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ABSTRACT

We study how couples allocate retirement-saving contributions across each spouse's account. In a new dataset covering over a million U.S. individuals, we find retirement contributions are not allocated to the account with the highest employer match rate. This lack of coordination—which goes against the assumptions of most models of household decision-making—is common, costly, persistent over time, and cannot be explained by inertia, auto-enrollment, or simple heuristics. Complementing the administrative evidence with an online survey, we find that inefficient allocations reflect both financial mistakes as well as deliberate choices—especially when trust and commitment inside the households are weak.

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1 Introduction

Most households are formed of multiple people. To study their decisions, economists must take a stance on how members of the same household resolve their conflicting desires. Whether households' decision-making results in efficient outcomes—where no resources are wasted in any given period—is not just a core assumption of many influential models of intra-household decision-making; it also affects their ability to take full advantage of the economic opportunities available to them.

This paper offers a transparent non-parametric test of the efficiency of married couples' decisions in a high-stakes setting: saving for retirement. Nearly two-thirds of U.S. workers have access to an employer-sponsored Defined Contribution retirement saving plan (Myers and Topoleski (2020)), and over four-fifths of these plans offer an employer 'match' (Arnoud et al. (2021)). Because match schedules vary across employers, the financial incentives for saving differ across each spouse's account. We ask whether married couples allocate their retirement contributions to efficiently exploit the match incentives available at the household level. For instance, if one spouse has a dollar-for-dollar employer match up to a cap, and the other spouse has a 50 cents-on-the-dollar match, then the efficient allocation at the household level is to fully exploit the match offered to the first spouse before making any contribution to the second spouse's account.

To study this question, we create a new data set of employer-sponsored retirement plan characteristics. This employer data set is generated by hand-coding narrative descriptions contained in the annual regulatory filings of over 6,000 DC retirement plans, covering approximately 40 million employees. We link this employer data set to administrative records on employee retirement saving choices. Our employee data comes from IRS tax returns filed by individuals (which allow us to link spouses together) and W-2 forms filed by employers (which report yearly contributions by each employee to these plans). In this matched employer-employee dataset, match schedules vary substantially across employers, which creates an ideal laboratory to study the efficiency of households' financial decisions. The incentives created by the employer match are large and easily measurable and the efficient allocation for a couple that achieves efficiency can be clearly defined. The setting that we study generates favorable conditions for coordination to emerge; for example, retirement assets are considered marital property in the case of a divorce.

We find that close to 20% of couples in our sample fail to exploit a within-period intra-household arbitrage condition. That is, these couples could increase their retirement wealth without changing their consumption (or increase their consumption at no cost to retirement wealth) by simply reallocating existing contributions from the account of the spouse with a lower marginal match incentive to the account of the spouse with a higher marginal match incentive. The roughly four-fifths of couples who do not fail this efficiency test are not necessarily actively coordinating: they may happen to (independently) choose individual contributions that are consistent with household-level efficiency (e.g. both could be fully exploiting their employer match). To provide a benchmark for the extent of inefficient allocations absent any coordination between spouses, we compare real couples to randomly drawn pairs of individuals with similar observable characteristics. In these placebo samples—in which there is (by construction) no coordination—we find that only 33%-34% of placebo couples fail to exploit an available arbitrage opportunity. Relative to these benchmarks, our finding of 19.3% of married couples having some foregone match is consistent with some couples actively coordinating their contributions and a majority not doing so.

The mean and median levels of foregone match for couples who fail to exploit the intra-household arbitrage opportunity are substantial, at $757 and $383 per year, respectively. In addition, inefficiency is persistent: more than half of couples with an inefficient allocation in a given year, still allocate their savings inefficiently four years later. Relative to a counterfactual in which all couples saved the same amount but allocated their contributions efficiently, we estimate in a simple simulation exercise that non-coordination throughout the lifecycle can lower wealth at retirement by an average of approximately $14,000.

Next, we explore mechanisms, and evaluate the extent to which the lack of coordination we document is driven by deliberate non-coordination and the extent to which is due to mistakes. We complement our administrative data with a survey in which married people were asked to make a hypothetical allocation between their account and their spouse's account, which has a different match rate. For the 40% of survey respondents who do not maximize the overall match their couple could have achieved, we follow up with a set of questions designed to evaluate whether the decision was a deliberate one (i.e., respondents *knowingly* left money on the table) or an accidental one (indicated by respondents wishing to reallocate their contributions to maximize the match once we alert them to the implications of their choice). We find that approximately half of the incidence of foregone match in our survey can be characterized as deliberate and half as accidental.

For drivers of accidental foregone match, using our administrative data, we find no evidence that rational inattention, inertia, or simple savings allocation heuristics explain the non-coordination that we document. Couples do not systematically improve efficiency when they make active savings decisions–evidence against inertia driving our results. We do find evidence of the use of a savings heuristic whereby couples equalize their contributions, but this strategy acts to facilitate efficiency rather than driving inefficiency. We also show that non-coordination is insensitive to the stakes of coordination and that it persists even when there is more than \$6,000, or 5% of joint earnings, at stake. We interpret this finding as evidence that our results are not driven by rational inattention. However, using our survey we show that inattention (albeit perhaps not of a rational type) is a contributing factor: the vast majority of those who had hypothetical foregone match without realizing it re-allocated their contributions once we alerted them to the implications of their choice. Additionally, among the respondents to our survey for whom both they and their spouse have DC plans in real life, over a third stated that they had not considered that there might be gains to coordination.

Turning to drivers of deliberate foregone match, we note that Chiappori and Mazzocco (2017) suggest that efficient coordination can be facilitated by the "proximity and durability of the relation". Consistent with this mechanism and using our administrative data, we find that plausible proxies for marital commitment are correlated with efficient coordination. Conditional on a couple's observable characteristics, the incidence of foregone match is higher for couples who our data show will subsequently divorce and is lower for couples who we observe to have a joint bank account in the years before marriage. Given that wealth in retirement accounts is a marital asset, this could be seen as a puzzle: even if divorce is *certain*, there are incentives to avail of the arbitrage opportunity. Our survey reveals, however, that close to a fifth of couples say that they do not know how their retirement wealth would be divided on divorce, and over a third believe that they would keep their own accounts. Those who believe the latter are also substantially more likely to have foregone match in the hypothetical choice problem than those who believe retirement wealth is split on divorce.

We make contributions to two branches of the literature. First, our paper is related to a large theoretical and empirical literature on intra-household decision-making. On the theoretical side, both the unitary model and the broader class of collective household models, make the minimally-restrictive assumption that decisions made in each time period are Pareto Efficient – that is, no available resources are wasted within the household (see Chiappori and Meghir (2015) and Chiappori and Mazzocco (2017) for reviews).[1] Our results of widespread inefficiency are suggestive of a

[1]Dynamic implementations of the collective model (e.g. Ligon (2002) and Mazzocco (2007)) confront the fact that individuals cannot commit to future behavior. These models admit outcomes that are inside the Pareto Frontier which would be attainable if couples could fully commit to future actions. But they still retain the property that in every period no surplus is left on the table. It is this (weaker) notion of ex-post, or static, efficiency that we test.

greater role for intertemporal models of the household which do not assume that couples achieve ex-post efficiency in every period. For example, Basu (2006) and Hertzberg (2016) develop models in which equilibrium behavior leads to inefficient outcomes. Models have also been proposed in which there exists a non-cooperative threat-point in the bargaining problem (see for example Lundberg and Pollak (1993), Del Boca and Flinn (2012) and Browning et al. (2010)). In such models, couples may behave inefficiently when the threat-point is realized.

On the empirical side, a large number of studies have tested the efficiency of decisions using household data from many developed and developing countries (Bourguignon et al. (1993) Chiappori et al. (2002), Blundell et al. (2007), Cherchye et al. (2009), Dauphin et al. (2011), Attanasio and Lechene (2014), LaFave and Thomas (2017) and Bargain et al. (2022)). While results and approaches differ between papers, most of these studies have failed to reject the efficiency of married couples' decisions using survey data of consumption. Dauphin et al. (2018) suggests that these consumption tests in small samples may be underpowered to reject household efficiency.

A number of papers have tested the efficiency of household investment decisions in developing countries and found more mixed results in both observational and field-experimental data. Udry (1996) finds that agricultural plots controlled by women in Burkina Faso are farmed less intensively than plots controlled by their husbands, and that a reallocation of inputs could be Pareto Improving.[2] However, other interpretations consistent with household efficiency have been offered: differences in investments intensity could reflect gender differences in the security of land rights (Goldstein and Udry (2008)) or measurement error (Rangel and Thomas (2019)). These empirical challenges can be overcome in field experiments. While experimental evidence supporting efficiency exists (e.g., Bobonis (2009)), there is also ample experimental evidence of inefficient outcomes. Ashraf (2009), Schaner (2015), and Almås et al. (2018) all find evidence of spouses preferring lower levels of resources that they control over higher levels of resource with shared control.[3] These field-experimental results provide powerful evidence that inefficiencies can occur, but the extent to which these findings extend to naturally-occurring high-stakes financial decisions in which cooperation could emerge after repeated interactions remains an open question.

Our contribution to that literature is to implement a transparent non-parametric test of household-level efficiency in a naturally-occurring setting, in which the incentives are directly measurable to the researcher and relatively simple to the individuals (i.e., unlike with agricultural investments,

[2]See Walther (2018) for similar recent evidence from Malawi.

[3]There is additional experimental evidence of inefficient household decision-making in settings other than saving choices. For example, see recent work on information aggregation in India (Conlon et al. (2021)).

the rate of return is given by a straightforward deterministic formula). The substantial level of inefficiency that we document is all the more notable given that our setting creates favorable conditions for cooperation to emerge: (i) our sample of married tax filers with access to two employer-sponsored DC accounts earns approximately twice as much as the average U.S. household and, as such, is presumably more educated and financially literate; (ii) retirement saving is a repeated decision, and couples have time to learn and build familiarity with the setting, and (iii) retirement assets are relatively illiquid before retirement and, across all U.S. states, are divided independently of who made the contribution in the case of a divorce, which should alleviate commitment frictions.

We also contribute to a growing literature on sub-optimal financial decision-making by households. This includes evidence that households do not locate their financial assets efficiently across their taxable and non-taxable savings accounts (Bergstresser and Poterba (2004)), fail to refinance a fixed-rate mortgage when it is beneficial to do so (Andersen et al. (2020)), and co-hold low-interest liquid savings and high-interest rate credit card debt (Gross and Souleles (2002)). While some of these apparent mistakes could be rationalized depending on household circumstances, evidence of unambiguous financial mistakes (e.g., no-arbitrage violations) is more limited. Leading findings include older workers not taking advantage of a 401(k) match despite being eligible for immediate penalty-free withdrawals (Choi et al. (2011)) and credit card borrowers not prioritizing the repayment of the balance with the highest interest rate Gathergood et al. (2019).[4] We add to this literature by documenting a new instance of inefficient financial behavior with a large and persistent cost. The average annual cost of inefficient behavior in our setting is an order of magnitude larger than in the credit card setting studied by Gathergood et al. (2019)) and affects a larger share of the population that the setting studied by Choi et al. (2011) (which is limited to workers older than 59.5 in plans that do not restrict in-service withdrawals). We show a relationship between the non-coordination we document and financial decision-making, and so contribute to a burgeoning literature which has linked poor financial literacy to sub-optimal decision-making (see Lusardi and Mitchell (2023) for a recent review).

The paper proceeds as follows. Section 2 formalizes our test of efficiency and motivates our empirical approach. Section 3 describes our data. Section 4 contains our results on the incidence of non-coordination. Section 5 investigates the drivers of these patterns. Section 6 concludes.

[4] Goodman et al. (2023) find another example of an unambiguous mistake in retirement saving: 0.4% of retirement-age savers forgot about or abandoned their IRA accounts.

2 Framework and Empirical Approach

In this section, we motivate and formalize our non-parametric test of Pareto Efficiency.

2.1 Framework

We consider a couple with members $i \in \{A, B\}$, who live for two periods $t \in \{1, 2\}$. Each member of the couple earns an income in the first period y_1^i and no income in the second period. Saving done by individual i in period 1 (s^i) yields a level of wealth available for consumption in period 2 of $R \times (s^i + m^i(s^i))$, where $R \times (s^i + m^i(s^i))$ is the technology that converts saving to wealth. This comprises a gross investment return R, assumed identical between the members of the household, and the employer match $m^i(s^i)$, which may differ between the two spouses and may be nonlinear.

The household chooses consumption and saving for each individual in each period, subject to an intertemporal budget constraint:

$$\sum_{i=A,B} \left(c_1^i + c_2^i \right) \leq \sum_{i=A,B} \left((y_1^i - s^i) + R \times \left(s^i + m^i(s^i) \right) \right) \tag{1}$$

Household preferences could be characterized using a single utility function (as in a unitary model) or as a weighted sum of individual utility functions (as in a collective model of Chiappori (1988) and Browning and Chiappori (1998)). These are the two workhorse models that have been used for empirically studying household decision-making.[5] In either case, however, as long as these utility functions are always increasing in consumption, and the framework is one which assumes 'ex-post' (or 'static') efficiency[6], the aggregate saving in the first period must be done in a fashion that maximizes the employer match. That is, letting S be the total amount of saving ($s^A + s^B$), the optimal allocation of saving across spouses $\{s^{*A}(S), s^{*B}(S)\}$ must satisfy:

$$\{s^{*A}(S), s^{*B}(S)\} \in \arg\max \; m^A(s^A) + m^B(s^B) \quad \text{s.t.} \quad s^A + s^B \leq S \tag{2}$$

[5]The unitary model implies that proxies of spouses' bargaining power do not affect household choices. Evidence exists that they do (Schultz (1990), Thomas (1990), Lundberg et al. (1997), Aura (2005), Addoum (2017), Cesarini et al. (2017)). The collective model can accommodate this in a framework which assumes efficiency. Recent papers in the collective spirit include the study of the impact of divorce laws (Voena (2015), Reynoso (2020)), educational choices by gender (Bronson (2014)), the interplay between marriage, education, and labor supply (Chiappori et al. (2018)), welfare reform (Low et al. (2018)), portfolio choice (Gu et al. (2021)), fertility decisions (Low (2017), Doepke and Kindermann (2019)), intra-household allocation (Lise and Yamada (2019)), inter-temporal discounting (Adams et al. (2014)), joint leisure choice (Cosaert et al. (2022)) and sorting in the marriage market (Calvo et al. (2021)).

[6]Limited commitment models (e.g. Mazzocco (2007)), which assume individuals cannot commit to future behavior, admit outcomes that are ex-ante inefficient (that is, they are inside the Pareto Frontier which exists at the time the couple forms) but retain the assumption that, in each time period, no surplus is left on the table.

We can define the excess of maximum possible match over the actual match as the "foregone match" (FM):

$$FM = \Big(m^A\big(s^{*A}(S)\big) + m^B\big(s^{*B}(S)\big)\Big) - \Big(m^A\big(s^A\big) + m^B\big(s^B\big)\Big) \tag{3}$$

The condition $FM = 0$ is testable given variation within households in the marginal savings technology of individuals, equal to $R\frac{dm^i}{ds^i}$. Our setting provides such variation, both because spouses may face different match schedules m and because these match schedules are, in general, nonlinear.

2.2 Empirical Approach

Our test of household efficiency requires us to measure the amount of foregone match for each household in a sample. The empirical requirements for such an exercise are a dataset which i) links individuals in married couples; ii) contains details of saving at the *individual* level s^A, s^B, and iii) measures match schedules, also at the *individual* level $(m^A(.), m^B(.))$. With such a dataset, which we create, calculation of FM using equations (2) and (3) is straightforward. We report results using these calculations in Section 4. Finding $FM > 0$ for a particular couple indicates that a couple is not allocating contributions in an efficient fashion.

2.3 The role of divorce and death

While the framework outlined does not incorporate marital separation (in the form of divorce or death), the legal system offers strong protections for spouses after such events. These protections create strong incentives for spouses to coordinate their contributions even when facing the threat of separation. Taking death first, under the Retirement Equity Act (1984), a spouse must be the beneficiary of the DC plan, unless they provided written consent to waive their entitlement. As regards divorce, across all U.S. states, the disposition of retirement accounts, in the absence of a pre-nuptial agreement, is not influenced by which spouse made contributions. There is no direct influence because it is generally assumed that married parties cooperate as a partnership in determining which roles each spouse should take on to maximize their income, and thus, each spouse should share equitably in the accumulated assets. This implies that couples have an incentive to maximize their joint retirement wealth even if they are certain to divorce later on.

2.4 Incentives to prefer contributions to own account

Even if the division of retirement wealth on divorce in the US is generally unaffected by who made the contribution, there could remain strategic reasons for a spouse to prefer to contribute to their own account, rather than their spouse's account with a higher match. First, a pre-nuptial agreement might have been signed, carving out retirement wealth as personal property (but, arguably, such agreements could be revised after the marriage—with a post-nuptial agreement—to ensure the couple can take advantage of available arbitrage opportunities). Second, understanding of divorce law might be imperfect (and in Section 5.3.2, we provide survey evidence that it is). Third, having funds in one's own account might bring a greater influence over the use of the funds in retirement, or more power in the marriage more generally. Such strategic considerations could rationalize a spouse saving at a match which is lower that the potential match available to their spouse.

Strategic considerations by themselves do not mean that two spouses could not profitably benefit from this arbitrage opportunity. Consider spouse A, with a match rate of m^A in period 1, who wishes to have a saved amount $(1 + m^A)s$ available in period 2; she can achieve this by saving s in her account. If spouse B has a higher (unused) match rate of m^B, then spouse A could generate a surplus $(m^B - m^A)s$ by contributing s to spouse B's account instead of her own. This surplus can be divided between the two spouses, leaving both of them strictly better off in period 1. For this trade to be Pareto-optimal, spouse B must then transfer back the savings $(1 + m^A)s$. Enforcing these transfers in period 2 could be done using a post-nuptial or other contractual arrangement or—within a repeated game—through a punishment strategy of spouse A that enforces cooperation. In some relationships, marital commitment will be sufficient to enforce coordination, in others perhaps not, and below we document positive correlations between plausible proxies for commitment and cooperation.

3 Data

Our administrative data bring together newly-constructed employer data on retirement plan characteristics – matching schedules, vesting schedules and 'auto-features' – and employee data on retirement saving. The next two subsections discuss each in turn.

3.1 Retirement Plan Data

The Employee Retirement Income Security Act (1974) is a federal law which governs the provision of employee benefits, including retirement plans. Compliance with the Act requires an annual report from all firms with retirement plans. The reporting involves submitting a Form 5500, which reports, for example, the type of plan offered (Defined Benefit or Defined Contribution), the total number of participants, aggregate employer contributions, and aggregate employee contributions. Match schedules and vesting schedules are not collected as part of the regulatory form, but plans with more than 100 participants are also obliged to submit an auditor's report which contains a narrative 'Description of the Plan', which must describe in free form text, amongst many other details, the plan's match schedules (if any), its vesting schedules (if any), and its auto-features (if any). These narrative retirement plan descriptions are publicly available to download.[7]

To create a new data set, we extract and hand-code matching schedules, vesting rules, and auto-features from these narrative descriptions. Full details are given in Appendix A.1; briefly, the approach involves first finding the relevant key passages by identifying key words and phrases (e.g., 'Description of the Plan', 'matching', 'vesting' etc.) and then extracting the relevant pages for a sample of firms before finally reading the retirement plans and codifying them into a dataset.

The retirement plan data used in this paper is formed by codifying the plan characteristics of over 6,000 401(k) and 403(b) plans in the U.S. The bulk of this sample is comprised of the largest approximately 5,000 plans, where we define size of plan as the mean number of participants over the period from 2003 to 2018. We also codified the details of a random sample of smaller plans. Our data is longitudinal – we have hand-collected retirement plan characteristics for each year over that period, yielding over 70,000 plan year observations.[8] A concern with any hand-collected data is that it could be contaminated by measurement error. In Appendix B, we provide several pieces of evidence that our results are not driven by measurement error.

The three key retirement plan features on which we collect data are match schedules, vesting schedules and auto-features. We also collect data on whether a single schedule of plan details applies to all members or whether different plan features are offered to different categories of worker: our linking of employee to plan requires that all employees have access to the same plan and we define our analysis sample accordingly. Matching schedules, the piece of retirement plan data at the heart

[7] `www.efast.dol.gov/5500search/`

[8] For more on this process and our new data set see Arnoud et al. (2021) which uses a subset of the data. Other papers that codify details in the Form 5500 narratives include Bubb and Warren (2020) and Rauh et al. (2020).

of our test of efficiency, are piece-wise linear functions which determine the contribution employers make to their employees' accounts. Figures A1 andA2 illustrate these match schedules.

3.2 Combining with Employee Savings Data

Our data on earnings and retirement plan contributions comes from tax return data. In particular, Form W-2 identifies the DC contributions made by a given employee at a particular job (identified by the EIN on the W-2). We develop a crosswalk to map from EINs reported on Form 5500 to EINs reported on Form W-2; Appendix A.2 gives further details.

For our empirical approach, it is very important to correctly match a given worker to a given DC plan, so we drop firms that substantively offer more than one plan – e.g., one plan for a certain class of employees and another plan for a different class.

3.3 Defining our population

The population we study is married couples in the U.S. who satisfy four restrictions. First, they must file a tax return jointly. Second, both spouses must have positive wages. Third, both spouses must be employed at firms that offer an employer-sponsored Defined Contribution plan. Fourth, both spouses must be at least 21 years of age.[9] These restrictions leave us with a study population that contains approximately one-third of the entire population of married U.S. tax filers. This study population differs systematically from the broader U.S. population. Panel A of Table 1 shows, for 2015, mean and median income, mean age and mean duration (up to that year) of the marriage. It shows median income for our study population is approximately $101,000. This is close to twice the median household income of the population that year. The population that we study is on average, therefore, substantially better off than couples in the U.S. population.

Our test of efficiency requires both employee data on savings and employer data on plan details. To maximize the size of our analysis sample, as discussed in the previous subsection, we chose to code the retirement plan data of the largest private and non-profit sector plans. We also code the plan of the federal government, though we exclude all federal employees whose W-2 comes from military payers (as uniformed members of the military were not eligible for the match in 2015). Our sample is therefore large, representing retirement plans approximately 40 million individual employees, a substantial proportion of the U.S. workforce, and an even more substantial share of

[9] While firms are disallowed by ERISA law in restricting eligibility to certain classes of employees, they are allowed to exclude those aged under 21 from participating.

those in the U.S. workforce who have access to an employer-sponsored Defined Contribution plan. Our matched sample is comprised of couples where both spouses i) are in the population we defined above, ii) are in our individual merged data set and iii) are members of plans where there is a single match schedule (ensuring we know the match schedule that pertains to them). This yields a sample of approximately 505,000 couples. Panel B of Table 1 shows summary statistics for the couples in this matched sample. Differences between our merged sample and the underlying population we have set out to study are modest.

To form a sample of couples on whom our test can be carried out, we make two further restrictions. We require that a least one spouse makes a DC contribution and at least one spouse works for an employer that offers a match. Applying these restrictions to our matched sample gives our full 'analysis' sample, summary statistics for which are given in the first row of Panel C. Row 2 further restricts the sample to a 'baseline' sample – which makes three further restrictions designed to eliminate couples where our empirical test is somewhat less clean. First we remove couples where either spouse has insufficient tenure to be fully vested in their employer's contributions; this ensures that foregone match is not driven by differing probabilities of forfeiture between spouses. We can do this as we have codified the vesting rules in our plan data, and can measure tenure in the administrative data. Second, we remove spouses where, due to an age difference, there is an difference across spouses in how liquid a plan is. In general, withdrawals before the age of 59.5 are subject to a 10% tax penalty. For this reason, we drop couples where one spouse is older than 59.5 and the other is younger; we also drop couples where both spouses are younger than 59.5 but one spouse is considerably older than the other.[10] Such a difference in age could make contributing to an account with a lower marginal match rate favorable if it is likely to become liquid much sooner. Third, we drop couples where either member had tenure of one year or less. We do this as firms are allowed to exclude employees who have less than one year's tenure from their retirement plan.[11]

Unless otherwise stated, our analysis uses a single cross-section (2015). In some analyses, we exploit the panel dimension of our data, and we give details of this where relevant below.

[10]Specifically, we drop couples where (59.5 minus the age of the younger spouse) is more than twice (59.5 minus the age of the older spouse).

[11]While these three restrictions are *a priori* important they actually make little difference to our headline result.

Table 1: Summary Statistics on Employee Data

	Income (000s)		Age	Marriage	Pop.	Share federal	
	Mean	Median		length	size	(either)	(both)
Panel A: Population	$136.1	$101.3	43.8	11.1	27,381,700	0.046	0.005
Panel B: Matched Sample	$147.3	$119.3	44.4	10.9	504,831	0.435	0.284
Panel C: Analysis sample							
1. Analysis sample: full	$159.2	$129.8	45.1	11.4	424,278	0.487	0.323
2. Analysis sample: baseline	$175.7	$143.6	45.1	11.9	184,619	0.578	0.448

Notes: Panel A shows summary statistics on the couples in the population we define for 2015, who are those couples in the U.S. who satisfy four requirements: i) They file a tax return, ii) Both spouses are employed, iii) Both spouses have access to a DC plan, (iv) Both spouses are at least 21 years of age. Panel B gives summary statistics on our sample in 2015. These are those couples where both spouses are members of retirement plans in our plan dataset. Panel C shows summary statistics for our analysis sample. Row 1 gives our 'full' analysis sample which restricts to couples where at least one spouse makes a DC contribution and at least one spouse works for an employer that offers a match – as these are the couples for which our empirical test can be performed. Row 2 restricts to our 'baseline' sample in 2015, which is a subset of our full sample where both members of the couple are vested in their retirement plan, have at least two years of tenure, and do not have a substantial age gap (details of this final restriction are discussed in the text). The rationale for these restrictions is discussed in Section 4. "Income" is adjusted gross income. Marriage length is censored above at 19 due to data limitations. To protect taxpayer privacy, medians are calculated as pseudomedians, equal to the mean of the 20 observations nearest the true median.

4 Results

In this section, we present the results from our test of efficiency. We show that roughly one in five couples allocate their savings inefficiently and that such non-coordination is persistent over time, costly, and pervasive across sub-samples defined by the characteristics of the plan and of couples. In order to interpret the magnitude of these results, in sub-section 4.3 we form benchmarks for how much inefficiency we should expect absent any coordination between spouses. A comparison of our results with these benchmarks suggests non-coordination of financial decision-making is widespread.

4.1 Non-coordination is common, persistent, and costly

4.1.1 A motivating example

We start with a particular sub-sample of our data where the efficient strategy is simple: one spouse should do *all* the saving. We impose two conditions to guarantee that this is the unique match-maximizing strategy. First, each spouse must face a different match rate on their first dollar of saving. We refer to the spouse with lower and higher match rates as, respectively, L and H. The second condition is that the couple's total saving must be weakly less than the first match cap of

Figure 1: Patterns in simplest case

		Match Rate of High Match Rate Spouse				
		0	(0,50)	50	(50-100)	[100, Max]
	0	?	100%	100%	100%	100%
Match Rate	(0,50)		?	100%	100%	100%
of Low Match	50			?	100%	100%
Rate Spouse	(50-100)				?	100%
	[100, Max]					?

(a) Prediction: savings share of high match spouse

		Match Rate of High Match Rate Spouse				
		0	(0,50)	50	(50-100)	[100, Max]
	0	-	86%	81%	84%	76%
Match Rate	(0,50)		-	71%	77%	67%
of Low Match	50			-	70%	55%
Rate Spouse	(50-100)				-	63%
	[100, Max]					-

(b) Observed: savings share of high match spouse

		Match Rate of High Match Rate Spouse				
		0	(0,50)	50	(50-100)	[100, Max]
	0	?	0%	0%	0%	0%
Match Rate	(0,50)		?	0%	0%	0%
of Low Match	50			?	0%	0%
Rate Spouse	(50-100)				?	0%
	[100, Max]					?

(c) Prediction: share of couples with foregone match

		Match Rate of High Match Rate Spouse				
		0	(0,50)	50	(50-100)	[100, Max]
	0	-	24%	30%	31%	31%
Match Rate	(0,50)		-	47%	48%	44%
of Low Match	50			-	59%	57%
Rate Spouse	(50-100)				-	49%
	[100, Max]					-

(d) Observed: share of couples with foregone match

		0	(0,50)	50	(50-100)	[100, Max]
	0	-	197	532	807	1014
Match Rate	(0,50)		-	268	814	716
of Low Match	50			-	298	570
Rate Spouse	(50-100)				-	435
	[100, Max]					-

(e) Average Foregone Match

Notes: This figure restricts to the subset of couples where it the only efficient allocation is to save entirely in one spouse's account; this spouse is denoted as spouse H and the other spouse L. This restriction leaves only a small share of our sample: 2,800 observations. Panels (a) and (b) compare the theoretical predictions and empirical findings for the share of saving allocated to spouse H's account. Panels (c) and (d) compare the theoretical predictions and empirical findings for the presence of foregone match. Panel (e) reports the average foregone match (conditional on positive foregone match). Foregone match is defined as the 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.

spouse H (that is, the point at which the match rate falls, either to zero or to a lower match rate).[12]

Figure 1 summarizes the extent of inefficiency for the sample who satisfy these two conditions. The figure uses two metrics: the share of saving done by the spouse with the higher match rate and the proportion of couples with some foregone match.[13] Efficiency would imply that these would be 100% and 0% respectively, illustrated in Figures 1(a) and (c). In all exhibits, the match rate of spouse L is given in the rows, the match rate of spouse H is given in the columns. We group match rates into 5 groups: the first group has no match, subsequent groups have: a match rate of greater than 0 but less than 50%; a 50% match; a match greater than 50% but less than 100%; and a match greater than or equal to 100%. Figure 1(b) gives the proportions saved by spouse H in reality. The proportions range from 55% to 86% – meaningfully different from the theoretical benchmark of 100%. Figure 1(d) shows that, in reality, the proportion of couples with a foregone match ranges from 24% to 59%, far from the efficient outcome of 0%.

[12]Out of 184,600 couples in our baseline sample, these restrictions exclude all but 2,800. Beginning in section 4.1.2, we expand our analysis to the full set of couples.

[13]In defining positive foregone match, we apply a *de minimis* threshold of $10 per year in all our analyses. See Appendix A.3 for additional details on how we calculate the foregone match.

4.1.2 Non-coordination is common

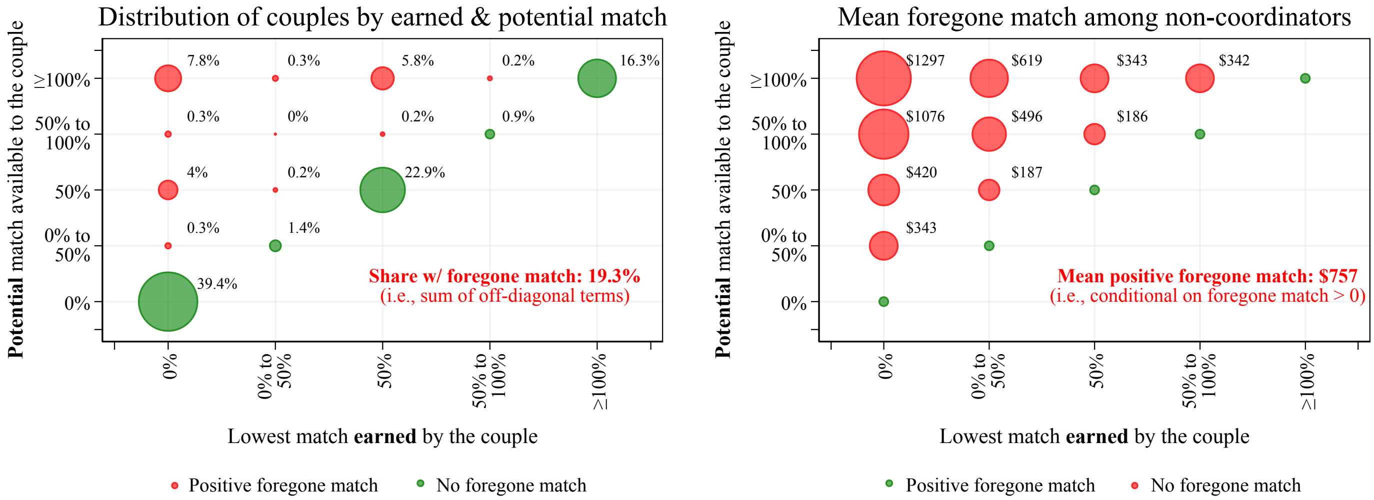
The analysis in the motivating example above uses a small and select group of couples. We excluded couples with the same match schedule; such couples can behave efficiently, or can have foregone match if one spouse saves beyond their match cap while the other does not fully exploit their match. We also only included spouses with saving low enough for the efficient strategy to be for one spouse to do all the saving. Moving to our baseline analysis sample, however, we still find substantial foregone match: **19.3%** (with a standard error of 0.09%) of couples have some foregone match and are, therefore, not coordinating their retirement savings decisions. These couples could receive, on average, an additional \$757 a year in employer matching contributions by moving some of their existing savings from the account of the spouse with a lower marginal match rate to the account of the spouse with the higher marginal match rate.

Figure 2 illustrates the combination of saving decisions that lead to this outcome. The left panel plots the distribution of couples by the combination of the i) smallest match rate that either spouse earned on their last dollar (their *earned match*) and ii) the highest potential match that either spouse could have have earned on their next dollar (*potential match*). For couples represented by a point on the diagonal, whose *earned match* is equal to the maximum *potential match*, there is no reallocation that would increase their collective match.[14] The most common scenario for efficient allocations, accounting for 39% of our couples, occurs when both the last earned match and the potential match is zero. Most such cases are where both spouses are saving more than their match cap. In this situation, the highest potential marginal match available is 0% for both spouses, and the allocation is efficient. The most frequent case of non-coordinated allocations, representing 7.8% of all couples in our sample, arises when one spouse contributes above their match cap (resulting in a marginal match rate of 0%) while the other spouse has access to a marginal match equal to or greater than 100%. The proportion of couples with foregone match (19.3%) is certainly a lower bound on the proportion of couples not coordinating their financial decisions. Many couples will have no foregone match even if they are not coordinating: as one example of this, both spouses could be independently fully exploiting their employer match. We return to this in Section 4.3.

In cases where a couple earns less than the maximum match available, they miss out on some potential employer-matching contributions. The right panel of Figure 2 shows the average foregone

[14] In computing the earned match and potential match, we take into account corners: individuals cannot reduce their contributions if they are contributing zero, and they cannot increase their contributions if they are contributing the maximum annual limit. If both spouses are contributing at their annual limit, then they have no foregone match; they are assigned to the diagonal associated with the maximum match rate of the couple on their last dollar.

Figure 2: The distribution of couples by earned and potential match



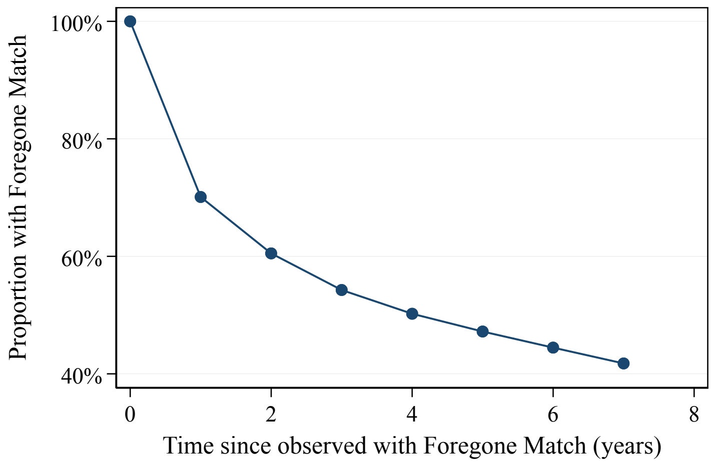
Notes: We define the "lowest match earned" by the couple as the lowest match rate earned by either spouse on their last dollar of contribution. We define the "potential match available to the couple" as the largest match rate that could be earned by either spouse on their next dollar of contribution. We assign couples that have no potential match because they are both constrained by annual contribution limits to the diagonal corresponding to the highest match earned. We similarly assign couples whose foregone match is below the $10 de minimis threshold of $10 to the diagonal corresponding to their highest match earned. 0.2% of those couples on the diagonal nevertheless have foregone match (beyond the de minimis threshold) due to the binning of the x and y axes in the figure. Foregone match is defined as the 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.

match for couples at each point in the graph. The most frequent case of non-coordinated allocations, when one spouse contributes above their match cap (resulting in a marginal match rate of 0%) while the other spouse has access to a marginal match equal to or greater than 100% is also the most costly form of non-coordination, resulting in an average foregone match of $1,297 a year. The second most frequent form of inefficient allocations, accounting for 5.8% of couples, arises when one spouse contributes and earns a marginal match rate of 50%, when additional contributions to their spouse's account could have earned a match equal to or greater than 100%. Because this form of inefficiency induces a smaller wedge between earned and potential matches, it is, on average, less costly and results in a foregone match of $343 a year.

4.1.3 Non-coordination is persistent

We exploit the panel nature of our data to analyze the persistence in having foregone match. We take all observations with positive foregone match from 2003 through 2011, and restrict attention to those that we observe continuously for the subsequent seven years at the same employer. Figure 3 shows that, conditional on having some foregone match in a given year, 70% have some foregone

Figure 3: Persistence of having a foregone match



Notes: The sample for this figure is the set of all observations in our panel (subject to our baseline vesting, tenure, and age restrictions) from 2003 to 2011, where (1) there is positive foregone match and (2) we observe seven consecutive subsequent years of data. We plot the share of observations that experience positive foregone match in years zero through seven relative to the initial observation. Foregone match is defined as the 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.

match one year later, 61% still have some two years later, while 42% have some foregone match seven years later.

There are several ways that one can move from having foregone match to not having it. It could be that the couple re-allocate their contributions holding saving constant (perhaps as a deliberate step in response to the fact that they had an unexploited arbitrage condition). Only 11% of transitions out of foregone match are of this type. 25% of transitions are associated with a decrease in the average saving rate, while most (64%) occur with an increase in the average saving rate.[15] We take from this that the pattern by which couples move out of having forgone match is predominantly not the easiest possible reallocation that they could do to 'fix' the foregone match that we document.

4.1.4 Non-coordination is costly

Panel A of Table 2 summarizes the distribution of the annual cost of non-coordination for those couples who exhibit it. While the average foregone match is $757 annually, the underlying distribu-

[15]These statistics discard the 1% of such couples where at least one spouse's firm changed their match schedule. We define the average saving rate of the couple as the simple average of the saving rate of the two spouses. We consider the average saving rate to be held fixed when it does not change by more than 0.25 percentage points. In our panel (i.e., without restricting to those escaping non-coordination), 40 percent of couples with positive saving at time $t-1$ hold their savings rate fixed between $t-1$ and t using this 0.25 percentage point threshold.

tion is skewed: at the 90th percentile, couples fail to capture $1,940 in potential match, whereas the median is $383 annually. These forgone employer matches represent a sizeable portion of potential savings for couples, on average equivalent to 13% of their household-level employee contributions to retirement accounts, and 8% at the median.

To quantify the *lifetime* cost of non-coordination, we build a simple simulation model based on the empirical evidence in our panel sample. We simulate the evolution of foregone match for 100,000 couples between ages 25 and 59 and calibrate the model to match the incidence and the amount of foregone match by age and lagged foregone match status in the preceding three years. We assume that the foregone savings would have been invested in a Target Date Fund, the typical investment in employer-sponsored retirement accounts, until age 65.[16] In our simulation, relative to a counterfactual in which all couples saved the same amount but allocated their contributions efficiently, non-coordination results in an average of $13,807 less wealth when retiring at age 65. At the 90th percentile, our simulation suggests that non-coordination over an assumed marriage length of 35 years results in $41,495 less wealth when retiring at age 65. These simulation results provide an order of magnitude for the potentially large lifetime negative impact of not coordinating retirement contributions on retirement wealth accumulation.

4.2 Non-coordination occurs across demographic groups and plan types

Figure 4 shows how the incidence of foregone match varies along six different characteristics. These are total wages (in quartiles), age (mean couple of age divided into < 35, $35 - 49$, 50+) education (0, 1-4, and 5 or more calendar years of post-secondary education), parental income (in quartiles), the number of federal employees, and the divorce law regime in the couple's state (Community Property or Equitable Division).[17] The incidence of foregone match falls modestly with own wages, parental income and age. Those with the highest levels of education are less likely than those with less to have foregone match, and couples with one federal employee have higher foregone match than those with none or one. Yet, foregone match is substantial within all of these cells.

The preceding analysis divides the sample by the characteristics of couple, we also show results in the appendix that foregone match is observed across characteristics of the plans. Appendix Table D3 shows that foregone match varies only modestly across different types of retirement plans facing

[16]We assume that savings are invested in and earn the average returns of three asset classes: equities, bonds, and treasury bills. We calibrate the asset allocation by age to match that of Fidelity Target Date Funds. See Appendix C for additional details.

[17]See Appendix A.4 for details on how we measure education and parental income.

Table 2: Distribution of FM (foregone match) (per year, for those not coordinating)

Panel A - Annual cost for couples with positive foregone match

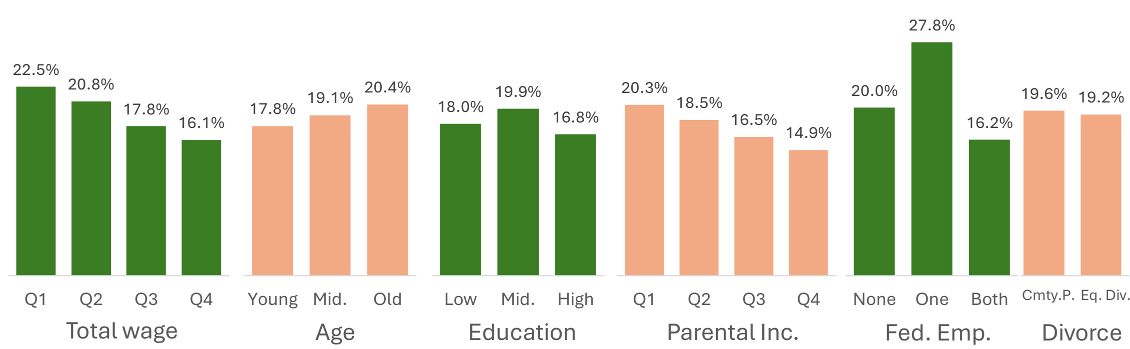
	Mean	p10	p25	p50	p75	p90
Dollars per year	$ 757.2 ($5.7)	$ 58.8 ($0.9)	$ 155.4 ($1.6)	$ 382.6 ($3.2)	$ 886.4 ($8.0)	$ 1,939.5 ($19.8)
As a share of couples' employee contributions	12.6% (0.08%)	1.0% (0.02%)	2.9% (0.03%)	7.8% (0.07%)	17.2% (0.09%)	30.5% (0.24%)

Panel B - Simulated lifetime cost of non-coordination

	Mean	p10	p25	p50	p75	p90
Sum of foregone dollars	$ 3,778	$ 0	$ 0	$ 1,485	$ 5,481	$ 11,243
Foregone wealth at age 65	$ 13,807	$ 0	$ 0	$ 4,805	$ 18,917	$ 41,495

Notes: In panel A, the first row summarizes the distribution of annual foregone match, conditional on foregone match being positive (and subject to a *de minimis* threshold of $10) in our baseline sample. The second row summarizes the distribution expressed as a proportion of the total employee contributions made by both spouses. For disclosure protection, all percentiles in Panel A are "pseudopercentiles", equal to the mean of the 20 observations nearest the true percentile. In panel B, the first row summarizes the distribution of simulated cumulative foregone dollars over an assumed marriage length of 35 years. Row 2 summarizes the distribution of foregone wealth at age 65, under the assumption that foregone dollars would have been invested in a typical retirement investment fund (see Appendix C for details). Foregone match is defined as the 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 4: Foregone Match and Demographic Characteristics



Notes: Total wages are the total W-2 wages received by the couple (gross of pre-tax retirement contributions) at all jobs. We form quartiles. For age, couples are divided into three groups based on their mean age: Young (< 35), Middle ($35 \leq age < 50$), Older (≥ 50). Education groups are formed using calendar years in higher education (measured as receipt of Form 1098-T); low/medium/high corresponds to zero calendar years, 1-4 calendar years, and 5 or more calendar years respectively. Parental income is divided in quartiles. See Appendix A.4 for further details on the construction of parental income and education variables. We divide our sample by whether neither, one, or both spouses are federal employees. Finally we divide the sample by whether the couple lives in a state governed by equitable division or community property divorce law. Education and parental income is only observed for a part of our sample. For the education analysis, we restrict to couples where both spouses attained age 18 in 1999 or later. For the parental income analysis, we restrict to couples where we are able to match each spouse to a parent. Foregone match is defined as the 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.

couples. Match schedules are piece-wise linear functions, with most plans either having one or two tiers. We divide our sample of plans by whether there is no match, whether the match has one tier, or whether it has two (or more) tiers. The table reports results by the combination of plan types. The lowest incidence of foregone match (16.2%) is found among those where both spouses have single tier plans, while the highest incidence (29.5%) is found among those where one spouse has a single-tier plan and the other has a two-or-more-tier plan. Average foregone match among those not coordinating is very similar across plans.

4.3 Using non-coordinating benchmarks to estimate the share of couples that are not coordinating

If a couple has positive foregone match ($FM > 0$), we can infer that they are not actively coordinating. However, the inverse is not true: no foregone match ($FM = 0$) does not imply that a couple is actively coordinating. Since the set of efficient allocations can be large, spouses could be making unilateral contribution decisions which just happen to be consistent with household efficiency. For example, both members of the couple could be fully exploiting their respective matches (in which case the marginal match rate would be zero for both spouses), or they could both face the same single-tier match schedule with neither saving beyond their match cap.

In this section, we estimate the share of couples not actively coordinating. Since actively coordinating couples have $FM = 0$, we can express the probability of positive foregone match as the product of (a) the probability of not coordinating and (b) the probability of positive foregone match conditional on not coordinating. We have directly observed that the probability of foregone match is 19.3%. Thus, given an estimate of the probability of positive foregone match conditional on not coordinating, we can estimate the share of couples not actively coordinating.

We implement two approaches to evaluate the probability of foregone match we would expect to see absent any coordination between spouses. First, we create placebo samples of synthetic couples with similar characteristics to real couples. Second, we observe couples in the years prior to marriage and the years following a divorce. Both approaches yield quantitatively similar results: in the absence of any coordination we would expect to see in the region of 33% of couples allocate retirement contributions inefficiently. A comparison of the incidence of foregone match in the data with that in these no-coordination benchmarks, suggests that about two thirds of couples do not actively coordinate – i.e., inefficiency is widespread.

Synthetic couples benchmarks. For our first approach, we generate two placebo samples of synthetic couples for whom we should expect no coordination of retirement contributions. Formally, we denote our sample of couples as C, made up of couples $c_1, c_2, \ldots, c_N$. Each couple c_i is comprised of spouses a_i and b_i.[18] We generate two samples of synthetic couples. In the first one, we take the sample of married people in our data and re-arrange them; we refer to this sample as the "Reshuffled Couples" sample. Specifically, for each a_i, we find a spouse b'_i from the set $B_i^{donor} = (b_1, b_2, \ldots, b_{i-1}, b_{i+1}, \ldots, b_N)$ such that b'_i matches the age, gender, and earnings of the true spouse b_i.[19] The couple $\hat{c}_i^M$ is then comprised of a_i and b'_i. In the second synthetic sample, we find a set of single individuals that match each individual (a_i and b_i) in our real sample; we refer to this sample as the "Pairs of Singles" sample. Let $\hat{A}_i$ denote the set of matches for a_i, which is comprised of single individuals $\hat{a}_1, \hat{a}_2, \ldots, \hat{a}_{N_i}$; $\hat{B}_i$ is defined similarly.[20] We then form all possible synthetic couples for couple i by taking the Cartesian product of $\hat{A}_i$ and $\hat{B}_i$. Within this set of potential synthetic couples, we choose the couple $\hat{c}_i^S = (a''_i, b''_i)$ whose total DC contributions match the total DC contributions of the true couple most closely.

As in our actual sample, we observe the incentives and contributions of each synthetic spouse in our two placebo samples. We can therefore calculate foregone match for each couple in the synthetic samples. We use the incidence of foregone match in the synthetic samples as estimates of the (counterfactual) incidence of FM that would be observed in the absence of any coordination.

Columns (1) and (2) of Table 3 compare our observed foregone match to the values in the two non-coordination benchmarks. Panel A reports the observed share with foregone match in the baseline sample of true couples. Panel B gives the proportion with foregone match in each of the two synthetic samples. These estimates imply that, without any coordination within the household, 33% to 34% of placebo couples end up with some foregone match. The final row (Panel C) uses these two figures to estimate that share of couples not coordinating is 57% to 58%.[21]

Evolution around marriage and divorce. Our second non-coordination benchmark leverages the fact that we have longitudinal data. We calculate the share of couples who have foregone

[18] Our sample includes both different-gender and same-gender couples (as well as a small number of couples where at least one member has unknown gender). We randomly assign one member of the couple to be a and the other b.

[19] We enforce an exact match on gender and year of birth. Within the set of possible matches, we choose the b'_i that matches the closest on earnings.

[20] We require an exact match on firm and gender, a match in age within 10 years, and a match in earnings within 20%. For computational tractability, we require that $\hat{A}_i$ and $\hat{B}_i$ contain no more than 10 individuals; if there are more than 10 satisfying the match conditions, we choose the 10 with the closest match on earnings.

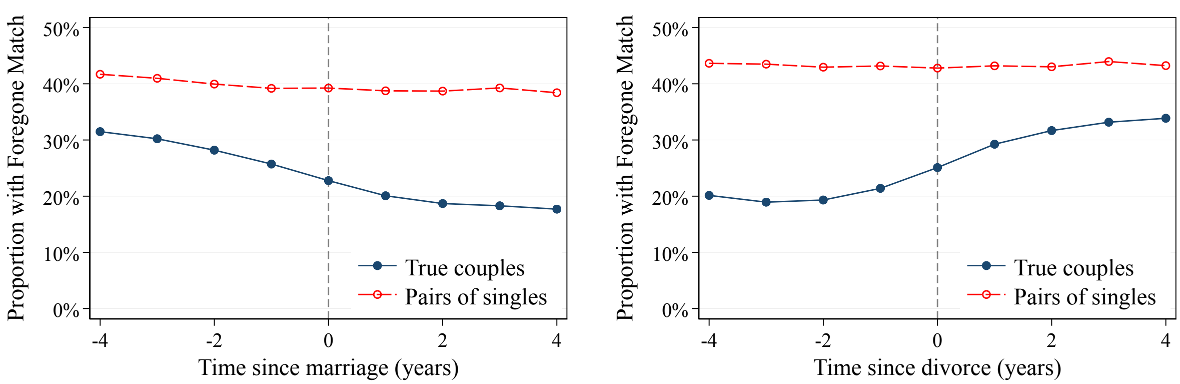
[21] Formally, some couples are coordinating ($Coord = 1$) and some are not ($Coord = 0$). Those who are coordinating have $FM = 0$; those who are not have $FM > 0$ with some probability $P(FM > 0|Coord = 0)$. Thus, we can write $P(FM > 0) = P(FM > 0|Coord = 0)P(Cord = 0)$. Since we observe $P(FM > 0)$, the estimates of $P(FM > 0|Coord = 0)$ (from our synthetic samples) allows us to recover $P(Coord = 0)$, equal to $\frac{P(FM>0)}{P(FM>0|Coord=0)}$.

Table 3: Estimated proportion failing to actively coordinate

	(1)	(2)	(3)	(4)
Panel A:	Baseline sample		Four years	Four years
Sample of true couples:			after marriage	before divorce
Proportion with foregone match	0.193		0.177	0.201
Panel B:	Reshuffled	Pairs	Four years	Four years
Non-Coordination Benchmark:	Couples	of Singles	before marriage	after divorce
Proportion with foregone match	0.331	0.336	0.315	0.339
Panel C:				
Implied share non-coord.	0.582	0.574	0.562	0.595

Notes: Panel A reports the share of couples that have foregone match in each sample. In columns (1) and (2), we use the baseline sample. In column (3), we use our panel observations four years after their marriage in the sample from Figure 5, panel (a). In column (4), we use our panel observations four years prior to their observed divorce in the sample from Figure 5, panel (b). Panel B reports the benchmark estimate – that is, the estimate for the probability of observing foregone match when not coordinating. In columns (1) and (2), the benchmarks are the Reshuffled Couples and Pairs of Singles samples, respectively. The former is constructed by taking the married couples in our sample and re-arranging them. A (random) reference spouse in each couple is matched with someone with the same age and gender, and similar earnings as their actual spouse. The latter sample is formed by arranged singles in synthetic couples such that the combination of ages, gender, earnings, and the total contributions match the real couple. In column (3), the benchmark is the same set of couples as in column(3) of Panel A, but four years prior to their marriage. In column (4), the benchmark is the same set of couples as in column (4) of Panel A, but four years after their divorce. In Panel C, we report the implied share of true couples that are not coordinating in each sample, based the benchmark in Panel B, equal to the proportion with $FM > 0$ in the true sample (i.e., Panel A) divided by the proportion with $FM > 0$ in the benchmark in question (i.e., Panel B). Foregone match is defined as the 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 5: Probability of non-coordination around marriage and divorce



(a) Marriage

(b) Divorce

Notes: These graphs (solid series) show the probability of non-coordination around marriage (panel (a)) and divorce (panel (b)) where the dependent variable is having some foregone match (over a *de minimis* threshold of \$10) using our panel data. The sample is comprised of all individuals in our panel restricted to those for whom we have at least 4 years of consecutive data on each side of the event in question. We require that all couple-year observations satisfy our baseline vesting, tenure, and age restrictions. The dashed series show the probability of non-coordination for a sample of synthetic couples, comprised of single individuals, constructed in a manner analogous to the $\hat{C}_S$ sample in the cross-section. Foregone match is defined as the 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.

match at their chosen saving choices prior to their marriage and/or after their divorce. Figure 5 shows the probability of having some foregone match in a balanced panel four years prior and after marriage and divorce. Panel (a) shows that in the 9 year period centered around the time of marriage, the incidence of non-coordination falls by 14 percentage points. Panel (b) shows that in the 9 year period centered around divorce, the incidence of non-coordination increases by 14 percentage points. In the case of marriage, the trend starts several years before marriage, indicating that the tendency towards coordination (for those who coordinate) is a gradual process. In the case of divorce, the unwinding of cooperation starts just before the year of divorce.

In both panels, we include a panel of synthetic couples as well. These samples are analogous to the $\hat{C}_S$ sample in the cross-section: we find each spouse an unmarried individual with similar age, similar earnings, same gender, and who works at the same firm. We form all such possible couples, choosing the one with total contributions that match the true couple most closely.[22] These synthetic couples do not experience any substantial change in their probability of foregoing a match, suggesting that the patterns in the true couples around marriage and divorce are not spuriously driven by aging or any other factors.[23]

In both cases, the non-cooperative benchmark (four years pre-marriage and four years post-divorce for true couples) is that approximately 32% to 34% of couples would have some foregone match. Columns (3)-(4) of Table 3 use these benchmarks to infer the share of true couples not actively coordinating. Panel A contains the observed share foregone match for true couples in each sample.[24] For those we observe marrying (divorcing), we measure foregone match four years after (before) marriage (divorce). Panel B of Table 3 reports the share of foregone match in our non-coordination benchmarks (i.e., four years before marriage or four years after divorce). Finally, Panel C compares true couples to these non-coordination benchmarks. The implied share of couples not coordinating when comparing the same couples before/after divorce and marriage is close to that we estimate when comparing our baseline sample to our synthetic benchmarks of random pairs with similar characteristics to real couples.

[22]The procedure is nearly identical to the $\hat{C}_S$ sample in the cross-section. The only exception is that earnings and contributions are computed as the average of the nine-year period surrounding the divorce or marriage event.

[23]In Appendix D.5, we study marriage and divorce in an event study framework, leveraging the synthetic couples as never-treated units. These results are qualitatively similar (with slight attenuation) when controlling for total contributions and earnings.

[24]The samples here are different from our baseline and are comprised only of couples where the event (marriage or divorce) is observed and for whom we have 4 years of consecutive data on each side of the event in question.

5 What explains lack of coordination?

5.1 Supplementing the administrative data with survey evidence

In this section, we examine the mechanisms behind the lack of coordination that we have documented. We use both our administrative data and a tailored survey which we ran through the survey platform Prolific. We surveyed 1,000 individuals screened to be living in the U.S., working, married, aged between 18 and 59, and who had a retirement plan. Appendix E describes the survey design and a provides standalone discussion of the results.

Central to the the survey was a hypothetical choice problem in which respondents were asked to divide a given level of savings between their account and their spouse's account, mimicking the maximization problem faced by couples in reality. The two accounts had different match schedules. We varied the combinations of match schedules such that there were three versions which differed in the match-maximizing strategy; for one-third respondents the match-maximizing strategy was to put all the funds in their own account, for one-third it was to put all the funds in their spouse's account, and in the final third it was to split it between accounts. We also measure respondents' financial literacy, their understanding of the rules around asset division in divorce, and details about their retirement plans and those of their spouse.

We find that 40% of respondents have foregone match – that is, they allocate their savings in a way that does not maximize the amount of employer match.[25] Subsequent questions allow us to distinguish between whether the foregone match was deliberate or not. We inform respondents when their allocation did not maximize the match, ask them whether this was intentional, and if it was not, we give them an opportunity to make a new choice. We classify those who did so intentionally, as well as those who didn't realize but declined to re-optimize as having 'deliberate' foregone match. Those who chose to reallocate and whose subsequent choice has no foregone match, are classified as having *accidental* foregone match.[26] Of the 40% of survey respondents that have some foregone match, approximately 45% have accidental foregone match and 48% have deliberate

[25] For several reasons, we expect the incidence of foregone match to differ between the the survey and the administrative data. In the survey, only one allocation achieves efficiency by construction, while in the administrative data, there is often a range of allocations consistent with efficiency. Second, the intra-household dimension of the problem is more salient in the hypothetical survey experiment. Third, unlike in the administrative data, survey respondents effectively act as dictators, choosing the contribution to both accounts. Nevertheless, we expect that the correlates of foregone match in the survey data can be informative about the drivers of foregone match for real couples.

[26] A third possibility is that the respondent makes a new choice, but that new choice also has foregone match. In this small number of cases (39), we subjectively classify the respondent into "accidental", "deliberate", or "other" (i.e., unknown) foregone match based on narrative explanations. Appendix E.4.2 gives further details.

foregone match, with the remaining 7% having 'other' or unknown foregone match.[27]

In the next two sections, we further investigate drivers of non-coordination, taking insights both from the administrative data and the survey evidence. We segment our investigation into non-coordination as a mistake (in Section 5.2) or as a deliberate decision (in Section 5.3).

Table 4: Foregone match, and its types in the survey data

	(1) N	(2) Prop. of total	(3) Prop. of those w/ foregone match
No foregone match	594	60.2	
Has foregone match	393	39.8	
of which			
...Accidental foregone match	177	17.9%	45.0%
...Deliberate foregone match	187	18.9%	47.6%
...Other	29	2.9%	7.4%

Notes: The sample for this table is the full survey sample. The first two rows give number and proportion of observations by whether they have foregone match or not. The last three rows give number and proportion of observations by the type of foregone match. The Proportions are given out of the total sample (column (2)), and out of just those with foregone match (column (3)). A full description of our classification of foregone match into its types is given in Appendix E.4.2.

5.2 Non-coordination as a financial mistake

We start by showing evidence against some factors that might have driven our result: we use our administrative data to show that non-coordination is not substantially driven by inertia or by heuristics which lead spouses to contribute equal amounts. We then show that the incidence of of foregone match does not vary with the financial stakes of the match, suggesting that rational inattention is not driving the results. We then use our survey data to show, though, that inattention (albeit perhaps not of a rational type) is likely relevant: many couples have not even considered that there might be gains to coordination, and when the implications of their decisions are brought to their attention, they change their behavior.

5.2.1 Non-coordination is not driven by inertia or auto-enrollment

A plausible explanation for non-coordination could be inertia: couples may want to maximize their match but fail to do so due to passive behavior or adjustment costs. Indeed, inertia has been shown to be consequential in the retirement saving setting (Madrian and Shea, 2001; Derby et al.,

[27]'Other' foregone match comprises those who (a) did not understand the question about whether they realized that they had not maximized the match and (b) those who opted to reallocate, did not maximize the match, but whose narrative explanation was not sufficient to determine a classification.

2023; Choukhmane, 2023). To investigate whether inertia can account for the observed lack of coordination across spouses, we exploit the longitudinal aspect of our data. To the extent that inertia is driving our results, we would expect that when couples make an *active* change to their contributions, they should eliminate foregone match. We focus on the sub-sample of couples where both spouses made an active change to their retirement plan contributions and we evaluate the *change* in the extent of their foregone match. In defining this sample, we further restrict to those with plans that did not have auto-features, to ensure that the changes in the contributions that we observe arise from active decisions.

Appendix 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 suggests that the non-coordination we document is not driven by spouses being temporarily away from the efficient allocation due to inertia.

A second manifestation of behavior driven by inertia would be if those individuals in plans that offer auto-enrollment stay at the auto-enrollment default. There is, of course, no reason that the default contribution rates should align with the contribution rates that maximize employer matching at the household level. However, we find the incidence of foregone match among couples not subject to auto-enrollment is similar to the share of inefficient allocations in the baseline sample: 18.8% for couples hired under an opt-in regime relative to 19.3% in the overall sample.

We conclude that inertia is not a major driver of the patterns of non-coordination.

5.2.2 Equal-saving heuristics do not explain inefficient allocations

Next, we investigate whether couples allocate contributions to retirement accounts in a manner that is 'equal' but does not consider economic incentives at the household level.[28] Couples may fail to allocate their savings efficiently because they abide by a rule of contributing equal amounts to their respective retirement accounts. We investigate this hypothesis in Figure 6. In Panel (a), we plot the density of couples as a function of the husband's contribution rate relative to the couple's

[28]Gathergood et al. (2019) show that heuristic-type behavior explains how individuals allocate credit card repayments across cards.

average contribution rate, both for the true sample and the two synthetic samples.[29] More precisely, we first calculate the contribution rate of each spouse ($\frac{s^j}{y^j}$) for $j = \{H, W\}$ (husband and wife). We then measure how they relate to each other by calculating a 'husband's relative contribution' as follows: $\frac{\frac{s^H}{y^H}}{0.5\left(\frac{s^H}{y^H}+\frac{s^W}{y^W}\right)}$. This quantity ranges from 0% to 200%, and a relative contribution of 100% indicates that both spouses are contributing the same proportion of their salary. We find that, indeed, there is an excess mass near 100%. This finding, which to our knowledge is novel, is consistent with some couples engaging in an equal-saving heuristic.[30] In Appendix Figure D2 we show the figures are qualitatively similar if the husband's relative contribution is measured in dollars rather than proportions of salary, i.e. $\frac{s_H}{0.5(s_H+s_W)}$.

However, this type of behavior seems to, if anything, attenuate, rather than contribute to, the prevalence of inefficient allocations that we document. Panel (b) plots the share of couples with FM as a function of the husband's contribution share. In both the true and synthetic samples, the proportion with FM is *smaller* when the husband's relative contribution is near 100%. Mechanically, this occurs because such an allocation is less likely to involve one spouse contributing beyond their match cap while the other fails to exploit their match. Thus, if anything, equal saving heuristics may be *reducing*, rather than creating, foregone match.

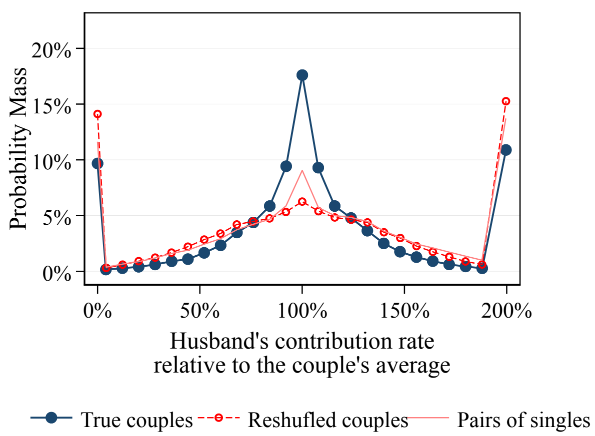
5.2.3 Non-coordination persists when stakes are high

If the cost of failing to coordinate is sufficiently low, then *rational* inattention could explain the non-coordination that we observe. To test this hypothesis, we use both spouses' matching formulas to construct a measure of 'stakes' by asking the following question: given the couples' combined savings and their combination of match schedules, what is the maximum match they could forego from not coordinating saving decisions? For some couples, stakes are lower because the worst-case outcome from not coordinating contributions generates only a small (or zero) amount of foregone match. In other cases, non-coordination could cost several thousand dollars in foregone match. To do this, we first calculate the maximum match and minimum match a couple can obtain at their combined savings. The minimum match might be zero (if one spouse is in an employer that does not offer any match) or positive if both spouses have a match. The difference between

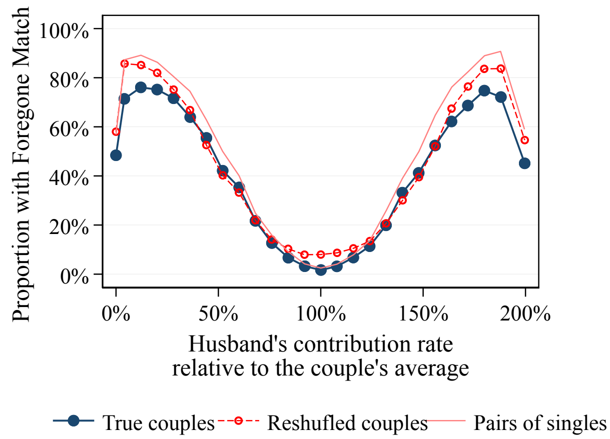
[29] For this exercise, we drop same-gender couples. We also drop couples where at least one spouse is contributing 95 percent or greater of the statutory maximum, which is $18,000 or $24,000 depending on age. This second restriction eliminates couples whose savings are equal to each other because they are both contributing at the statutory maximum.

[30] A contributing factor to this result could be a tendency for spouses to have equal earnings (Bertrand et al. (2015). However, even among spouses whose earnings differ we find evidence of a tendency for spouses to have similar saving rates.

Figure 6: Equal Saving Heuristics: Density, and Probability of foregone match, by Husband's Share of Contribution



(a) Distribution of husband's contribution rate rel to couple's average



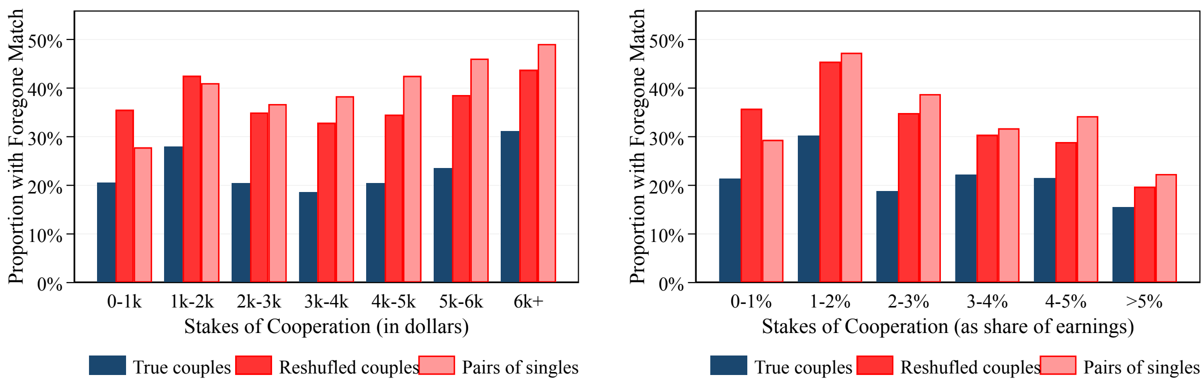
(b) Proportion with foregone match, by husband's contribution rate rel to couple's average

Notes: Panel (a) plots the density of the husband's contribution rate relative to the couple's average contribution rate, i.e. $\frac{\frac{s^H}{y^H}}{0.5\left(\frac{s^H}{y^H}+\frac{s^W}{y^W}\right)}$. Panel (b) plots the probability of foregone match as a function of the husband's contribution rate relative to the couple's average contribution rate, calculated in the same manner. 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). This figure restricts to different-gender couples. Foregone match is defined as the 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.

the match the couples receive in the best case and that which they receive in the worst case is our measure of the 'stakes' of the decision. Figure 7 shows how foregone match varies with this measure of the stakes. In Figure 7(a) we measure the stakes in dollars, while in Figure 7(b) we express the stakes as a proportion of the joint earnings of the couple. In each panel, the solid blue bars represent true couples, while the lighter red and orange bars represent the two samples of synthetic couples. We take two lessons from this graph. First, whether the stakes are low or high, true couples outperform synthetic couples – that is, some coordination occurs. But, second, even when the stakes are extremely high (e.g. when non-coordination could cost the couple more than 5,000 dollars or 5% of their earnings per year), a substantial proportion of couples are allocating their saving inefficiently, and whether we measure stakes in dollars or as a share of earnings, the incidence of coordination is not sensitive to stakes of the decision.

While we take this as evidence that rational inattention is not driving our results, in the next subsection we show that inattention (albeit perhaps not of a rational type) *is* a contributing factor.

Figure 7: Share with foregone match as a Function of the "Stakes" of the Decision



(a) Share w/foregone match, by stakes (dollars) (b) Share w/foregone match, by stakes (earnings pp)

Notes: This figure plots the share of couples with foregone match as a function of the "stakes" of the decision. The "stakes" are defined as the difference between the maximum possible match and the minimum possible match, given the total contributions of the couple and the match schedule faced by each spouse. In panel (a), the stakes are measured in dollars. In panel (b), the stakes are scaled by the total earnings of the couple. This figure uses the baseline sample. Foregone match is defined as the 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.

5.2.4 Many couples have not considered that there might be gains to coordination

We provide two pieces of evidence from our survey data that many couples might not have considered that there can be gains to coordination.

First, for the respondents who a) had foregone match in our hypothetical choice experiment, and b) stated that they had not realized this fact, we asked them whether they would like to change their answers. 90% stated they would like to change their allocation, and of those, 80% reallocated their contributions to remove foregone match (details are given in Appendix Table E3). These respondents had not noticed the intra-household arbitrage opportunity available to them, despite the fact that, in our survey, this fact was surely more salient than it would have been in reality. When prompted to re-optimize and offered the chance to, most took the opportunity.

Second, for the subsample of our survey respondents where both spouses have a DC plan, we explicitly asked respondents whether they had considered that there might be gains from coordination. 36% of this subsample, and 55% of those with accidental foregone match, answered that they had not considered this prospect (Appendix Table E9 shows these results). This stated lack of attention to the potential arbitrage opportunity in their real retirement savings decisions correlates with a demonstrated lack of attention to the hypothetical choice experiment. Those who state that they have considered the gains to coordinating in reality were less than half as likely to have accidental foregone match in the hypothetical survey experiment than those who had not

Table 5: Foregone match for those working at the same and different firms

Circumstance	Proportion with Foregone Match
Different match, different firm	29.2%
Same match, different firm	25.4%
Same match, same firm	15.2%

Notes: "Same match" means that both spouses face a schedule with identical match rates and match caps (expressed as a share of earnings). Foregone match is defined as the 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.

considered it (with, respectively, 13% and 29% of each group having accidental foregone match).

The salience of this arbitrage opportunity at the household level might be limited because retirement plans are individually managed and sponsored by one's employer. The household dimension of the decision is potentially more salient for couples working for the same firm. Consistent with this, Table 5 shows that foregone match is less common for couples who work for the same employer – even compared to those who face identical match schedules but work at different employers.

5.2.5 Non-coordination falls with Financial Literacy

A growing literature emphasizes financial literacy's role in financial decision-making (see Lusardi and Mitchell (2023) for a recent survey). Couples with lower levels of financial literacy might be more prone to financial mistakes and less likely to realize that coordinating retirement contributions can lead to their receiving larger matching contributions. We measure financial literacy based on the responses to five questions. Four questions are taken from the existing literature and capture respondent's understanding of simple financial concepts (we take three questions taken from Lusardi and Mitchell (2011) and one from Lusardi (2008)). The fifth question is new and tailored to our setting: we survey respondents on their knowledge of the tax treatment of traditional 401(k) accounts. The questions are given in Appendix E.2.4. We group respondents by whether they answered two or fewer, three, four, or five questions correctly.[31]

Table 6 summarizes the relationship between Financial Literacy and foregone match. Column (1) gives the proportion of our sample in each of the financial literacy groups. Columns (2) to (5) show incidence of foregone match overall and for each type. Foregone match falls steeply with measured Financial Literacy.[32]

[31] Only approximately 5% of respondents got one or no questions correct, hence our grouping at the bottom.

[32] This gradient might partly reflect differences in survey engagement: respondents who are less engaged in the survey are presumably more likely to get financial literacy questions wrong and to choose a savings allocation that does not maximize the match.

Table 6: 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: This table groups our survey respondents by the number of financial literacy questions they got correct. Appendix E.2.4 gives the questions. Column (1) gives the proportion of the sample getting each number of questions correct. The remaining columns show, by the number of questions correct, the proportion with foregone match. Columns (2) gives proportion with any foregone match; columns (3), (4), and (5) give proportion with deliberate, accidental and other foregone match, respectively.

5.2.6 Taking Stock

We take these results as evidence that a sizeable share of non-coordination is a result of a financial mistakes. Inertia or a heuristic of equal saving do not appear to drive our results; a combination of inattention to the fact that there might be gains from coordination and financial literacy do.

5.3 Non-coordination as a deliberate decision

Table 4 showed that approximately half of those who did not maximize the match in response to our hypothetical choice question stated that they did so deliberately. Section 2.4 discussed reasons why spouses might have (or perceive that they have) incentives to prefer contributions to their own accounts and that such reasons might be correlated with the degree of commitment. In this sub-section we provide evidence that this deliberate decision to forego match is associated with proxies for the strength of marital commitment, is more prevalent for those who believe that they will keep their own accounts on divorce, and that notions of trust in one's spouse and fairness contribute to the decision.

5.3.1 Non-coordination is correlated with proxies for commitment

We propose five measures in our administrative data that are plausibly correlated with the strength of marital commitment. These are the length of the marriage, the presence of children, the couple having a mortgage, a divorce event in the near future[33], and whether the couple had a joint bank account before getting married. For the first four of these we observe the indicator for our entire

[33] The results we show are for a cross-section in 2015, and as 2022 represents the most recent year of tax data, our divorce indicator will only capture divorce realizations in the 7 years subsequent to the year of observation.

sample, for the last–whether they had a joint back account before marriage–we can only construct if the couple was was first observed as married in 2008 or later.[34]

Table 7 shows the relationship between these proxies of marital commitment and the occurrence of foregone match (panel (a)) and the size of the foregone match as a proportion of employee contributions (panel (b)). We can construct our joint bank account indicator for only a subset of our data, and so columns (1) and (3) omit that and use our full sample, while columns (2) and (4) include it and use the sample for which it is observed. We control for household earnings, household contributions and their interaction, and a set of other characteristics, listed in the table notes. Appendix Tables D5 and D6 report the coefficients on these characteristics.

In each case we see that those who are less committed (per our proxies) are more likely to have foregone match. The magnitudes of the association between some of these proxy variables and non-coordination is sizeable, especially given that they are likely to be only noisy measures of the strength of marital commitment. Having a mortgage, for example, is associated with a 2.4 percentage point reduction in non-coordination (relative to an overall mean of 19.3%) in our specification with full controls. The imminence of divorce has an association of a roughly similar magnitude but of the opposite sign. Those with a joint account are 1.5 percentage points less likely to have some foregone match (from a base of 18.3% in this sample).

While these correlations are not conclusive evidence, and these noisy proxies only explain a small share of the total variation in the data, we take them as suggestive that spouses more committed to each other are, all else equal, less likely to forego match. This raises a question. Even in the absence of commitment to the marriage, if divorce is the outside option, spouses should still face strong incentives to coordinate, raising the question as to why there is a link between commitment and cooperation. The next subsection uses our survey to discuss one possible reason for this.

5.3.2 Knowledge of divorce law is imperfect and correlated with coordination

In our survey, we measured perceptions of how retirement wealth and other assets are treated in divorce (Appendix E.4.5 provides details). Wealth in a retirement account is a marital asset and, as such, is divided on divorce independently of who made the contributions.[35] Despite this, as column

[34]We code them as using a joint bank account if each member of the couple used the same bank account to receive a direct deposit of any tax (Form 1040) refund that they are entitled to. For this analysis, we restrict attention to those couples where both members of the couple received a Form 1040 refund in the final year prior to marriage and both elected direct deposit.

[35]States differ between those which are Community Property states, in which assets acquired during marriage tend to be divided equally, and 'Equitable Division' states, in which there is some judicial discretion to the splitting of assets. However, even in equitable division states, the identity of the particular spouse who made the retirement

Table 7: Foregone Match and Commitment

	a) Prop. with foregone match		b) Foregone match as prop. of emp'ee contribution	
	(1)	(2)	(3)	(4)
Length of marriage	-0.0010	-0.0019	-0.0141	-0.0292
	(0.0002)	(0.0006)	(0.0039)	(0.0117)
Kids	-0.0057	-0.0110	-0.1975	-0.1764
	(0.0020)	(0.0039)	(0.0401)	(0.0740)
Future divorce	0.0181	0.0108	0.2416	0.1192
	(0.0031)	(0.0054)	(0.0679)	(0.1058)
Mortgage	-0.0244	-0.0321	-0.3487	-0.4088
	(0.0025)	(0.0055)	(0.0541)	(0.1097)
Joint account prior to marriage		-0.0151		-0.2849
		(0.0053)		(0.0990)
Baseline mean	0.1929	0.1830	2.4302	2.1510
Partial R^2	0.0011	0.0021	0.0007	0.0012
Inc. x Conts. Controls	X	X	X	X
Full Controls	X	X	X	X
Observations	184,570	44,440	184,570	44,440

Notes: The dependent variable in columns (1) and (2) 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 columns (3) and (4) is the foregone match as a proportion of total employee contributions, scaled in percentage points; the coefficients are those from Ordinary Least Squares regressions. All rows control for the interaction of total earnings and total contributions by the couple, where earnings and contributions are measured with respect to employers $j(i_A)$ and $j(i_B)$ only. Our full set of controls additionally includes the mean age of spouses, the age gap between them, the mean tenure of the couple, tenure gap between them, the share of earnings earned by the primary earner, whether one or both members of the couple was hired during an automatic enrollment regime, whether they live in a state where divorce law requires equitable division, the log of adjusted gross income, whether each member the couple faces an identical match formula, and whether both members of the couple work for the same firm. The full set of coefficients for the probability of non-coordination and the extent of non-coordination are reported in Table D5 and D6 in Appendix D respectively. Columns (1) and (2) use our baseline sample. Columns (3) and (4) use the same for whom we can ascertain whether they had a joint bank account before marriage.

(1) of Table 8 shows, over a third of couples believe that they would keep their own retirement wealth, and close to a fifth do not know how assets would be divided. Around half of respondents say that they would split the assets, or that they would be divided in some other manner.

Columns (2), (3), and (4), 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 match respectively. There is a clear pattern. Those who think that accumulated retirement wealth remains one's own property on divorce are are much more likely than those who think such wealth with be split to have foregone match. 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

contributions should not be relevant to the division of retirement assets.

one's own wealth in case of divorce.

Table 8: Knowledge of Divorce Law and Association with Forgone March

	(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: This table groups our survey respondents by the number their knowledge of divorce law. Appendix E.4.5 describes the construction of the groups. Column (1) gives the proportion in each group. The remaining columns summarize, by divorce knowledge, the incidence of foregone match. Column (2) gives proportion with any foregone match; columns (3), (4), and (5) give proportion with deliberate, accidental and other foregone match, respectively.

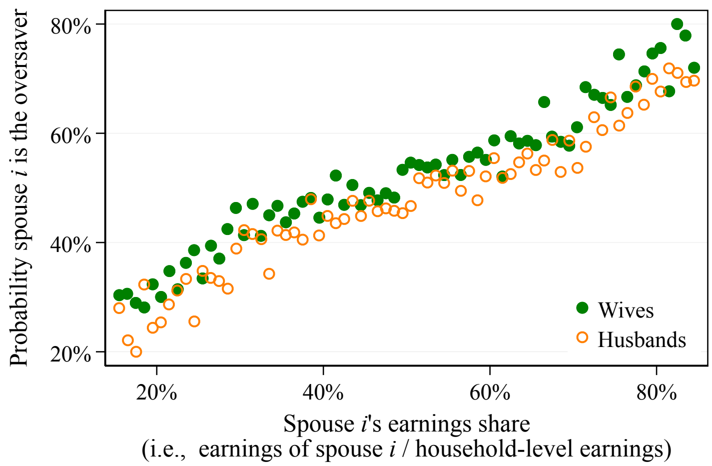
5.3.3 The role of gender

Across the administrative and survey samples, we find evidence consistent with women placing a higher value on maintaining their own retirement savings than men do.

In each couple that is not coordinating, efficiency would involve reallocating contributions from an over-saving spouse to an under-saving spouse. Among different-gender couples with some foregone match in the administrative data, Figure 8 shows how the probability of being the oversaver varies with their household share of earnings and their gender. The probability of being the oversaver increases with share of earnings. More strikingly, throughout the relative earnings distribution, wives are more likely to be the over-saving spouse relative to husbands. This finding suggests that women put a higher value on maintaining their own retirement savings.

This hypothesis is also consistent with the survey's results. As shown in Appendix Table E6, 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 (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 chose deliberate foregone match when the efficient allocation requires putting all the savings in their own account). We find similar results for different-gender couples in the administrative data. When it is optimal to save entirely in the wife's account, 38.7% of husbands nevertheless make positive contributions; when it is optimal to save entirely in the husband's account, 47.1% of wives nevertheless make positive contributions.

Figure 8: Differences by Relative Earnings and Gender



Notes: For each couple with foregone match, efficiency can be attained if savings are reallocated from one spouse (the "oversaver") to the other (the "undersaver"). This figure restricts to different-gender couples with foregone match. It plots the share of individuals that are oversavers as a function of the share of the couple's earnings that that individual represents, separately for wives (solid circles) and husbands (hollow circles). For legibility, we restrict to cells where the earnings share ranges from 15% to 85%. Foregone match is defined as the 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.

These differences across genders could reflect differences in beliefs about divorce outcomes. In the survey, women were more likely to believe retirement assets would be kept separate in case of a divorce: 39.6% of women and 28.9% of men expressed this incorrect understanding of divorce law.

5.3.4 The role of ownership of accounts, fairness, and risk

We conclude with a brief discussion of narrative explanations given in our survey for deliberate non-coordination. We asked all survey respondents who had foregone match (and did not correct their allocation later on) for a narrative explanation of their choice. From these narratives, we identified three main themes present in close to two-thirds of the responses: (i) the desire for financial independence, (ii) considerations of fairness, and (iii) concerns about risk. First, many respondents indicated a sense of personal ownership over their own account with a corresponding lack of ownership over their spouse's account. For instance, one respondent wrote: "I need independence. I don't want to put my money in someone else's account...even if it is my husband." Another one wrote: "Our finances are not combined. We each may have different retirement needs and trajectories." In some cases, this preference for financial independence was associated with a lack of trust in ones' spouse. For instance, one respondent wrote: "My husband has bad spending habits, so I would rather have things in my account." Second, several respondents cited fairness as a key consideration when making these allocation decisions. Many respondents said they aimed for an equitable distribution of funds, regardless of the potential for higher employer matches. For instance, one respondent wrote: "I wanted to be fair and split the money even though my spouse's employer matches more money". Third, many responses highlighted the importance of risk, with some specifically addressing the risk of divorce, while others pointed to general concerns about the future. One respondent wrote: "I wanted to ensure that in the event of any issues such as divorce or death, each of us had money. I would rather hedge against this and pay a fee of lost money than have none at all if unforeseen circumstances happen". Another one wrote: "You never know what's going to happen, so you don't want to put all the money in your spouse's retirement account." Although interpreting these responses involves some degree of subjectivity, the lesson we take is that for those couples who deliberately chose to forego some employer match, a substantial driver is that respondents don't view their and their spouse's retirement accounts as interchangeable.

6 Conclusion

We show that many married couples fail to take full advantage of arbitrage opportunities available at the household level. Exploiting differences in matching incentives across employers, we find that a fifth of couples could increase their total retirement saving, by an average of over $700 per year, simply by reallocating some of their existing contributions to the account of the spouse with a higher marginal employer match rate. In the absence of any coordination, we estimate that the proportion of couples who could similarly increase their saving would be around 33%.

We show, by complementing our administrative data with a survey, that part of this behavior is accidental, driven by the fact that couples do not appreciate that there are potential gains from coordination, and that part is deliberate, driven in part by a desire for control, and in part by an (often erroneous) belief that one's 401(k) remains exclusively one's own property in retirement.

Models where couples fail to realize the surplus available to them have been proposed – for example, the models of Basu (2006) and Hertzberg (2016) have the implication that spouses rely on inefficient strategies, and in the models of Lundberg and Pollak (1993), Browning et al. (2010) and Del Boca and Flinn (2012) spouses can behave efficiently but subject to a non-cooperative and potentially inefficient threat point. These alternatives to the workhorse collective household model, are, however, used less frequently in applied work. We take our results as suggesting a greater role for non-cooperative models in the study of households' economic decisions.

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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.

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

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

[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).

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.

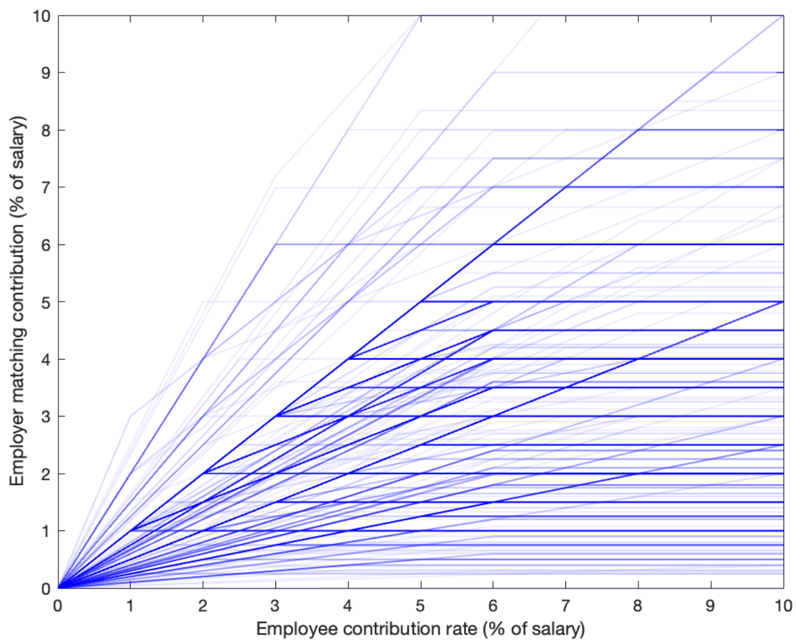
5. Step 5 involved checking and quality control. Any unusual entries were flagged for manual checking. This identified plans where the plan parameters were unusual (very large match rates, for example) or when plan features were coded as changed in one year and reverting the previous year – while in 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 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 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'

[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.

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.

A.2 Linking employees to DC plans

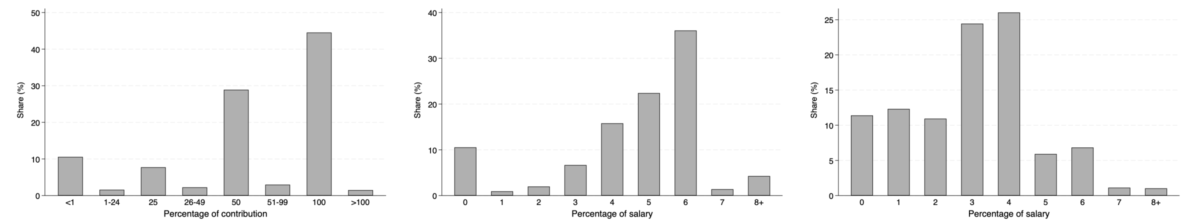
The hand-coded plan data described above needs to be linked to data on participant behavior, which we take from tax data, primarily Form W-2.

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

[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.

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).

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.

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

[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.

[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.

$Pr(j = j(i)|k = k(i), i \in S_{8955}) \geq 0.9$ 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 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

[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.

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}. 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;

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

[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.

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 $\tilde{m}(\alpha)$, 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

[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.

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 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 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%.

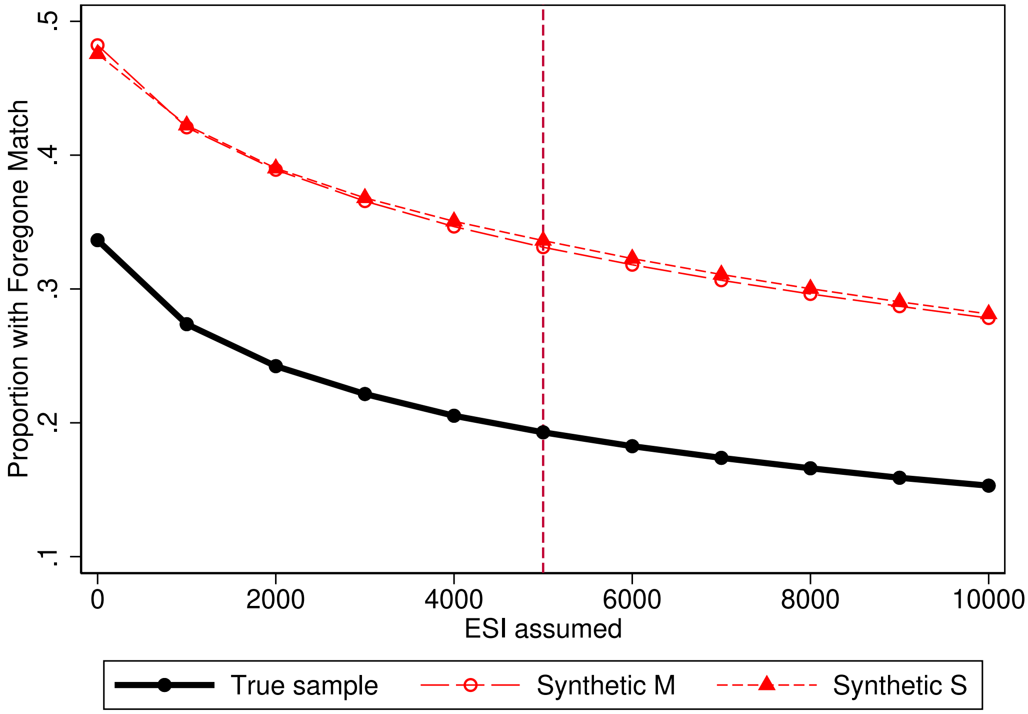


Figure A3: Sensitivity of results to ESI

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

[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).

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.

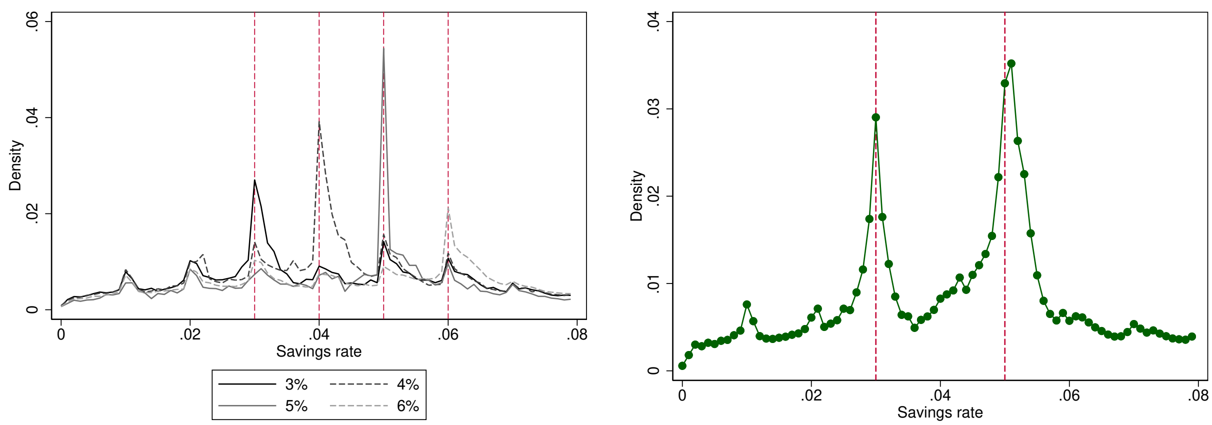
B.1 Bunching is observed at match caps

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 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.

[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.

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 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

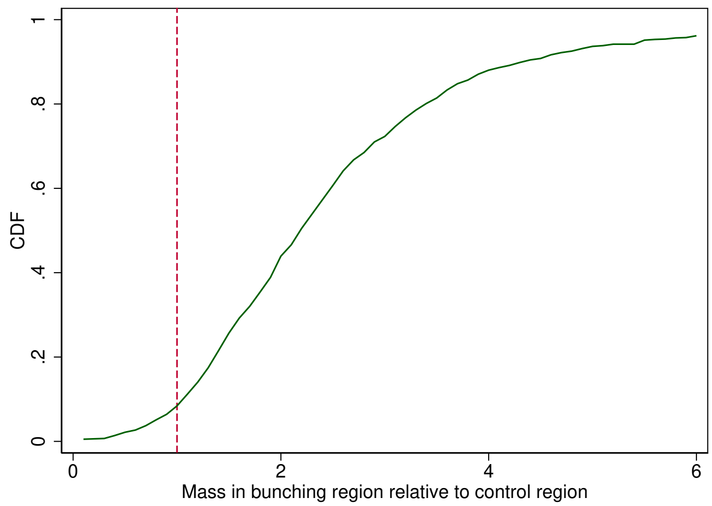


Figure B2: CDF of density in bunching region relative to density in control region

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.

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 par-

[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.

ticularly 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 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.

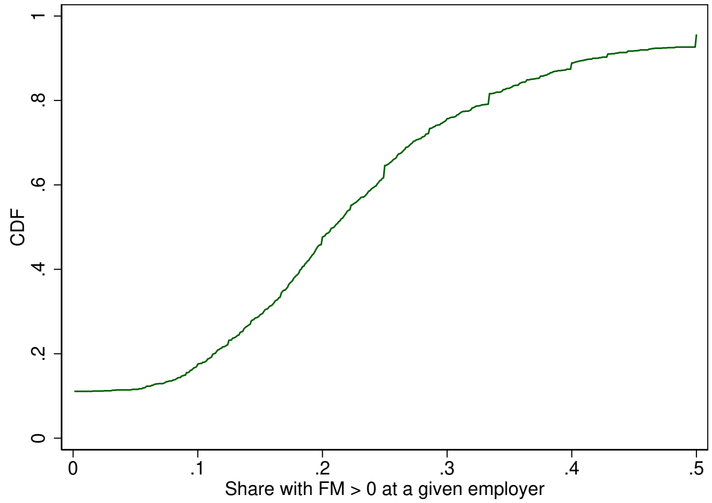


Figure B3: CDF of prevalence of foregone match at the employer level

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

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.

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 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

[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.

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 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

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

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 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

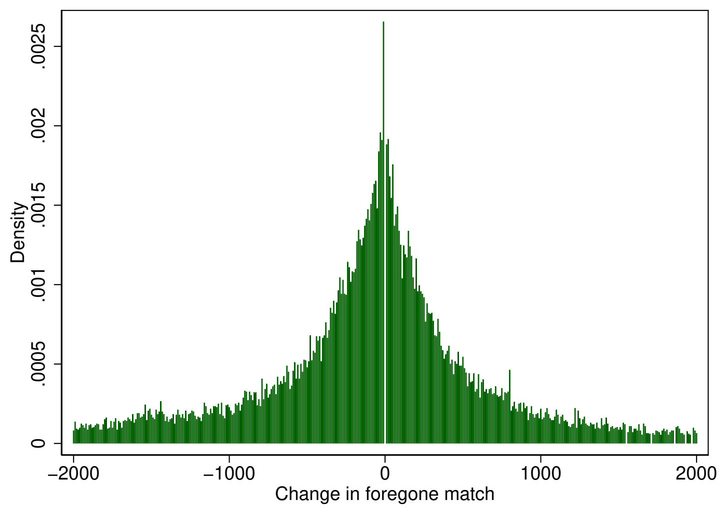
	Both spouses contribute	One spouse contributes
Neither above cap	0.254	0.147
One member above cap	0.406	0.193

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

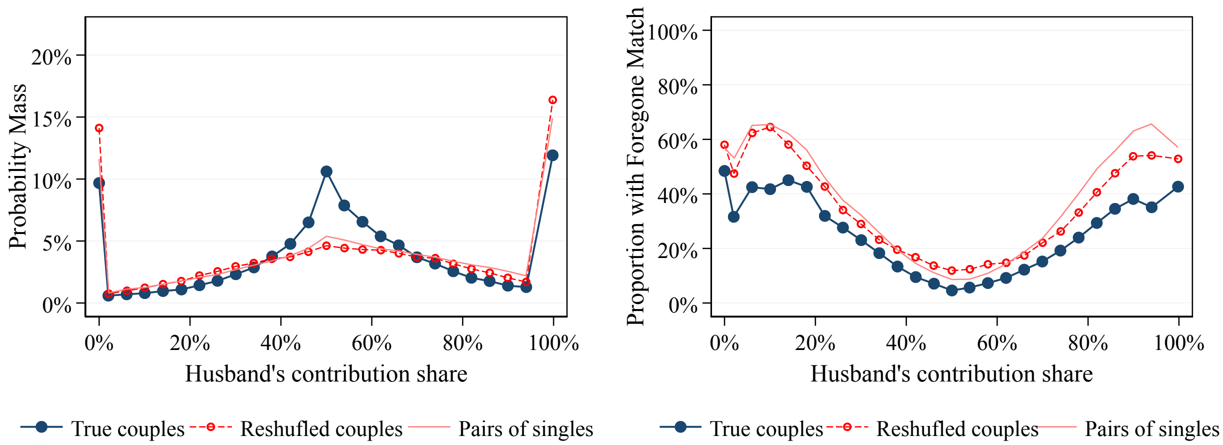
Figure D1 shows the distribution of the *change* in the quantity of foregone match by couples in which both spouses made an active change in their contribution rate (we omit a large mass at zero). This distribution has a mean of minus $11 – that is, the mean change in foregone match after an active change 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 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

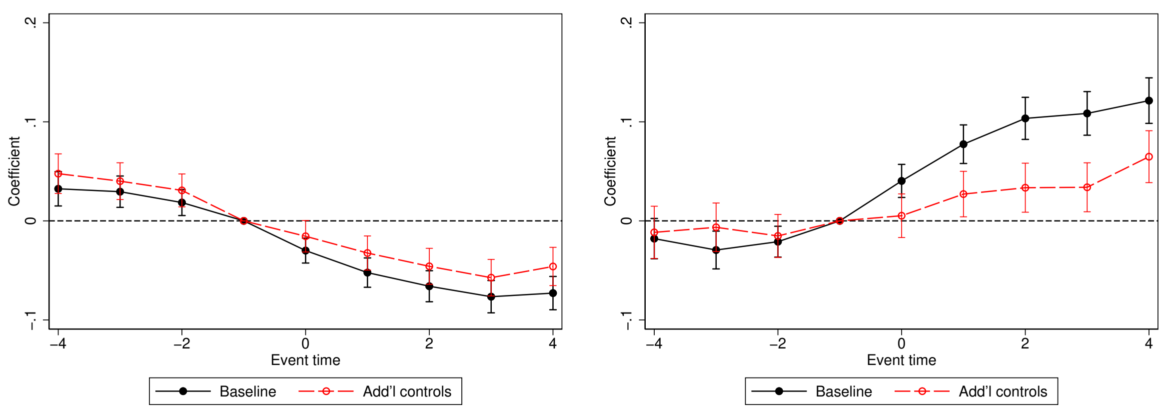
Figure 6 showed the density of how intra-household contribution shares are split between members of the couple, and shows how those shares relate to the probability of having foregone match. In that figure, we measure contributions as a proportion of earnings (i.e. savings rates) across spouses. Figure 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

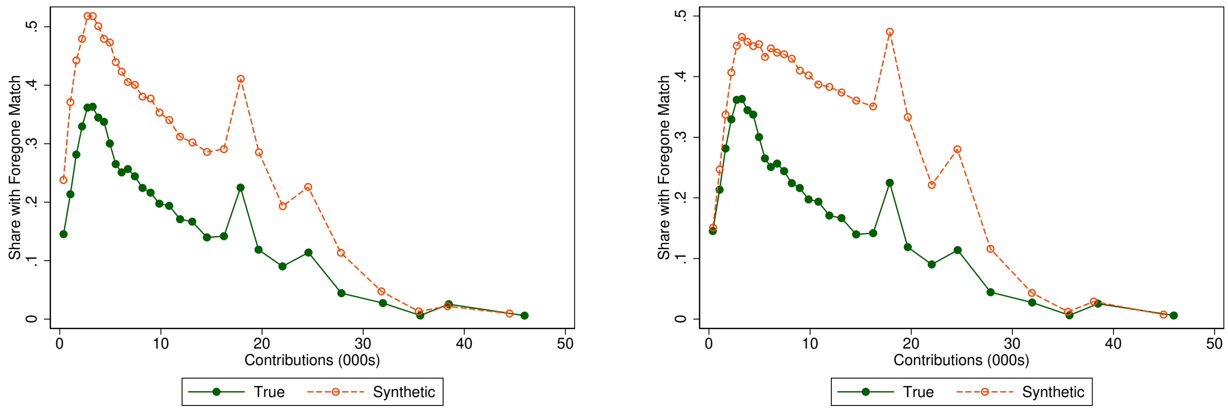
Notes: This table reports a series of regression coefficients, with one regression per column. The dependent variable is a dummy for $FM > 0$. All columns include interacted fixed effects for bins of total couple-level earnings and contributions. "P.E." stands for "primary earner" – the member of the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

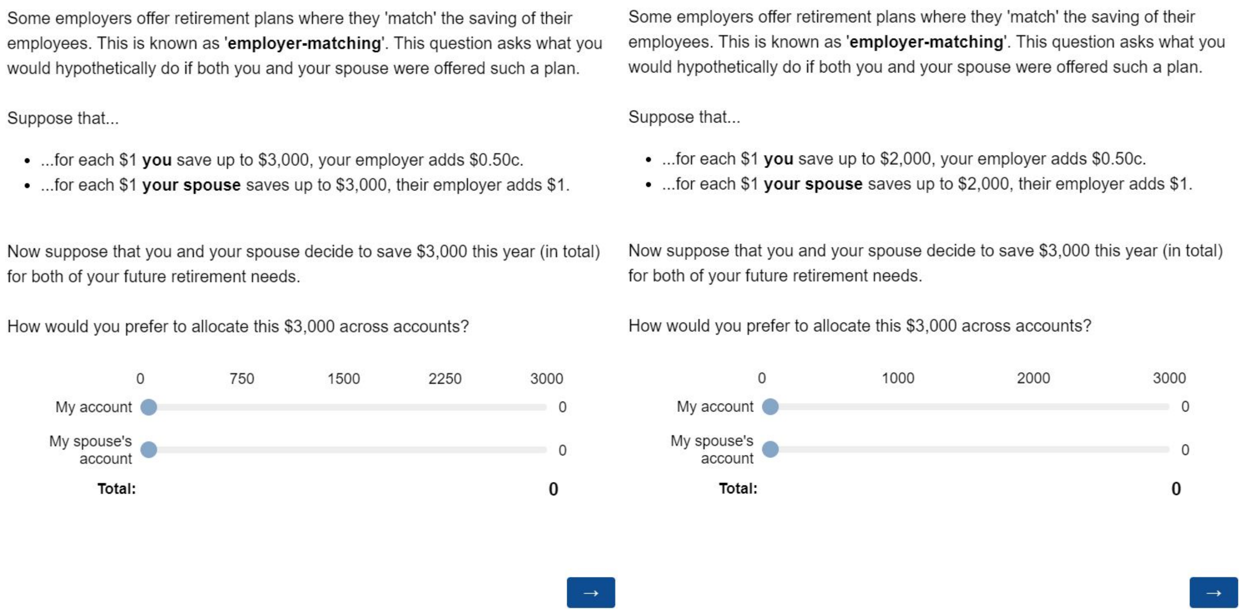
Figure E1 shows vignettes A and C. In 50% of cases respondents made their allocations using 'sliders' as shown. In 50% of cases they entered their answers in boxes.

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.

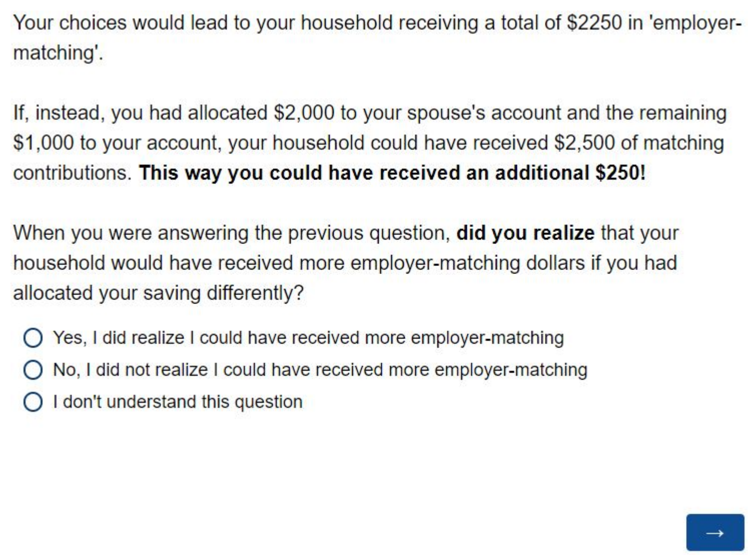
match after taking up the opportunity to re-allocate, we also ask them for a narrative justification of their choice. We use responses to these questions (in a manner discussed below) to distinguish between those who have "deliberate" foregone match (i.e., those who realized their allocation did not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



Your choices would lead to your household receiving a total of $2250 in 'employer-matching'.

If, instead, you had allocated $2,000 to your spouse's account and the remaining $1,000 to your account, your household could have received $2,500 of matching contributions. **This way you could have received an additional $250!**

When you were answering the previous question, **did you realize** that your household would have received more employer-matching dollars if you had allocated your saving differently?

- Yes, I did realize I could have received more employer-matching
- No, I did not realize I could have received more employer-matching
- I don't understand this question

→

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.