inertia.

Figure D1: Change in Foregone Match, conditional on active decision



Notes: Figure shows the distribution of the change in foregone match, conditional on making an active decision to change employee contribute rate. The sample is all those couples in our panel who are observed for at least four consecutive years, restricted to those couple-year observations where both spouses changed their contributions by at least 0.8% of earnings. We additionally require that all couple-year observations survive our baseline vesting, tenure, and age restrictions. We exclude those couples where either spouse is in a plan that has auto-features. The graph excludes a very large excess mass (84% of the population) located at exactly $0. Foregone match is defined as the difference between (a) the match that would be received if total contributions were allocated to maximize the total match received and (b) the match actually received.

Figure D2: Equal Saving Heuristics: Density and Prob. of FM, by Husband's Contribution Share



(a) Distribution of Husband's Contribution Share (b) Prop with FM, By Husband's Contribution Share

Notes: Panel (a) plots the density of the husband's dollar share of contributions across different-gender couples in our baseline sample. Panel (b) plots the probability of FM as a function of the husband's share of contributions, measured analogously. We drop couples where at least one spouse is contributing greater than 95% of the statutory maximum on individual contributions ($18,000 or $24,000 depending on age).

D.4 Additional analysis of role of saving heuristic

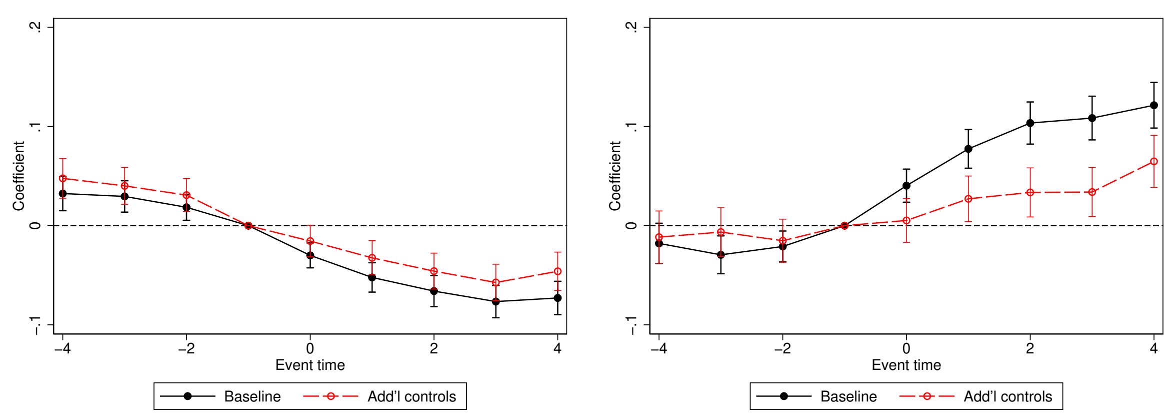
Figure 6 showed the density of how intra-household contribution shares are split between members of the couple, and shows how those shares relate to the probability of having foregone match. In that figure, we measure contributions as a proportion of earnings (i.e. savings rates) across spouses. Figure D2 shows equivalent analysis but where we measure contribution shares in dollars. The figures are very similar, and the conclusions we drew from Figure 6 are not sensitive to whether contribution shares are measured in dollars or as a proportion of earnings.

D.5 Marriage and Divorce Event Studies

Appendix Figure D3 plots stacked event study coefficients corresponding to Figure 5. The regression specification uses the true sample pooled (or "stacked") with the synthetic sample. The independent variables of interest are dummies for event time (year relative to marriage or divorce, -1 omitted) interacted with a dummy for being the "true" couple rather than the synthetic couple. The regression includes "pair-by-time" fixed effects, where (1) a given "pair" includes the true couple and its matched synthetic couple, and (2) "time" is the interaction of event time and the year of the marriage or divorce. As a result, the effects of interest are identified by comparing the time path of foregone match of the true couples against their matched synthetic couple (i.e., "clean controls"), which allows us to sidestep many of the issues raised by the recent difference-in-differences literature (see Roth et al. (2022) for a review).

The solid series plots these raw event study coefficients, yielding changes that are very similar to the raw changes observed in Figure 5.[56] Further, the event study framework allows us to add time-varying controls: namely, fixed effects for the total earnings and contributions of the couple interacted with event time. For instance, some of the reduction in foregone match at marriage could be caused by increases in contributions (e.g., putting both spouses above their match cap) correlated with marriage that have nothing to do with coordination. The dotted red series plots the event study with these additional controls. These controls reduce the total effect of marriage (comparing event times -4 and +4) from 10.5 percentage points to 9.5 percentage points, and reduce the total effect of divorce from 14 percentage points to 7.5 percentage points. Of course, coordination could also cause changes in total contributions – meaning that total contributions could be a "bad control", leading to attenuation bias on the effect of coordination. Thus, we interpret the two series as representing bounds on the effect of marriage and divorce on non-coordination.

Figure D3: Prob. of non-coordination: Marriage and Divorce Event Studies



(a) Marriage (b) Divorce

Notes: This Figure plots the coefficients from event study regressions of a dummy for positive foregone match on event time dummies interacted with a treatment indicator, with event time -1 omitted. The sample includes both true couples (who get married or divorced at event time zero) and their matched synthetic couple. The regression includes "pair-by-time" fixed effects, where (1) a given "pair" includes the true couple and its matched synthetic couple and (2) "time" is the interaction of event time and the year of the marriage or divorce. The treatment indicator is a dummy for being the "true" (rather than synthetic) couple. The regression additionally includes fixed effects for each true and each synthetic couple. In the red series, we add fixed effects for event time interacted with age, total couple-level earnings, and total couple-level contributions.

[56]This is not mechanical, since certain observations that are included in Figure 5 – such as an observation where the true couple contributes a positive amount but the synthetic couple contributes zero – drop out of Figure D3.

Table D5: Full regression results: Probability of $FM > 0$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Length of marriage	**0.0031**							**-0.0010**		**-0.0019**
	(0.0001)							(0.0002)		(0.0006)
Kids	**-0.0254**							**-0.0057**		**-0.0110**
	(0.0018)							(0.0020)		(0.0039)
Future divorce	**0.0109**							**0.0181**		**0.0108**
	(0.0032)							(0.0031)		(0.0054)
Mortgage	**-0.0188**							**-0.0244**		**-0.0321**
	(0.0025)							(0.0025)		(0.0055)
Mean age		**0.0035**						**0.0036**		**0.0031**
		(0.0001)						(0.0001)		(0.0003)
Age gap		**0.0001**						**0.0006**		**0.0002**
		(0.0004)						(0.0004)		(0.0007)
Share of income for P.E.			**0.1157**					**0.0071**		**-0.0477**
			(0.0076)					(0.0079)		(0.0166)
One hired after A.E.				**0.0299**				**0.0006**		**0.0079**
				(0.0023)				(0.0026)		(0.0052)
Both hired after A.E.				**-0.0622**				**-0.0366**		**-0.0377**
				(0.0029)				(0.0030)		(0.0055)
Equitable division state					**-0.0037**			**-0.0043**		**-0.0079**
					(0.0020)			(0.0020)		(0.0041)
Mean tenure						**0.0028**		**0.0008**		**0.0010**
						(0.0002)		(0.0003)		(0.0006)
Tenure gap						**0.0076**		**0.0027**		**0.0016**
						(0.0003)		(0.0003)		(0.0006)
Total income								**0.0186**		**0.0090**
								(0.0024)		(0.0062)
Same firm							**-0.0766**	**-0.0709**		**-0.0859**
							(0.0058)	(0.0059)		(0.0125)
Same match							**-0.0379**	**-0.0376**		**-0.0406**
							(0.0060)	(0.0060)		(0.0128)
Joint bank account									**-0.0172**	**-0.0151**
									(0.0053)	(0.0053)
Observations	184,600	184,600	184,600	184,600	184,600	184,600	184,600	184,600	44,400	44,400
Baseline mean	0.1929	0.1929	0.1929	0.1929	0.1929	0.1929	0.1929	0.1929	0.1830	0.1830

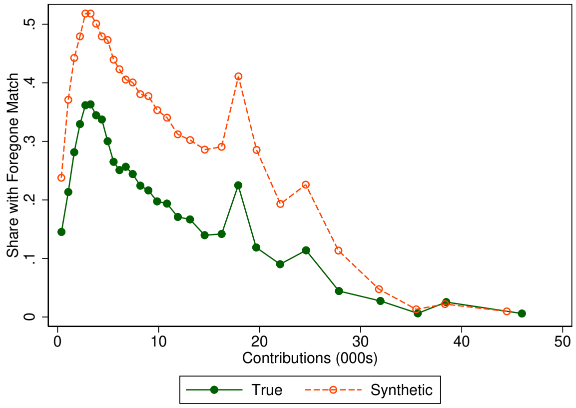
Notes: This table reports a series of regression coefficients, with one regression per column. The dependent variable is a dummy for $FM > 0$. All columns include interacted fixed effects for bins of total couple-level earnings and contributions. "P.E." stands for "primary earner" – the member of the couple with higher earnings. "A.E." stands for auto-enrollment. The sample is the baseline sample.

Table D6: Full regression results: FM scaled by contributions

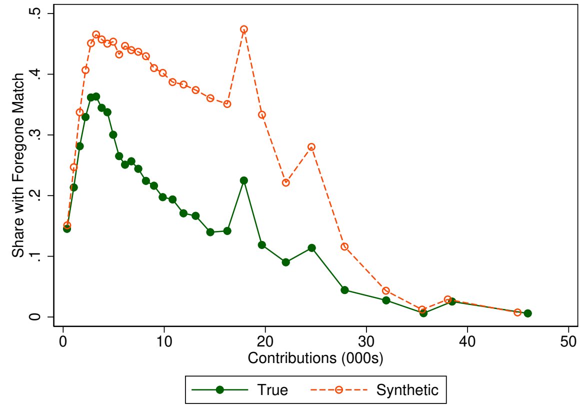
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Length of marriage	**0.0659**							**-0.0141**		**-0.0292**
	(0.0028)							(0.0039)		(0.0117)
Kids	**-0.6099**							**-0.1975**		**-0.1764**
	(0.0386)							(0.0401)		(0.0740)
Future divorce	**0.0949**							**0.2416**		**0.1192**
	(0.0687)							(0.0679)		(0.1058)
Mortgage	**-0.2676**							**-0.3487**		**-0.4088**
	(0.0546)							(0.0541)		(0.1097)
Mean age		**0.0726**						**0.0698**		**0.0599**
		(0.0021)						(0.0031)		(0.0062)
Age gap		**-0.0244**						**-0.0090**		**-0.0114**
		(0.0073)						(0.0074)		(0.0120)
Share of income for P.E.			**-1.9069**					**-4.0088**		**-4.3248**
			(0.1428)					(0.1529)		(0.3177)
One hired after A.E.				**0.4194**				**-0.0113**		**0.1820**
				(0.0504)				(0.0576)		(0.1087)
Both hired after A.E.				**-1.0519**				**-0.5092**		**-0.3892**
				(0.0528)				(0.0563)		(0.0966)
Equitable division state					**-0.0093**			**-0.0361**		**-0.0439**
					(0.0413)			(0.0409)		(0.0762)
Mean tenure						**0.0738**		**0.0292**		**0.0209**
						(0.0046)		(0.0056)		(0.0119)
Tenure gap						**0.1139**		**0.0323**		**0.0388**
						(0.0057)		(0.0061)		(0.0135)
Total income								**0.2245**		**0.0973**
								(0.0667)		(0.1050)
Same firm							**-0.5529**	**-0.7703**		**-0.9479**
							(0.0946)	(0.0954)		(0.1994)
Same match							**-1.6544**	**-1.6252**		**-1.4838**
							(0.1046)	(0.1038)		(0.2170)
Joint bank account									**-0.2947**	**-0.2849**
									(0.1002)	(0.0990)
Observations	184,600	184,600	184,600	184,600	184,600	184,600	184,600	184,600	44,400	44,400
Baseline mean	2.4302	2.4302	2.4302	2.4302	2.4302	2.4302	2.4302	2.4302	2.1510	2.1510

Notes: This table reports a series of regression coefficients, with one regression per column. The dependent variable is the ratio of FM to total contributions. All columns include interacted fixed effects for bins of total couple-level earnings and contributions. "P.E." stands for "primary earner" – the member of the couple with higher earnings. "A.E." stands for auto-enrollment. The sample is the baseline sample.

Figure D4: Incidence of Non-Coordination



(a) Comparison to Synthetic Sample 'M'



(b) Comparison to Synthetic Sample 'S'

Notes: Sample for 'True' profile is our baseline sample. Sample for 'Synthethic' is our placebo sample formed by matching singles. Each line shows proportion of sample with some foregone match (greater than a *de minimis* threshold of $10). The sharp spike in the incidence of non-cooperation seen in each graph at approximately $18,000 is located at the annual maximum for contributions for those aged under 50. There is an excess mass of couples located at these points which is comprised of couples where one member contributes the maximum and the other member contributes nothing. If, as is common, the spouse contributing the maximum is contributing in excess of the match cap, this combination of contribution will be inefficient if the non-contributing spouse is eligible to receive an employer match.

E Survey

E.1 Introduction

To explore the mechanism underlying the extent of foregone match, we designed and ran a survey (Choukhmane et al. (2025)). We recruited 1,000 respondents through the platform Prolific, and screened them so that they were working, married, aged between 18 and 59, and had a retirement plan. This appendix proceeds as follows. Section E.2 discusses the design of the survey. Section E.3 briefly describes the survey population. Section E.4 discusses our results.

E.2 Survey Design

This section summarizes the survey design. We start by introducing the central element of our survey: a hypothetical choice problem that resembles the retirement saving problem we study in the administrative data. After this, we summarize the rest of the survey instrument.

E.2.1 Hypothetical Choice Problem

Each survey respondent was asked how they would allocate $3,000 of retirement saving between their own retirement account and their spouse's retirement account. Both accounts have an employer match schedules attached. There were three versions of the pair of match schedules, and respondents were offered one of these at random:

- In a vignette which we refer to as our **'Max via spouse'** version, the match-maximizing strategy was to contribute all funds to the account of the spouse. Respondents' contributions were matched 50 cents on the dollar up to $3,000, and their spouse's contributions were matched dollar for dollar up to $3,000.
- In a vignette which we refer to as our **'Max via own'** version, the match-maximizing strategy was to contribute all funds to the account of the respondent. Respondents' contributions were matched dollar for dollar up to $3,000, and their spouse's contributions were matched 50 cents on the dollar up to $3,000.
- In a vignette which we refer to as our **'Max via split'** version, the match-maximizing strategy was to split the funds between the two accounts. Respondents' contributions were matched 50 cents on the dollar up to $2,000, and their spouse's contributions were matched dollar for dollar up to $2,000. To maximize the match therefore, respondents would need to allocate $2,000 to their spouse's account, and $1,000 to their own account.

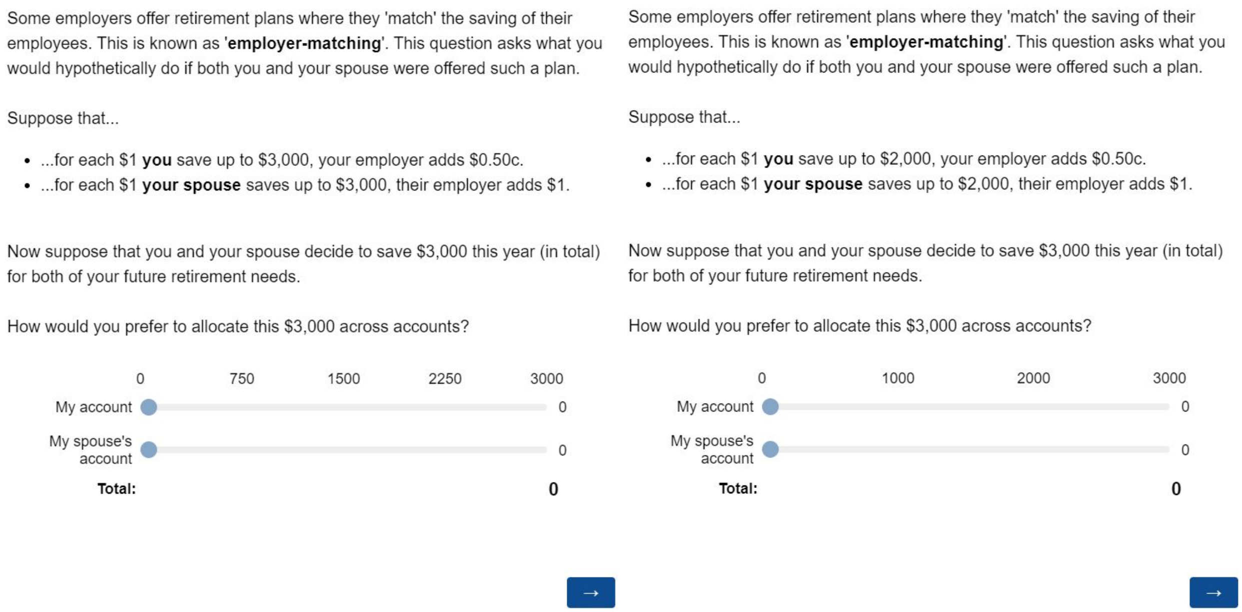
Figure E1 shows vignettes A and C. In 50% of cases respondents made their allocations using 'sliders' as shown. In 50% of cases they entered their answers in boxes.

E.2.2 Choice Problem Follow-Up

For those who did not choose the match-maximizing strategy, we tell them that that they could have received more matching dollars by reallocating their contributions and we query whether they realized this fact when making their choice. Figure E2 illustrates this question.

For respondents who did realize that they had foregone match, we ask them for a narrative explanation for their choice. For respondents who did not, we offered them a chance to re-allocate. Most take this opportunity and reallocate to maximize the match. For those who do maximize the

Figure E1: Examples of Vignette



(a) Vignette A: **'Max via spouse'**

(b) Vignette C: **'Max via split'**

Notes: Panel A gives a screenshot of the version of the hypothetical choice problem in which respondents would maximize their match by putting all contributions into their spouse's account. Panel B gives a screenshot of the version of the hypothetical choice problem in which respondents would maximize their match by putting $2,000 in their spouse's account and $1,000 into their own account. In half of cases, respondents recorded their answers on 'sliders', as shown here. In the other half of cases, respondents were asked to type their answers in boxes.

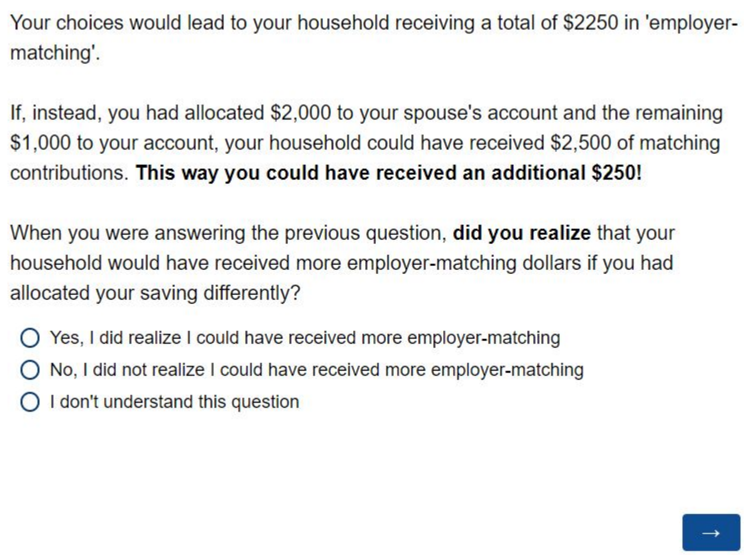
match after taking up the opportunity to re-allocate, we also ask them for a narrative justification of their choice. We use responses to these questions (in a manner discussed below) to distinguish between those who have "deliberate" foregone match (i.e., those who realized their allocation did not maximize the match) and those who have "accidental" foregone match (i.e., those who did not realize that their allocation did not maximize the match).

E.2.3 Divorce Law

We asked respondents a set of questions designed to evaluate their knowledge of how assets would be split in case of a divorce. For retirement wealth, we asked respondents to choose between:

1. "I don't know"
2. "I would keep my account and my spouse would keep theirs"
3. "How much I would receive depends on whose account is larger"
4. "We would each keep 50% of the total in all the accounts"
5. "Other"

Figure E2: Follow Up To Choice Problem



Notes: This figure gives screenshot of the screen which followed a respondent making a choice in which their selections implied some foregone match. The screen is populated with the quantity of foregone match implied by the their choices.

where the last two options are designed to span the set of actual rules. We group together those who choose option 2 and 3 (to form a group formed of those who think one largely retains one's own retirement wealth), and we group together those who chose option 4 or 5 (to form a group that recognizes that these funds will not necessarily be retained by the spouse that makes the contribution).

We also solicited their impressions of how funds in individual checking accounts, joint accounts, and housing wealth would be split. For individual checking accounts we offered the same set of options as for retirement accounts. For joint accounts and housing we offer a different set of options to choose from, recognizing the intrinsic jointness of the asset. The options are "We would each keep 50% of the total in all the accounts", "We would each keep what we contributed to the joint asset", and "Other/I don't know".

E.2.4 Financial Literacy

We included five questions on financial literacy, which are given below. Questions 1, 2 and 3 are taken from Lusardi and Mitchell (2011), question 4 is taken from Lusardi (2008), and question 5 is new, tailored to our setting, which solicits knowledge about the taxation of traditional 401(k) contributions and withdrawals.

1. Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow:
   - more than $102
   - exactly $102
   - less than $102
   - Don't know
2. Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, with the money in this account would you be able to buy:
   - More than today
   - Exactly the same as today
   - Less than today
   - Don't know
3. Do you think that the following statement is true or false? 'Buying a single company stock usually provides a safer return than a stock mutual fund.'
   - True
   - False
   - Don't know
4. If interest rates fall, what should happen to bond prices?
   - They would rise
   - They would fall
   - They would say the same
   - None of the above
   - Don't know
5. Thinking about saving in a traditional 401(k) and withdrawing after the age of 60, which of the following is true:
   - You pay income taxes on what you save now but do not pay income taxes on withdrawals
   - You pay no income taxes on what you save now but pay income taxes on withdrawals
   - You pay income taxes both on what you save now and on withdrawals
   - You pay no income taxes on what you save now and no income tax on withdrawals
   - Don't Know

More than 95% of respondents got at least one question correct, and so we group respondents into four groups depending on whether they got 2 or fewer, 3, 4, or 5 questions correct.

E.2.5 Retirement Plan Characteristics

We asked respondents whether they and their spouse had a retirement plan, and if so, whether it was a Defined Benefit or Defined Contribution Retirement Plan. For those respondents where either they or their spouse was reported to have a Defined Contribution Plan, we solicited the match schedule. In both cases we offered respondents the option of saying "I never knew", and "I don't know but I did at the time I (my spouse) made my (their) savings decision."

E.2.6 Questions on co-ordination in reality

For those who replied that both they and their spouse have a Defined Contribution plan (and so the arbitrage opportunity at the heart of our paper would be relevant) we asked whether they considered the possibility that coordinating their contributions might be profitable, and if so whether they were coordinating to avoid foregoing any employer match. The question and responses are given below.

"You and your spouse both have access to a 401(k) style retirement account. In some such situations, like in our previous hypothetical example, it might be beneficial for the spouse with the better employer match to do most or all of the retirement saving for the couple.

Is this something you have ever considered when making your retirement-saving decisions?"

- I have considered it, and we are coordinating our savings to get the most matching dollars
- I have considered it, but we are not coordinating our savings to get the most matching dollars
- I have considered it, but am not sure whether we are getting the most matching dollars
- I have not considered it, but I now will
- I have not considered it, and I still have no plans to
- I do not understand
- Refuse to answer

E.2.7 Demographic and marital characteristics

We included questions on own and spousal labor supply, income (in bands), and a set of questions that mirror those in our administrative data (whether the couple has a joint account, the length of their marriage, whether they have a mortgage, and whether they have a child).

E.3 Survey Population

Before turning to results, we describe the demographic composition of the survey respondents, to allow a comparison with our primary analysis sample. By design, all respondents are in employment, holding a retirement plan, are married and aged between 16 and 59. We recruited 1,000 participants, of which 987 comprise our analysis sample (12 respondents were dropped due to their reporting not being married, despite this screen, or incomplete data, and two observations appeared to be completed by the same person, and we dropped one of them). The sample is evenly balanced between men and women.

Table E1, further, summarizes distributions of own earnings, household earnings, age, and marriage length.[57] A comparison of the sample with Table 1 in the paper shows a close correspondence on these observables. Mean household earnings in the survey sample (baseline administrative sample) are $154,749 ($175,700). Mean age is 39.6 (45.1). Mean marriage length (censored at 19) is 9.8 (10.6).

Table E1: Demographic Composition

Characteristic	Mean	p25	p50	p75
Own earnings ($)	90333.3	55000	85000	125000
Household earnings ($)	154748.7	110000	140000	205000
Age	39.6	32	38	46
Marriage length (censored, see notes)	9.8	4	9	16

Notes: Earnings are recorded in bands. To calculate these percentiles we set each observation to the mid point of its band. For the lowest band, "less than $15,000", we assume earnings are $15,000. For the highest band, "greater than $150,000", we assume earnings are equal to $150,000. Marriage length is censored at 19 years, for comparability with our administrative data. The mean marriage length (uncensored) is 11 years.

The survey sample, as in case of our administrative sample, represents a population that is richer, on average, than the US population (note our discussion in Section 3.3). This arises out of

[57]Earnings are recorded in bands. To calculate these percentiles we set each observation to the mid point of its band. For the lowest band, "less than $15,000", we assume earnings are $15,000. For the highest band, "greater than $150,000", we assume earnings are equal to $150,000.

selecting a sample who are married, employed, and with a retirement plan.

E.4 Results

In this section we summarize the results from the survey

E.4.1 Two-fifths of respondents have foregone match

Table E2 gives the proportion of respondents with foregone match: approximately 40% of respondents make a choice in response to our hypothetical allocation problem that does not maximize the match. In calculating an indicator of having foregone match, we apply a *de minimis* threshold of $50–for those with lower levels of unexploited match, we do not code them as having foregone match.

Table E2: Foregone Match

Characteristic	N	Prop. of total
No foregone match	594	60.2
Has foregone match	393	39.8
Total	987	100.0

Notes: This tables gives the number and share of survey respondents who have, and do not have, foregone match.

E.4.2 Both deliberate foregone match and accidental foregone match are common

For those with foregone match we followed up to a) assess whether they had realized they had foregone match, and b) if they did not, we asked whether they wish to change the allocation of their contributions. Table E3 shows the responses to these questions. Panel A shows that approximately 40% did realize they had foregone match, while 60% did not realize this. Of the latter group, Panel B shows that, when given a chance to re-allocate, 74% chose to change their allocation in a way that removed foregone match, 17% chose to re-allocate but still did not maximize the match, while 9% stated they did not want to re-allocate their contributions.

We use the analysis summarized in Table E3 to characterizing the nature of foregone match as either 'deliberate' or 'accidental'. If respondents either answered that that they did realize that they had not maximized their match ($n = 159$), or that they did not realize but declined the opportunity to reallocate their match ($n = 21$), we categorize them as having deliberate foregone

Table E3: Foregone Match Types

	N	Proportion
A: Did you realize foregone match? ($N = 393$)		
Yes	159	40.5
No	230	58.5
I don't understand	4	1.0
B: Offered chance to re-allocate ($N = 230$)		
Reallocated to remove foregone match	170	73.9
Reallocated but did not remove foregone match	39	17.0
Did not wish to reallocate	21	9.1

Notes: Panel A divides the sample of who have no foregone match into the share that i) stated that realized that their choices did not maximize the match, ii) the share that did not realize that their choices did not maximize the match, and iii) the share who stated that they did not understand the question. Panel B divides the sample of those who stated that they did not realize that they had foregone match into shares that i) chose to reallocate their contributions and had no foregone match in their second choice, ii) chose to reallocate and again had foregone match in their second choice, and iii) chose not to reallocate their contributions.

match. If respondents did not realize that that they had foregone match and reallocated to maximize their match ($n = 170$), we characterize them as having accidental foregone match. If respondents stated that they did not understand the question about whether they realized they had foregone match, we categorize them as 'other' ($n = 4$). This leaves the 39 respondents who stated they did not realize that they initially had foregone match, chose to reallocate, but then chose another allocation that also did not maximize the match. In most of these cases we consider that they have 'other' foregone match. However, we inspected the narrative explanation given by this group of respondents. In 7 cases each, the narrative explanation suggests it is either deliberate or accidental, and we characterize these as such.[58] In the remaining 25 cases, the narrative explanations were not sufficient to categorize the foregone as either deliberate or accidental. These 25 cases, and the 4 cases in which respondents stated that they did not understand the question of whether they realized that they had foregone match are the 29 responses categorized as 'Other cases'.

[58] An example of a narrative explanation which we categorized as indicative of deliberate foregone match is "I still think that there should be some money in each of the retirement accounts. You never know what could happen and do not want to be left with nothing." An example of a narrative explanation which we categorized as indicative of accidental foregone match was "Oops made a mistake".

Table E4: Characterizing Foregone Match

	N	Prop. of Total	Prop. of FM
Deliberate foregone match	187	18.9	47.6
Accidental foregone match	177	17.9	45.0
Other foregone match	29	2.9	7.4
Total	393	39.8	100.0

Notes: Those in the deliberate foregone match category are i) those who stated that they realized they were making a choice which did not maximize their match, ii) those who did not realize this, but after being informed, chose not to change their allocation, and iii) those who chose to reallocate their contributions, did not remove foregone match in the reallocation and had a narrative explanation which implied this was an intentional decision. Those in the accidental foregone match are i) those who did not realize that they did not maximize their match, but after being informed of this changed their allocation to maximize the match, ii) those who chose to reallocate their contributions, did not remove foregone match in the reallocation and had a narrative explanation which implied this was an accidental decision. Those in the 'other' foregone match category are i) those who did not understand the question about whether they realized they had foregone match and ii) those who chose to reallocate their contributions, did not remove foregone match in the reallocation and had a narrative explanation which did not indicate clearly whether not maximizing the match was intentional or accidental.

E.4.3 Foregone Match is more likely when the spouse's account offers the more favorable match

Table E5 shows how the rates of foregone match differ across the three versions of the choice allocation problem. In both the 'Max via spouse' and 'Max via own' versions, the strategy that maximizes the match is to place all savings ($3,000) into one account. In the former, all savings should be placed in the spouse's account, while in the latter all savings should be placed in the respondents' account. While there is little difference in the rates of *accidental* foregone match (13.7% versus 16.7%) across these two versions of the vignette, the proportion of respondents with *deliberate* foregone match is almost twice as high when the match-maximising strategy is to put all the savings into the spouse's account (relative to the setting in which the match-maximizing strategy is to put all the savings into one's own account).

In the third version, our 'Max via split' version, the match-maximizing strategy is to split the saving (with $2,000 going to the spouse's account and $1,000 into the respondents account). In this version, we observe higher rates of accidental forgone match (23.5%) than in the other two cases, and levels of deliberate foregone match (14.0%) that are similar to the 'max via own' case and smaller than the 'max via spouse case'. The higher rate of accidental foregone match could reflect that, as a savings problem where the solution is to split allocations, the match-maximizing problem is a more difficult one. The lower rate of deliberate foregone match could reflect that a lesser aversion to sharing contributions by splitting than to placing all in one account.

Table E5: Foregone Match, by Vignette Type

	(1) Prop. w/ any foregone match	(2) Prop. w/ deliberate foregone match	(3) Prop. w/ accidental foregone match	(4) Prop. w/ other foregone match
Max via spouse	43.6	28.1	13.7	1.8
Max via own	34.4	14.2	16.7	3.5
Max via split	41.2	14.0	23.5	3.7

Notes: This table shows the proportion with foregone match, and foregone match of each type (deliberate, accidental, or other) by the vignette type. In the 'Max via spouse' vignette, the match-maximizing strategy was to contribute all funds to the account of the spouse. In the 'Max via own' vignette, the match-maximizing strategy was to contribute all funds to the account of the respondent. In the 'Max via split', the match-maximizing strategy was to allocate two thirds of the contributions to their spouse's account, and one third to their own account.

E.4.4 Women are more likely to have foregone match than men

Table E6 shows that women are more likely to have foregone match than men. There are particularly striking differences in deliberate foregone match by the type of vignette question. Among respondents who received the survey question where maximizing the match requires putting all the savings in the spouse's account, female respondents are more likely to have deliberate foregone match than male respondents (i.e., 31.5% of women and 24.7% of men chose deliberate foregone match in this scenario). In contrast, when achieving efficiency does not require giving up control over the savings, we find little differences in deliberate foregone by gender (i.e., 14.3% of women and 14.1% of men deliberate chose an allocation with foregone match when the efficient allocation requires putting all the savings in their own account).

Table E6: Foregone Match, by Gender and Vignette Type

	Prop. w/ any foregone match		Prop. w/ deliberate foregone match		Prop. w/ accidental foregone match	
	Male	Female	Male	Female	Male	Female
Max via spouse	38.5	48.8	24.7	31.5	12.6	14.9
Max via own	29.5	38.7	14.1	14.3	12.1	20.8
Max via split	36.3	46.5	12.9	15.3	18.7	28.7
Total	35.0	44.6	17.4	20.5	14.6	21.3

E.4.5 There is incomplete and often erroneous knowledge of implications of divorce for assets, and this matters for saving allocations

Wealth in a retirement account is a marital asset, and as such will be divided on divorce.[59] Despite this, as column (1) of Table E7 shows, over a third of couples think that, on divorce they would keep the funds in their own retirement account and close to a fifth don't know. Close to half of respondents say that they would split the assets, or that they would be divided in some other manner. The proportion who recognize that retirement wealth is liable to be split is lower than the proportions who answer that individual checking and savings accounts will be split (57%), joint checking and savings accounts (75%), and housing wealth (71%). What we are picking up therefore, is a general tendency to underestimate the extent to which wealth accumulated during a marriage will be split, a tendency which is particularly acute in the case of retirement wealth (perhaps due to the fact that these are accounts are in one spouse's name and attached to their job).

Columns (2), (3), (4), and (5) show, conditional on a particular answer to the question, what proportion of respondents have any foregone match, deliberate foregone match, accidental foregone match, or other foregone respectively. There is a clear pattern. Those who think that accumulated retirement wealth remains one's own property on divorce are much more likely to have foregone match, than those who think such wealth with be split. The difference comes largely from a tendency to have substantially greater deliberate foregone match. It is clear that, for a share of the population, placing wealth in one's own account is viewed as a device to protect one's own wealth in case of divorce.

Table E7: Knowledge of Divorce Law and Association with foregone Match

	(1) Prop. of sample	(2) Prop. w/ any foregone match	(3) Prop. w/ deliberate foregone match	(4) Prop. w/ accidental foregone match	(5) Prop. w/ other foregone match
Keep own	34.2%	51.2%	27.8%	19.2%	4.1%
Split/Other	46.9%	36.9%	15.3%	19.0%	2.6%
Don't know	18.8%	26.3%	11.8%	12.9%	1.6%

Notes: Column (1) shows the distribution of respondents by their beliefs about the division of assets in divorce. Columns (2) to (5) give the proportion with foregone match, and foregone match of each type (deliberate, accidental, or other) by a respondent's divorce law belief.

[59] States differ between those which are Community Property states, in which assets acquired during marriage tend to divided equally and 'Equitable Division' states, there is some judicial discretion to the splitting of assets. However, even in those states, the particular spouse who remitted the payment should not be relevant to the division.

E.4.6 The incidence of foregone natch falls with Financial Literacy

Table E8 summarizes the relationship between Financial Literacy and foregone match. Column (1) gives the proportion of our sample in each of five groups: those who get two or fewer questions correct, those who get 3, 4, and 5 questions correct. Foregone match falls steeply with Financial Literacy, and this is true for both Deliberate and Accidental Foregone match.

Table E8: Financial Literacy

	(1) Prop. of sample	(2) Prop. w/ any foregone match	(3) Prop. w/ deliberate foregone match	(4) Prop. w/ accidental foregone match	(5) Prop. w/ other foregone match
≤ 2	17.4	64.0	29.1	29.1	5.8
3	22.1	49.1	27.1	18.8	3.2
4	36.9	32.1	14.6	15.9	1.6
5	23.6	25.3	10.7	12.0	2.6

Notes: Column (1) shows the distribution of respondents by the number of questions that they got right. Columns (2) to (5) give the proportion with foregone match, and foregone match of each type (deliberate, accidental, or other) by the number of questions that they got right.

E.4.7 A substantial fraction of couples with DC plans have not considered the fact that there might be gains from reallocation

For the approximately one third of the sample (346 respondents) who stated that both they and their spouse had a retirement plan, and that both plans were Defined Contribution plans, we evaluated whether they were aware that there might be gains from coordinating. Respondents were asked to select from options outlined in Section E.2.6. Table E9 gives the number and proportion who selected each option (in columns (1) and (2)), and gives the proportion of respondents with foregone match (and its categories) for each group (in columns (3) to (6)). We take two lessons from this analysis. The first is about the coherence between stated real world decisions and hypothetical responses to our vignette. Those who say they are coordinating in reality have the lowest levels of foregone match in the hypothetical choice problem, while those who say they are not coordinating in reality have the highest levels. Those who had considered the potential of gains from coordinating have substantially higher accidental foregone match than those who had considered it. The second lesson is that a substantial proportion of this sample (36.5%), who have retirement plans, had not considered the fact that their might be gains from coordination. We take this as evidence that many couples might not be aware of the stakes to cooperation.

Table E9: Have you considered the gains to co-ordination

	(1) N	(2) Prop. of sample	(3) Prop. w/ any FM	(4) Prop. w/ deliberate FM	(5) Prop. w/ accidental FM	(6) Prop. w/ other FM
Considered; are coordinating	137	39.6	21.2	8.8	11.7	0.7
Considered; maybe coordinating	33	9.5	45.5	24.2	18.2	3.0
Considered; not coordinating	50	14.5	58.0	40.0	14.0	4.0
Not considered; still won't	50	14.5	44.0	22.0	20.0	2.0
Not considered; now will	76	22.0	55.3	18.4	34.2	2.6
Total	346	100.0				

Notes: The sample for this table are the 346 respondents who stated that they have a DC retirement account and that their spouse has a DC retirement account. Columns (1) and (2) shows the number and proportion of respondent by their answer to the question of whether they have considered that there might be gains to coordination. Columns (3) to (6) give the proportion with foregone match, and foregone match of each type (deliberate, accidental, or other) by their response to this question.

E.4.8 Regression analysis of correlates of foregone match

In this sub-section we show the association between a variety of other characteristics and foregone match.

We divide our characteristics into three categories:

1. The first are measures of divorce law beliefs and financial literacy, for which we showed correlations with foregone match in sections E.4.5 and E.4.6 respectively.

2. The second are demographic characteristics: gender, age, own earnings, and spouse earnings.

3. The third are our closest survey analogues to the characteristics which we assumed proxied for marital commitment in Section 5.3.1. These are whether the respondent and their spouse primarily uses a joint bank account, whether they have children, whether they have a mortgage, and their marriage length.

Table E10 shows results for regressions of an indicator of having any foregone match on each of these three groups of characteristics by themselves (in columns (1), (2), and (3)), and all characteristics (in column (4)). Table E11 shows just the saturated regression for each of any foregone match (repeating the last column of Table E10, and regressions with the dependent variable as an indicator for deliberate foregone match (column 2) and accidental foregone match (column 3).

Table E10: Regression of Foregone Match Indicator

	(1)	(2)	(3)	(4)
FL: 3 Correct	-0.125			-0.130
	(0.049)			(0.049)
FL: 4 Correct	-0.283			-0.272
	(0.044)			(0.045)
FL: 5 Correct	-0.367			-0.342
	(0.048)			(0.050)
Divorce (Split)	-0.109			-0.100
	(0.034)			(0.035)
Divorce (Don't Know)	-0.248			-0.253
	(0.044)			(0.044)
Women		0.092		0.028
		(0.034)		(0.033)
Age 30s		0.024		0.008
		(0.050)		(0.052)
Age 40s		0.051		0.024
		(0.053)		(0.062)
Age 50s		0.063		0.056
		(0.058)		(0.071)
Earnings Quart. 2		-0.041		-0.012
		(0.042)		(0.040)
Earnings Quart. 3		-0.137		-0.090
		(0.044)		(0.042)
Earnings Quart. 4		-0.113		-0.047
		(0.052)		(0.050)
Spouse Quart. 2		0.055		0.017
		(0.041)		(0.039)
Spouse Quart. 3		-0.003		-0.023
		(0.048)		(0.046)
Spouse Quart. 4		-0.029		-0.048
		(0.049)		(0.047)
Marriage Length 5-9			0.040	0.043
			(0.046)	(0.047)
Marriage Length 10-18			0.050	0.037
			(0.049)	(0.054)
Marriage Length 19+			0.072	0.061
			(0.054)	(0.067)
Has Joint Account			-0.119	-0.084
			(0.035)	(0.033)
Has Children			0.068	0.080
			(0.040)	(0.038)
Has Mortgage			-0.115	-0.101
			(0.038)	(0.037)
Constant	0.708	0.358	0.466	0.740
	(0.040)	(0.055)	(0.040)	(0.069)
N	943	943	943	943

Notes: The sample for this regression is comprised of the observations for which we have non missing observations for each of the covariates. The dependent variable in the regression in each column is whether the respondent has foregone match in the hypothetical choice problem. The results in column (1) are from a regression in which just financial literacy dummies and divorce law belief dummies are included. The results in column (1) are from a regression in which just gender, age, and own and spouse earnings dummies are included. The results in column (3) are from a regression in which dummies for marriage length, whether the couple has a joint account, whether they have children, and whether they have a mortgage are included. The results in column (4) are from a regression in which all characteristics are included. Omitted groups are as follows. getting two or fewer questions correct; thinking that one keeps one's own retirement account in divorce; male; being in one's 20s; being in the bottom quartile of own and spouse earnings; being married for less than 5 years; not having a joint account, not having children, and not having a mortgage.

Table E11: Types of Foregone Match

	(1) Prop. w/ any foregone match	(2) Prop. w/ deliberate foregone match	(3) Prop. w/ accidental foregone match
FL: 3 Correct	-0.130	-0.029	-0.092
	(0.049)	(0.040)	(0.040)
FL: 4 Correct	-0.272	-0.134	-0.114
	(0.045)	(0.037)	(0.037)
FL: 5 Correct	-0.342	-0.175	-0.153
	(0.050)	(0.041)	(0.041)
Divorce (Split)	-0.100	-0.110	0.022
	(0.035)	(0.029)	(0.029)
Divorce (Don't Know)	-0.253	-0.159	-0.071
	(0.044)	(0.036)	(0.036)
Women	0.028	-0.025	0.055
	(0.033)	(0.027)	(0.027)
Age 30s	0.008	0.009	-0.015
	(0.052)	(0.043)	(0.043)
Age 40s	0.024	0.023	-0.023
	(0.062)	(0.051)	(0.052)
Age 50s	0.056	0.019	-0.004
	(0.071)	(0.059)	(0.059)
Earnings Quart. 2	-0.012	-0.015	0.009
	(0.040)	(0.033)	(0.033)
Earnings Quart. 3	-0.090	-0.088	-0.001
	(0.042)	(0.035)	(0.035)
Earnings Quart. 4	-0.047	-0.065	0.028
	(0.050)	(0.041)	(0.041)
Spouse Quart. 2	0.017	0.019	-0.012
	(0.039)	(0.032)	(0.032)
Spouse Quart. 3	-0.023	0.017	-0.030
	(0.046)	(0.038)	(0.038)
Spouse Quart. 4	-0.048	-0.004	-0.038
	(0.047)	(0.039)	(0.039)
Marriage Length 5-9	0.043	0.019	-0.008
	(0.047)	(0.039)	(0.039)
Marriage Length 10-18	0.037	0.052	-0.040
	(0.054)	(0.045)	(0.045)
Marriage Length 19+	0.061	0.038	0.016
	(0.067)	(0.055)	(0.056)
Has Joint Account	-0.084	-0.005	-0.058
	(0.033)	(0.028)	(0.028)
Has Children	0.080	0.036	0.044
	(0.038)	(0.032)	(0.032)
Has Mortgage	-0.101	-0.067	-0.046
	(0.037)	(0.030)	(0.030)
Constant	0.740	0.390	0.326
	(0.069)	(0.057)	(0.057)
N	943	943	943

Notes: The sample for this regression is comprised of the observations for which we have non missing observations for each of the covariates. The dependent variable in the regression in column (1) is whether the respondent has any foregone match. The dependent variable in the regression in column (2) is whether the respondent has deliberate foregone match. he dependent variable in the regression in column (3) is whether the respondent has accidental foregone match. Omitted groups are as follows. getting two or fewer questions correct; thinking that one keeps one's own retirement account in divorce; male; being in one's 20s; being in the bottom quartile of own and spouse earnings; being married for less than 5 years; not having a joint account, not having children, and not having a mortgage.