THE ROLE OF INFORMATION AND SOCIAL INTERACTIONS IN RETIREMENT PLAN DECISIONS: EVIDENCE FROM A RANDOMIZED EXPERIMENT*

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This paper analyzes a randomized experiment to shed light on the role of information and social interactions in employees' decisions to enroll in a Tax Deferred Account (TDA) retirement plan within a large university. The experiment encouraged a random sample of employees in a subset of departments to attend a benefits information fair organized by the university, by promising a monetary reward for attendance. The experiment multiplied by more than five the attendance rate of these treated individuals (relative to controls), and tripled that of untreated individuals within departments where some individuals were treated. TDA enrollment five and eleven months after the fair was significantly higher in departments where some individuals were treated than in departments where nobody was treated. However, the effect on TDA enrollment is almost as large for individuals in treated departments who did not receive the encouragement as for those who did. We provide three interpretations—differential treatment effects, social network effects, and motivational reward effects—to account for these results.

I. INTRODUCTION

There is growing concern in the United States about low levels of savings for retirement. For most U. S. families, employers' pensions are the main source of cash income during retirement, over and above Social Security benefits (see, e.g., Poterba, Venti, and Wise [1996]). However, over the last 25 years, traditional Defined Benefits and Defined Contribution employer pension plans where employee participation is mandatory have been partly replaced with Tax Deferred Account (TDA) retirement plans such as 401(k)s where employees choose whether to participate and how much to save for their retirement (see Poterba, Venti, and Wise [2001]). As a result, most U. S. workers now have to make a decision about how much to save for their retirement,

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instead of being passive participants in their employer's pension plan. This makes it very important to understand how retirement savings decisions are made.

Deciding how much to save for retirement and how to invest requires solving a complicated intertemporal optimization problem, and having information about the rules governing different instruments. In such a context, one could expect that information may have a large impact on savings behavior. As a result, financial education is considered a potentially important avenue to improve the quality of financial decision making, both by policy makers [Summers 2000] and by companies. A telephone survey we conducted with all Fortune 500 companies revealed that 71 percent of these companies systematically hold financial information sessions. A further 10 percent conducts them occasionally. Bernheim and Garrett [1996], Bayer, Bernheim, and Scholz [1996], and Bernheim, Garrett, and Maki [2001], among others, present evidence that participation in a firm's savings plan is higher when firms offer financial education. However, they recognize that an employer's decision to provide this information might be endogenous, which complicates the interpretation of these differences.

Because how much and how to save is a difficult decision, it is also likely that individuals' decisions are affected by the decisions of others in their peer group. First, they may obtain information about the employer retirement plan from discussions with their colleagues, or make inferences based on observing their decisions. Second, consumption and savings behavior may be subject to peer pressure and social norms, leading to conformity in behavior. As a result, social network effects within the workplace might play an important role in the decision to contribute to 401(k) retirement plans.

This paper analyzes the evidence from a randomized experiment, designed to shed light on both the role of information and social interactions on the employees' decision to enroll in the employer-sponsored TDA plan of a large university. This allows us to overcome some of the very difficult identification problems in the presence of peer effects, described notably in Manski [1993, 1995].

Each year, the university organizes a benefits fair and invites all of its employees to the fair in order to provide information on benefits. In particular, a stated goal of the fair is to increase the enrollment rate in TDA which is relatively low (around 35 percent). Obviously, comparing the TDA enrollment decisions of fair attendees with those who did not attend the fair would not provide convincing evidence of a causal effect of fair attendance on TDA enrollment, because the decision to attend the fair is endogenous.[1] To circumvent this selection problem, we have implemented the following experiment. We selected a random sample of employees not yet enrolled in the TDA and sent them an invitation letter promising a $20 reward for attending the fair. This type of experiment is a classical encouragement design, often used in medical science, where treatments are offered to a random group of patients who then decide whether or not to take the treatment. Furthermore, we designed our experiment such that we are able to estimate social interaction effects. Namely, "treated" individuals who were sent the invitation letter were selected only within a random subset of departments (the "treated" departments).

The first stage of our study analyzes the effect of the invitation letter on fair attendance. Treated individuals are more than five times as likely to attend the fair as control individuals. Interestingly, nontreated individuals in treated departments are three times as likely to attend the fair as control individuals in nontreated departments, despite the fact that only original letter recipients could claim the $20 reward. This shows that the invitation letters not only increased the fair attendance rate for individuals who received them but also had a spillover social effect on their colleagues within departments.

The second stage of the study tries to estimate the causal effect of fair attendance and social effects on the decision to enroll in the TDA. We show that, five and eleven months after the fair, individuals in treated departments are significantly more likely to have started contributing to the TDA than control individuals. This shows that our experiment, and hence the fair, was successful in increasing TDA enrollment. However, there is no significant difference in TDA enrollment between those who actually received our encouragement letter and those in the same departments who did not. We propose three different interpretations, not necessarily mutually exclusive, to account for these facts. First, this could be explained by social effects at the department

1. For example, individuals who had already decided to enroll, but are not sure exactly how much they want to contribute, may be more likely to attend the fair. See Madrian and Shea [2002] for evidence of selection in the decision to attend information sessions within a large firm.

level. Fair attendees might be able to spread information obtained from the fair in their departments. Second, our results could also be explained by differential treatment effects. Employees who come to the fair only because of the financial reward are different from those who decide to come to the fair because of their colleagues, and it is plausible to think that the treatment effect is larger for the latter group than for the former. Finally, our results might also be explained by motivational reward effects. Paying individuals to attend the fair might affect their subjective motivation and therefore the perceived value or quality of the information they obtain at the fair. Our experiment does not allow us to separately identify these three effects, but it allows us to conclude that the important decision about how much to save for retirement can be affected by small shocks such as a very small financial reward or the influence of peers, and thus does not seem to be the consequence of an elaborate decision process.

The remainder of the paper is organized as follows. Section II presents a brief discussion of the mechanisms by which financial education and social interactions can affect retirement savings decisions. Section III describes the benefits fair and the design of our experiment. Section IV discusses the reduced-form evidence. Section V develops a simple model to interpret our results. Finally, Section VI offers a brief conclusion.

II. Information and Social Interactions in Savings Decisions

A number of recent studies have emphasized the important role of factors others than financial incentives in the decision to enroll in TDA plans. Madrian and Shea [2001] and Choi, Laibson, Madrian, and Metrick [2001a, 2001b] show that default rules have an enormous impact on employees' participation, contributions, and asset allocations. When employees are enrolled by default in a TDA, very few opt out, and most employees do not change the default contribution rate or the default allocation of assets. This evidence could be interpreted in two ways: either individuals lack information, and interpret the default option as a signal, or they do not think very much about their retirement savings, and can be influenced by very small changes in their environment. Distinguishing these two mechanisms is important, since they have very different policy implications. If lack of information is important, this suggests a potentially important role for financial education sessions, through which individuals can obtain general information about retirement plan features as well as be guided through their intertemporal maximization decision. However, if financial education has only a modest impact on retirement plan decisions, this suggests that the second hypothesis is true.

The literature on social interactions suggests that in both cases, social interactions are likely to affect retirement decisions. First, individuals may learn from their coworkers, either through discussions or by making inferences from their actions. The literature on informational cascades [Bikhchandani, Hirshleifer, and Welch 1992; Banerjee 1992; Ellison and Fudenberg 1993] provide reasons why information (correct or not) obtained from coworkers may be an important factor in deciding whether to participate and how to invest—giving rise to peer effects. Second, savings decisions may be influenced by social norms or beliefs about social norms. By observing coworkers, people can learn about the proper behavior of their social group, as emphasized by models of conformity [Bernheim 1994]. Individuals may want to maintain the same consumption level as what is common in their social group. In both cases, there is a "social multiplier" effect: the aggregate impact of an intervention on a group is larger than the sum of its effects on each individual's decision. As discussed in Glaeser, Sacerdote, and Scheinkman [2002], it is often important for policy purposes to separate direct individual effects from social multiplier effects.

There is a growing empirical literature which shows evidence of social interaction effects in a number of areas. Some empirical papers have focused on information transmission,[2] while others have focused on peer pressure.[3] Most of these studies are observational and hence subject to difficult identification problems [Manski 1993, 1995].

Glaeser, Sacerdote, and Scheinkman [1996, 2002] propose two main avenues to obtain suggestive evidence on the presence of social interaction effects. First, in the presence of positive spillovers, the variance across social groups will be larger than what would be predicted by random draws. Second, there will be

2. See Besley and Case [1994] and Foster and Rosenzweig [1995] on technology adoption in developing countries.

3. See, for example, Evans, Oates, and Schwab [1992] on teenagers' behavior, Bertrand, Mullainathan, and Luttner [2000] on welfare participation, and Munshi [2000] on contraception.

a correlation between individual behavior and a prediction of aggregate behavior based on demographic characteristics in the group. Duflo and Saez [2002a] show that, in the university studied in this paper, both effects are present: there is little variance of participation *within* departments, compared with the variance in participation rates *across* departments, and individual participation rates are correlated with predicted participation in their peer groups. While this evidence is suggestive, it might be contaminated by omitted variables, correlated within the group and correlated with the observed variables used to predict aggregate participation rates. To address this problem, we set up a randomized experiment, where we affect the incentives of a subset of the peer group in some randomly selected groups, and evaluate whether the impact of this intervention extends beyond the targeted group, which would be direct evidence of a social multiplier effect.[4]

III. Context and Experiment Design

III.A. Benefits and the Benefits Fair

The university we study has approximately 12,500 employees. About a quarter of the employees are faculty members. Our study was limited to nonfaculty employees only.[5] The university provides retirement benefits to its employees through a traditional mandatory pension plan, but employees can also voluntarily contribute to a complementary Tax Deferred Account (TDA) 403(b) plan. Every employee can contribute to the 403(b) plan any percentage of their salary up to the IRS limit ($10,500 per year for each individual in 2001). The university does not match contributions. Employees can choose how to invest their contributions in any combination of four different vendors.

Each year, the university organizes a benefits fair where all

4. Two recent studies have used experimental or quasi-experimental situations to study social interaction effects. Katz, Kling, and Liebman [2001] evaluate a randomly assigned housing voucher program whereby households living in high poverty public housing projects were given the opportunity to move out of the project. Sacerdote [2001] analyzes peer effects among first-year students at Dartmouth College randomly assigned to dorms. Both studies have found evidence of spillovers.

5. Duflo and Saez [2002a] present suggestive evidence that staff employees' TDA choices are not influenced by faculty choices and vice versa. Furthermore, staff employees may be more representative of average U. S. workers than faculty members.

employees are invited to come and learn about the different kinds of benefits (such as health benefits, retirement benefits, etc.) provided by the university. The fair is held on two consecutive days in early November in two different locations, each one close to the two separate main university campuses. About one week before the fair, every employee receives a letter through the university mail system inviting her to attend the fair. This letter also provides a brief description of the event. At the same time, under separate cover, every employee receives a packet describing in detail university benefits along with enrollment forms. November is "open enrollment" month during which each employee may change her benefits choices by submitting the enrollment form. If the employee does not send back the form, her benefits choices are automatically carried over from the previous year. However, employees are free to enroll in the TDA or change their contribution level or investment decision at any time throughout the year.

In both locations, the fair is held in a large hotel reception room. There are a large number of stands representing the university Benefits Office, and the various health and retirement benefits service providers. The university Benefits Office offers information on all benefits through direct conversation with Benefits Office staff present at the fair, and through a number of information pamphlets freely available at their stand. The benefits office also provides information on how the other stands at the fair are organized. These other stands are run by each of the specialized service providers. For example, each of the mutual fund vendors has a stand at which they provide information about the TDA plan and the specific services they offer within that plan. The fair also offers individuals the chance to use a specially designed computer program to analyze their specific situation. Employees are free to come any time during the three and a half hours during which the fair is held, and visit any number of stands they want.

III.B. Experiment Design

The university organizes the annual fair in order to disseminate information about benefits and to help its employees make better decisions. The benefits office of the university realizes that the participation rate among staff (34 percent) is too low compared with that at other universities, and suspects that this may be due to lack of information.

In order to identify the causal effect of fair attendance on TDA enrollment, we set up an "encouragement design," by promising a random subset of employees a small amount of money for attending the fair. In order to shed light on social effects within departments, we sent those letters only within a random subset of departments. There are thus two distinct treatments in our experiment: receiving the letter, and being in the same department as someone who receives a letter.

We used a cross section of administrative data provided by the university on all its employees as of August 2000. We restricted the sample to staff employees (i.e., nonfaculty employees) aged less than 65 and eligible to participate in the TDA. Of the 9700 employees meeting these criteria, around 3500 were enrolled in the TDA as of August 2000. From now on, we refer to these individuals as the pre-enrolled individuals. The remaining 6200 individuals were not enrolled in the TDA by August 2000. As very few employees stop contributing to the TDA once they are enrolled, we focus on the decision to start participating in the TDA. Thus, the sample of 6200 nonenrolled individuals is our sample of primary interest.[6]

The University is divided into 330 departments. Departments include each of the academic departments such as Economics or Cell Biology, etc. In addition to academic departments, there are many administrative departments. For example, each library is a separate department. Each of the dining halls is also a department. In most cases, each department has a single geographical location. Departments sometimes share a same building or floor within a building, but even in those cases, work interactions within departments are much more intense than across departments.[7] Of course, there is communication across departments, but it is mainly concentrated among higher ranked employees within departments.[8] The average number of staff employees per department is 30, but the median size is much smaller, around 15. Therefore, except in a few large departments,

6. Only 80 of the 3500 employees enrolled in the TDA stopped contributing during the one-year period we examine. More than five times as many employees started contributing to the TDA during the same period. We have not found any significant differences in the decision to stop contributing to the TDA across treated and nontreated departments.

7. Academics know very well that, even when departments are close geographically, interactions across departments are always minimal. To a large extent, the same is true for staff in administrative departments.

8. For example, administrative managers from different departments participate in many meetings with the central administration.

we would expect each employee to know most of her colleagues in the department. The fact that departments may not correspond exactly to social units should lead to an attenuation bias in our social network effects estimates.

In the first step, we randomly selected two-thirds of the departments of the university (220 out of a total of 330) as follows. In order to maximize the power of the experiment (in a context in which we know there are strong department effects), we first matched departments according to their size (i.e., number of employees) and participation rate in the TDA before the fair. We separated departments into deciles of participation rates among the staff. Each decile contains 33 departments. We then ranked them by size within each decile, and formed groups of three departments by putting three consecutive departments on these lists in the same triplet. Within each of these triplets, we randomly selected two departments to be part of the group of treated departments. From now on, we denote by the dummy variable D the treatment status of departments. We have $D = 1$ in treated departments and $D = 0$ in control departments.

In the second step, within each of the treated departments, any individual not enrolled as of August 2000, was selected with a probability of one-half.[9] This treatment group is composed of 2039 individuals. From now on, we denote by the dummy variable L the selection status of individuals. We refer to this group as the Treated individuals and denote them by 11 ($D = 1$ for Treated department and $L = 1$ for being selected). The group formed by the employees in the treated departments who were not selected contains 2129 individuals and is denoted by 10 ($D = 1$ for Treated department and $L = 0$ for not being selected). In total, there are 4168 individuals in the treated departments. The control group is formed by employees in the control departments where no treatments were selected; it contains 2043 individuals and is denoted by 00 ($D = 0$ and $L = 0$).

One week before the fair, we sent a letter via university mail to the 2039 employees in the treatment group 11. The letter reminded them of the fair and informed them that they would receive a check for $20 from us if they were to come to the fair and register at our desk. This letter is reproduced in the Appendix.

At the fair, we set up a stand for the employees who received our invitation letter to register their name. Unfortunately, the

9. This selection probability is independent across individuals.

Benefits Office did not authorize us to record the names of the fair participants who did not receive our letter. However, we recorded their total number: a student stood at the fair entrance and distributed a coupon to each person who entered the hall. The coupons had different colors according to the status of the participant (active or retired), which allowed us to count the number of active employees who attended the fair. Everybody had to pass through the narrow entrance to enter the fair, and the few people who refused the coupon were carefully counted. We are thus confident that we accurately recorded the number of participants. In order to collect information on the TDA status and the department affiliation of all the fair participants, we organized a raffle. The coupons that were distributed at the entrance of the fair had two parts, with a number written twice. Each fair attendant who wanted to participate in the raffle gave us half of the coupon. We asked all the raffle participants their department affiliation and whether they were currently enrolled in the TDA. The raffle was held every 30 minutes, and the prize was a \$50 Macy's gift certificate. A total of 1617 active employees attended the fair, and 573 of them had received our letter. Out of the remaining 1044 employees, 766 (i.e., about three-quarters) came to play the raffle and registered their department affiliation and TDA enrollment status. An important issue that arises is whether there was selection by $D = 1$ versus $D = 0$ departments in who decided to play the raffle (and hence provide their department affiliation and TDA status). We do not believe this was the case. Most of those who refused to play the raffle did so because they visited our stand just after the previous raffle had been played, and did not want to stay at the fair long enough to wait for the next raffle. Therefore, we assume that fair attendants who did not register their department affiliation are distributed between $D = 1$ and $D = 0$ departments as those who did register. Therefore, in what follows, we scale up the attendance recorded in each department by a factor of 1044/766.[10]

In order to assess the effects of the experiment and the fair on TDA participation, the university provided us with three waves of data. The first wave was obtained in September 2000, just before the fair. The second wave was from March 2001 (4.5 months after the fair), and the third wave from October 2001 (11 months after the fair).

10. We will discuss how modifying this assumption would affect our results.

IV. Results: Summary Statistics and Reduced-Form Differences

In the presence of social interactions, employees who work in departments where some people received the letter can be affected by the experiment even if they did not receive the letter themselves. They may be more likely to come to the fair themselves, because they are reminded by others of the event, or because employees come to the fair in groups.[11] They may also be more likely to enroll in the TDA even if they do not come to the fair themselves, either because they are directly influenced by the action of those who went to the fair, or because these individuals share the information they gathered at the fair. Thus, employees are potentially subjected to two kinds of treatments: they can receive the invitation letter themselves (group 11), or they can be in a department where some employees received the letter (group 10 and group 11). Those who receive the letter are, obviously, subject to both treatments.

The summary statistics are displayed in Table I, broken down into four groups. In columns (1) to (3) we present the statistics for individuals who belong to treated departments. Column (1) has the statistics for the entire group (group $D = 1$), column (2) has the statistics for the group of treated individuals (group 11), and column (3) has the statistics for the untreated individuals in treated departments (group 10). In column (4) we present the statistics for individuals who belong to the untreated departments (group 00).[12]

Panel A presents background characteristics. In the first wave (in September 2000, before the fair), a very small proportion of employees started contributing to the TDA (the first wave is from September 2000, but we used data from August 2000, to construct the randomization), but there is no apparent difference across groups in these proportions. Since we are interested in changes caused by the fair, we focus in the remainder of the analysis on individuals who were still not enrolled in the first wave (i.e., by September 2000). Because the groups were chosen randomly, the mean of observable characteristics such as sex, years of service, annual salary, and age, are very similar across groups, and none of the differences are significant.

11. This is something we observed at the fair.

12. It is important to note that all these statistics (except the first row of Panel A and the second row of Panel B) focus only on individuals not enrolled in the TDA in September 2000, before the fair.

TABLE I
DESCRIPTIVE STATISTICS, BY GROUPS

	Treated departments			
	All (group $D = 1$)	Treated (group $D = 1$, $L = 1$)	Untreated (group $D = 1$, $L = 0$)	Untreated departments (group $D = 0$)
	(1)	(2)	(3)	(4)
PANEL A: BACKGROUND CHARACTERISTICS				
TDA participation before the fair (Sept. 2000)	0.010	0.009	0.011	0.012
	(.0015)	(.0021)	(.0022)	(.0024)
Observations	4168	2039	2129	2043
Sex (fraction male)	0.398	0.400	0.396	0.418
	(.0076)	(.0109)	(.0107)	(.011)
Years of service	5.898	5.864	5.930	6.008
	(.114)	(.161)	(.16)	(.157)
Annual salary	38,547	38,807	38,297	38,213
	(304)	(438)	(422)	(416)
Age	38.3	38.4	38.2	38.7
	(.17)	(.24)	(.24)	(.24)
Observations	4126	2020	2106	2018
PANEL B: FAIR ATTENDANCE (REGISTRATION DATA)				
Fair attendance rate among non-TDA enrollees	0.214	0.280	0.151	0.049
	(.0064)	(.01)	(.0078)	(.0048)
Observations	4126	2020	2106	2018
Fair attendance rate for all staff employees	0.192			0.063
	(.0132)			(.0103)
Observations	6687			3311
PANEL C: TDA PARTICIPATION (ADMINISTRATIVE DATA)				
TDA participation rate after 4.5 months	0.049	0.045	0.053	0.040
	(.0035)	(.0049)	(.0051)	(.0045)
Observations	3726	1832	1894	1861
TDA participation rate after 11 months	0.088	0.089	0.088	0.075
	(.005)	(.0071)	(.007)	(.0065)
Observations	3246	1608	1638	1633

a. Standard errors are in parentheses.
b. The first part of Panel B includes all individuals not enrolled in the TDA by September 2000. The second part includes all employees (enrolled or not in the TDA).
c. The average fair participation in the nontreated departments was obtained from the registration information collected at the fair. Since only 75 percent of the participants registered, the participation was adjusted by a proportionality factor.
d. Demographic information and TDA participation are all obtained from administrative data.

In Panel B we can see that our inducement strategy had a dramatic effect on the probability of attending the fair: in treated departments, as many as 21.4 percent of individuals attended the fair. In control departments, fewer than 5 percent of individuals attended the fair. Comparing treated individuals versus controls in the treated departments in columns (2) and (3) shows that social effects account for a large fraction of the effect of our experiment on fair attendance. The fair attendance rate of those who received our letter is 28 percent, and is 15.1 percent for those in the treated departments who did not receive the letter. Thus, the difference in the attendance rate between group 10 and group 00 (which is solely due to social effects) is over 10 percentage points.[13]

In Panel C we look at TDA participation. After 4.5 months relatively few people have enrolled. However, employees in treated departments are already significantly more likely to be enrolled than employees in control departments (4.9 percent versus 4 percent). However, individuals in group 11 are not more likely to be enrolled than individuals in group 10. The difference between groups 10 and 00 is relatively large at 1.3 percentage points. Eleven months after the fair, enrollment is higher still, and the difference between treated departments and control departments is 1.4 percentage points. The difference between groups 11 and 10 is now positive, but still very small and insignificant. The difference between group $D = 1$ and group $D = 0$ remains equal to 1.3 percentage points.

In order to analyze the differences, we consider simple reduced-form regression specifications. Denote, respectively, by f_{ij} and y_{ij} the fair attendance and the TDA enrollment decisions of individual i in department j. Similarly, L_{ij} is the dummy for receiving the inducement letter, and D_j the treatment status of the department. The average effects on fair attendance and TDA enrollment of being in a treated department ($D = 1$) versus a control department ($D = 0$) (irrespective of individual treatment status L) are captured by the following specifications:

$$f_{ij} = \alpha_1 + \beta_1 D_j + \epsilon_{ij}, \quad (1)$$

and

13. This result is, of course, sensitive to the assumption we made about department affiliation of fair attendants who did not register at our desk. If we make the extreme assumption that all nonregistered individuals come from $D = 0$ departments, the fair participation rate for group 10 would drop to 11 percent but still be higher than for group 00 (which would go up to 9 percent). In addition, we show below that the increase in fair attendance in group 10 is paralleled by an increase in their TDA participation.

TABLE II
REDUCED-FORM ESTIMATES (OLS)

	Dependent variable		
	Fair attendance	TDA enrollment after	
		4.5 months	11 months
	(1)	(2)	(3)
PANEL A: Average effect of department treatment			
Treated	0.166	0.0093	0.0125
Department dummy D	(.013)	(.0043)	(.0065)
Observations	6144	5587	4879
PANEL B: Effect of letter and department treatment			
Letter dummy L	0.129	−0.0066	0.0005
	(.0226)	(.0061)	(.0102)
Treated	0.102	0.0125	0.0123
Department dummy D	(.0139)	(.0054)	(.0086)
Observations	6144	5587	4879

a. Dependent variables are individual fair participation (column (1)), TDA enrollment 4.5 months and 11 months after the fair (columns (2) and (3)).
b. Independent variable in Panel A is the department treatment dummy D.
c. Independent variables in Panel B are the individual letter dummy L and the department treatment dummy D.
d. All regressions control for the triplet of the department, gender, year of service, age, and salary.
e. Standard errors (in parentheses) are corrected for clustering at the department level.

$$y_{ij} = \alpha_2 + \beta_2 D_j + \eta_{ij}. \tag{2}$$

The estimates for β_1 and β_2 are reported on Panel A of Table II for fair attendance, (column (1)), and TDA enrollment after 4.5 months (column (2)) and 11 months (column (3)). These estimates correspond to the difference in fair attendance and TDA enrollment between treated and untreated departments reported in columns (1) and (4) of Table I, respectively. The regressions also include fixed effects for the stratification triplet (see Section III), as well as controls for background variables—gender, year of service, age, and salary. All standard errors are corrected standard errors for clustering at the department level.[14] Being in a treated department increases the probability of attending the fair by 16.6 percentage points. It also increases significantly the TDA

14. Adding the triplet dummies reduces the standard errors, by absorbing some unexplained differences across departments of similar size and prefair TDA enrollment rates. Baseline covariates are also included to improve the precision of our estimates.

enrollment rate by 0.93 and 1.25 percentage points (after 4.5 and 11 months).

Obtaining significant differences between these randomly chosen groups means that our experiment did have an impact on TDA enrollment. This impact is large in relative terms (an increase of 24 percent and 19 percent in the likelihood of enrollment after 4.5 and 11 months). However, because people update their TDA status very infrequently, it is small in absolute terms (an increase of only 1.25 percentage points of enrollment, on a base of 34 percent). This effect is tiny compared with interventions that change the default rules for TDA enrollment (such as in Madrian and Shea [2001] and Choi et al. [2001a, 2001b]) or that offer individuals the option of automatically allocating future pay raises to TDA contributions [Thaler and Benartzi 2001].

In order to estimate separately the effect of receiving the letter personally and that of just being in a department where some colleagues received the letter, we run the following reduced-form regressions:

$$f_{ij} = \alpha_1 + \mu_1 L_{ij} + \delta_1 D_j + \epsilon_{ij}, \tag{3}$$

and

$$y_{ij} = \alpha_2 + \mu_2 L_{ij} + \delta_2 D_j + \eta_{ij}. \tag{4}$$

The results of these regressions are reported in Panel B of Table II. The parameters μ_1 and μ_2 capture the difference in fair attendance and TDA enrollment between groups 11 and 10 (columns (2) and (3) of Table I). The parameters δ_1 and δ_2 capture the difference in fair attendance and TDA enrollment between groups 10 and 00 (columns (3) and (4) of Table I). Consistent with the results from Table I, being in a treated department increases the probability of attendance by 10.2 percentage points, and receiving the letter increases it further by 12.9 percentage points. These results suggest that the promise of the $20 reward did have a strong impact on the decision to attend the fair. Moreover, the fact that colleagues received the letter also increased one's probability of attending. These peer effects can be explained in two ways. First, an employee who sees colleagues receiving the inducement letter might be reminded of the fair and be led to think that this is an important event (worth rewarding employees for attending) and thus might decide to attend herself. Second, individuals who receive the letter and decide to go to the fair might ask their colleagues to join them. Our experiment does not allow us to separate these two effects but does allow us to conclude that social interactions play an important role in the decision to attend the fair.

Columns (2) and (3) of Table II show that receiving the letter does not increase the probability of enrolling in the TDA (the effect is slightly negative but insignificant after 4.5 months and slightly positive but insignificant as well after 11 months), while being in a treated department does increase the probability of TDA enrollment (by 1.25 and 1.23 percentage points after 4.5 and 11 months).[15] The next section presents simple models to interpret these results.

V. Estimating the Effects of the Experiment

V.A. The Model

We posit the following simple specification to explain the effect of the experiment on TDA enrollment:

$$y_{ij} = \alpha + \gamma_i f_{ij} + \Gamma \cdot D_j + u_{ij}. \tag{5}$$

This equation states that an individual's decision to participate in the TDA is potentially influenced by their own attendance at the fair as well as by whether some colleagues received inducement letters (treatment department dummy D). The effect of being in a treated department could be direct (when many people go to the fair, their colleagues feel compelled to go to the fair as well, and to enroll in the TDA), channeled through conventional peer effects (higher fair attendance in a department leads to higher TDA participation, which in turn influences the participation of others), or resulting from the diffusion of the information obtained at the fair. Here again, these effects cannot be separately identified, and we will make no attempt to separate them.

The individual fair effect γ_i may vary across individuals in our sample, for at least two reasons. First, the effect of attending the fair on TDA participation could vary across individuals. In particular, our experiment induced two distinct groups of individuals to attend the fair. Those who were in treated departments ($D = 1$), and those who in addition to being in a treated department, received the inducement letter themselves ($D = 1$, $L = 1$).

15. The estimate after 4.5 months is significant at the 5 percent level while the coefficient after eleven months has a t-statistic of 1.45.

As we discuss below, the effect of the fair may be different for these two groups.

Second, it is conceivable that, even for an individual who would have come to the fair with no external inducement, receiving the letter offering the $20 reward affects the fair effectiveness. Because the individual is now paid to attend the fair, she might convince herself that she is coming just for the $20 and thus that she is not really interested in the content of the fair. This type of effect is not standard in economic models, but there is substantial evidence in the psychology literature on the motivational consequences of rewards. This literature is summarized in Ross and Nisbett [1991, pp. 65–67] and more recently in Frey and Jegen [2001].

This motivational reward effect can be captured by assuming that the treatment effect γ_i is potentially (negatively) correlated with the letter treatment L_{ij}. In order to simplify the presentation, let us assume that γ_i takes the following simple form:

$$\gamma_i = \gamma_i^S - \nu L_{ij}, \tag{6}$$

where γ_i^S (the standard treatment effect component) is independent of L_{ij}, and ν represents the motivational reward effect. Assuming that no motivational reward effect amounts to simply assuming that $\nu = 0$ and thus that γ_i is independent of L_{ij}.

Each individual belongs to one of the groups 11, 10, or 00. In order to define treatment effects of fair attendance on TDA enrollment, it is useful to introduce the notion of potential outcomes for fair attendance. For each individual, we denote by $f_{ij}(11)$, $f_{ij}(10)$, and $f_{ij}(00)$ the fair attendance decision of individual i, if he had been in group 11, 10, or 00. Obviously, for each individual ij, we observe only one of the three potential outcomes for fair attendance. As the literature on differential treatment effects has recognized [Imbens and Angrist 1994], in order to be able to identify parameters of interest, we need to make the following assumption.

ASSUMPTION 1. Monotonicity assumption. For each individual i, $f_{ij}(11) \geq f_{ij}(10) \geq f_{ij}(00)$.

This assumption states that receiving the letter can only encourage an individual to attend the fair (and in no case deter them), and that having one's colleagues receive the letter can also only encourage an individual to attend the fair. This assumption sounds very plausible in the situation we analyze. The Monotonicity assumption implies that the population can be partitioned into four different types.

First, the *never takers* are individuals such that $f_{ij}(11) = f_{ij}(10) = f_{ij}(00) = 0$. These individuals would not attend regardless of the group to which they belong. Second, we define the *financial reward compliers* type as individuals such that $f_{ij}(11) = 1 > f_{ij}(10) = f_{ij}(00) = 0$. These individuals attend the fair only if they receive the letter with the financial reward promise. Third, we define the *social interaction compliers* as individuals such that $f_{ij}(11) = f_{ij}(10) = 1 > f_{ij}(00) = 0$. These individuals would not attend the fair if nobody in their department receives the letter, but would attend the fair if they are in a treated department (whether or not they themselves receive the letter). Finally, we define the *always takers* as individuals such that $f_{ij}(11) = f_{ij}(10) = f_{ij}(00) = 1$. These individuals attend the fair regardless of the group to which they belong.

We make the following additional assumption.

ASSUMPTION 2. Exclusion restriction assumption. u_{ij} is independent of L_{ij} and D_j.

The assumption that the error term u_{ij} is independent of the letter assignment status L_{ij} means that the letter inviting the employee to the fair has no direct effect on TDA participation decisions of those who do not attend the fair (beyond its effect on individual and departmental fair attendance). Likewise, the fact that other people received the letter is assumed to have no effect on TDA participation. To ensure the validity of Assumption 2, we did not mention TDA in the letters, and the letter did not contain any mention of the employee's TDA status (the letter is reproduced in the Appendix).[16]

It is now apparent that there are four parameters of interest in the model: the average treatment effect for financial reward compliers $E[\gamma_i | f_{ij}(11) - f_{ij}(10) = 1]$, the average treatment effect for social interaction compliers $E[\gamma_i | f_{ij}(10) - f_{ij}(00) = 1]$, the social network effect parameter Γ, and the motivational reward effect ν. However, our experiment provides us with only two instruments L_{ij} and D_{ij}, making it impossible to identify all four

16. A follow-up questionnaire which contained precise questions about savings and the TDA did not have an effect on TDA enrollment (see Duflo and Saez [2002b] for details). As a result, it is highly unlikely that the inducement letter could have had an effect on TDA enrollment.

parameters. Only if we make additional assumptions about two of these four parameters can we estimate the remaining two parameters. In the next subsection we discuss alternative sets of assumptions under which the remaining parameters of the model could be estimated. Our goal is not to claim that any particular set of assumptions is correct, but rather to explore the implications of each assumption, and to provide bounds to the different effects.

V.B. Interpretation under Alternative Identification Assumptions

If we assume that there is no motivational reward effect ($\nu = 0$) and γ_i is equal to γ for all individuals, equation (5) reduces to

$$y_{ij} = \alpha + \gamma f_{ij} + \Gamma \cdot D_j + u_{ij}. \tag{7}$$

This is a standard Instrumental Variables setup, and both parameters γ and Γ are identified. They can be obtained by an IV estimation of equation (7), using D_j and L_{ij} as instruments. These estimates are presented in column (1) in Table III. The results show, as we expected from Section IV, that the direct effect of fair attendance is zero while the social effect of being in a treated department is positive (and significant after 4.5 months). Being in a treated department increases the probability of enrollment by 1.8 and 1.2 percentage points (after 4.5 and 11 months, respectively). Under this set of assumptions, all the effects of the experiment are channeled indirectly through the social effect.

If we assume away social network effects, the parameter Γ is equal to zero, and equation (5) then reduces to

$$y_{ij} = \alpha + \gamma_i f_{ij} + u_{ij}. \tag{8}$$

If we assume first that there are no motivational reward effects ($\nu = 0$), then an IV regression of equation (8) using L_{ij} as an instrument for f_{ij} for the subsample of treated departments ($D_j = 1$) provides an estimate of the average treatment effect of financial incentive compliers, $E[\gamma_i | f_{ij}(11) - f_{ij}(10) = 1]$.[17] The estimates are reported in column (2) of Table III. As we expected, the average treatment effect for financial incentives compliers is zero and not significant. Since it is reasonable to assume that the fair does not have a *negative* effect on any individual's participation

17. Note that the presence of social effects would not bias this estimate as the social effect is assumed to be constant within departments in equation (5).

TABLE III
IV ESTIMATES OF FAIR ATTENDANCE AND DEPARTMENT EFFECTS ON TDA ENROLLMENT

	Assuming constant treatment effect	Assuming no social effects: Effect on financial incentive compliers	Assuming no social effects: Effect on social interaction compliers	OLS	Naïve IV
	(1)	(2)	(3)	(4)	(5)
PANEL A: Dependent variable: TDA participation after 4.5 months					
Fair attendance	−0.046	−0.050	0.117	0.016	−0.002
	(.0431)	(.0429)	(.0465)	(.0109)	(.0255)
Treated department	0.018				
	(.0092)				
Observations	5587	3726	3755	1832	5587
PANEL B: Dependent variable: TDA participation after 11 months					
Fair attendance	0.003	0.005	0.131	0.049	0.032
	(.0681)	(.0685)	(.0826)	(.018)	(.0397)
Treated department	0.012				
	(.0147)				
Observations	4879	3246	3271	1608	4879
Sample	Entire sample	Treated departments	No letter only	Letter only	Entire sample

a. Dependent variables are individual enrollment in the TDA 4.5 months and 11 months after the fair.
b. Independent variable are individual fair attendance and department treatment dummy D in column (1).
c. Independent variable is individual fair attendance in columns (2) to (5).
d. All regressions control for the triplet of the department, gender, year of service, age, and salary.
e. Standard errors (in parentheses) are corrected for clustering at the department level.

decision, the very small coefficient in column (2) (even slightly negative after 4.5 months) would imply that the treatment effect is very close to zero for *all* financial reward compliers, which seems unrealistic. This suggests that there was very likely a motivational reward effect associated with receiving the letter.

The average treatment effect for social interaction compliers $E[\gamma_i | f_{ij}(10) - f_{ij}(00) = 1]$ can be obtained by an IV regression of (8) using D_j as an instrument for f_{ij} for the subsample of individuals with no letter ($L_{ij} = 0$). Column (3) in Table III presents these IV estimates, for TDA enrollment 4.5 months and 11 months after the fair. The estimates are positive and significant showing that attending the fair increases the probability of enrolling by 11.7 and 13.1 percentage points after 4.5 and 11 months in this sample. The social interaction compliers are clearly not affected by the motivational reward, but may be subject to peer effects. Therefore, the IV estimates is an upper bound of the direct effect of the fair. These effects are of comparable size (slightly higher) than those estimated by Madrian and Shea [2002] in a nonexperimental setup. Therefore, the IV estimates suggest a positive treatment effect on social interaction compliers, and no effect on financial reward compliers. This differential treatment effect is plausible. Those who attend because of the reward may be less interested in the fair than those who decide to attend because of their colleagues.

If we assume that there are motivational reward effects, then the estimates in column (2) give the average treatment effect for financial reward compliers less the motivational reward effect. We cannot obtain estimates of the motivational reward effects unless we assume that, in the absence of motivational reward effects, the treatment effect would be constant for both groups of compliers. In that case, all the difference between columns (3) and (2) can be attributed to motivational reward effects. Under these assumptions, straightforward computations (see Duflo and Saez [2000b]) show that receiving the letter reduces the treatment effect of the fair by 63 percent for TDA participation after 4.5 months, and 41 percent for TDA participation after 11 months.

It is useful to compare the effects of fair attendance on TDA enrollment of columns (2) and (3) with the OLS effect obtained by regressing TDA enrollment on fair attendance. The OLS estimates are reported in column (4) for the sample of individuals who received the letter.[18] The OLS coefficient after 11 months is positive and significant, and would lead the researcher to conclude that the fair increased participation by 4.9 percentage points for those who attended it. This coefficient, as expected, is biased upward by selection bias.

In column (5) we present the "naive" IV estimate that uses the letter dummy as an instrument, in the complete sample, without taking social effect into account. This estimate lies between the estimates of column (2) and column (3). The naive estimate would underestimate the overall effect of the fair (since part of the "control" group is actually treated) and overestimate

18. That is the only group where we have actual individual fair attendance information. Computing the OLS estimate in the sample of controls would have been more interesting but is unfortunately unfeasible.

the direct effect on those who received the letter. This shows the potential bias in randomized trials that ignores externalities.

The distinction between differential treatment effects, social network effects, and motivational reward effects is clear conceptually but our experiment does not allow us to tell them apart. Thus, it is useful to describe what type of alternative experimental designs would be needed to separate these effects. Differential treatment effects arise in our setting because there is a first stage in our experiment where individuals decide whether or not to attend the fair. As a result, only a self-selected fraction of individuals attends the fair. Motivational reward effects arise because individuals receive a monetary payment for attending the fair.

Social network effects could be identified with the following experiment. Within a subsample of "treated" departments, a subsample of employees would all automatically attend an information session targeted to them only (and not their colleagues). This could be done by making attendance a job requirement for these employees. One could then test whether the TDA participation of nonattendees in treated departments rises relative to that of individuals in untreated departments. Motivational reward effects could be estimated by paying people for attending an information session in a situation where everybody is supposed to attend. For example, in many firms, new hires are often invited to attend information sessions about benefits. In some departments, this information session could be presented as a normal process through which all new employees go. In other departments, attending this information session could be presented as voluntary, but a financial reward could be offered for attendance (large enough to induce virtually everybody to attend). If everybody attends in both cases, the average treatment effect would be expected to be the same in both groups in the absence of a motivational reward effect. Evidence of differential treatment effects could potentially be obtained by using nonmonetary incentives of various intensity to attend the fair. For example, some employees could be sent a letter simply reminding them of the benefits fair. Others could be sent a more pointed letter telling them that important information can be obtained at the fair. One could also use emails, personal phone calls, or even remind them in person to attend the fair. These different encouragement designs are associated with different groups of compliers and may thus allow estimation of differential fair treatment effects.

V.C. Additional Evidence

In order to cast further light on our results, we have divided our sample by size of department, pre-experiment TDA participation rate, gender, salary, and years of service. These results are reported on Table IV. Column (1) reports fair attendance among those who received the letter (we know the identity of those who attended the fair only for this group). Fair participation was larger in small departments than in large departments, and for women than for men. In columns (2) and (3) we show the difference in TDA enrollment between treated and control departments after 4.5 and 11 months, respectively.

Panel A shows that the TDA enrollment effects are about the same in large and in small departments. Panel B shows that effects appear to be stronger in smaller departments than in larger ones after 11 months. Panel C shows that the effects are the same for men and women. After 4.5 months the treatment effect seems somewhat larger in departments where average salaries are high (Panel D), or for employees with more years of service (Panel E). However, after 11 months, this difference is actually reversed (in Panels D and E). This suggests that it takes more time for those in departments with lower salaries or seniority to adjust their TDA participation. Overall, there is no evidence that treatment effects are widely different across groups defined by observables after eleven months. However, most of the coefficients in Table IV are imprecisely estimated, and caution should be taken in the interpretation of results.

As mentioned above, following our experiment, we sent out a follow-up questionnaire to 917 employees designed to measure the employees' knowledge of the retirement benefits system in the university, as well as questions to elicit alternative retirement savings options available to employees and to measure the extent of procrastination. All the results are described in detail in Duflo and Saez [2002b]. Interestingly, we found that satisfaction with the fair was significantly higher for group 11 than for group 10. This is consistent both with differential treatment effects and motivational reward effects. The questionnaire results also show that individuals in group 11 do not seem to have a better knowledge of retirement benefits than those in group 10, even though they are less likely to think that they suffer from a lack of information.

TABLE IV
FAIR ATTENDANCE AND TREATMENT EFFECT IN DIFFERENT GROUPS

	Fair attendance among letter recipients (L = 1)	Difference group D = 1 − group D = 0: TDA participation after 4.5 months	Difference group D = 1 − group D = 0: TDA participation after 11 months
	(1)	(2)	(3)
PANEL A: DEPARTMENT SIZE			
Below median (81)	0.328	0.008	0.007
	(.015)	(.007)	(.0104)
Observations	985	2797	2403
Above median (81)	0.235	0.007	0.012
	(.0132)	(.0047)	(.0087)
Observations	1035	2790	2476
PANEL B: DEPARTMENT AVERAGE PARTICIPATION IN THE TDA BEFORE THE EXPERIMENT			
Below median (34%)	0.259	0.009	0.018
	(.0134)	(.0064)	(.0098)
Observations	1062	2929	2523
Above median (34%)	0.304	0.010	0.008
	(.0149)	(.0063)	(.0089)
Observations	958	2658	2356
PANEL C: GENDER			
Women	0.320	0.011	0.012
	(.0134)	(.0072)	(.0117)
Observations	1213	3298	2843
Men	0.221	0.008	0.010
	(.0146)	(.007)	(.0085)
Observations	807	2289	2036
PANEL D: SALARY			
Below median ($34,021)	0.269	0.003	0.018
	(.0141)	(.0065)	(.0093)
Observations	983	2745	2291
Above median ($34,021)	0.291	0.015	0.010
	(.0141)	(.0063)	(.0104)
Observations	1037	2842	2588
PANEL E: YEARS OF SERVICE			
Below median (2.84 years)	0.312	0.005	0.015
	(.0145)	(.0071)	(.0115)
Observations	1027	2706	2196
Above median (2.84 years)	0.248	0.013	0.010
	(.0137)	(.0054)	(.0083)
Observations	993	2881	2683

a. The sample in column (1) is composed of individuals in group 11 only.

b. In columns (2) and (3), dependent variables are TDA enrollment 4.5 months and 11 months after the fair. Independent variable is department treatment dummy D.

c. Regressions in columns (2) and (3) control for the triplet of the department, gender, year of service, age, and salary.

d. Standard errors (reported in parentheses below the coefficient) are corrected for clustering at the department level.

VI. Conclusion

Small financial incentives have successfully induced treated employees, as well as members of their peer groups, to attend a benefits fair. Moreover, individuals affected by the experiment, whether directly or indirectly, are more likely to enroll in the TDA after the fair. Interestingly, the direct effect is no larger than the indirect effect: in treated departments, those who received the letter and those who did not are about as likely to subsequently enroll in the TDA. We proposed three different interpretations of this finding: differential treatment effects, social network effects, and motivational reward effects. Our experiment does not allow us to unambiguously distinguish these interpretations, which illustrate how the analysis of a simple experiment in a social and economic context may be substantially more complicated than expected.

These three different explanations, however, have a common feature. They suggest that an individual's decision to participate in the TDA is affected by small changes in the environment, and not only by the information content of the fair. The strong social effects obtained in the fair attendance decision are not of primary economic interest per se. However, they are important in an indirect way, as they lead to significant changes in the decision to enroll in the TDA, which is a very important economic decision. Thus, social network effects definitely caused some people to take steps which ultimately led them to change their TDA participation decision.

The increase in TDA contribution generated by this experiment was much larger than its costs. Our program increased participation after one year by about 1.25 percentage points for the 4000 nonenrolled employees in treated departments relative to control departments. Hence our experiment induced 50 extra employees to start contributing to the TDA. Assuming that such employees contributed about $3500 per year (the average contribution of newly enrolled employees),[19] the extra TDA savings generated by our experiment is about $175,000 per year. If the treatment effect persists for many years, the total extra TDA savings could be many times that amount. Therefore, the extra

19. After eleven months the average yearly contribution of new contributors is $3500, and that figure is almost identical across groups 11, 10, and 00. New contributors also contribute $3500 on average after 4.5 months suggesting that employees rarely update their contribution levels after they enroll in the plan.

savings obtained is without doubt very large relative to the inducement cost (the rewards distributed amounted to about \$12,000).[20]

However, these effects remain very small compared with changing default enrollment rules [Madrian and Shea 2001] or offering delayed enrollment, as in the "Save More Tomorrow" program [Thaler and Benartzi 2001]. The large effect of a small reward on fair attendance, amplified by social effects, also suggests that individuals do not optimally seek out and process available information on their own. This suggests that individuals may not be giving much thought to their retirement savings decisions. This has extremely important policy implications for the optimal design of retirement plans.

Appendix

October 31, 2000

Name
Line 1
Line 2
City state zip

Dear Name:

You have just received your Open Enrollment packet from the Benefits Services Group, inviting you to the Benefits Fair 2001.

The Fair will be held in two locations:

November 7, 11am—2:30pm
ADDRESS ERASED

November 8, 11am—2:30pm
ADDRESS ERASED

This year, as part of a study (conducted jointly by the Benefits Services Group and economics researchers) to better understand the impact of the Fair on benefits choices, we are offering a reward of **\$20** to 2,000 employees, just for attending the Fair. Funding for these rewards was contributed from a research grant. We selected those employees by a simple lottery, and your name was among those drawn.

In order to receive this **\$20** reward, all you have to do is to

20. Moreover, this does not take into account potential effects on other benefits decisions, which are likely to be impacted by the fair as well. Unfortunately, we do not have access to the data on other benefits to study these other effects.

come to the Fair with this letter, and give your name at the registration table that will be located in the main hall. You will receive a check within the two weeks following the Fair.

We hope that you will find the Fair helpful in making your benefits choices. However, we want to emphasize that the reward is completely independent of your benefits decisions.

Make a note of these dates (November 7 or November 8) in your calendar, and we look forward to seeing you there.

Sincerely yours,
Name of the Benefits Office
Associate Director

MASSACHUSETTS INSTITUTE OF TECHNOLOGY AND NBER
UNIVERSITY OF CALIFORNIA AT BERKELEY AND NBER

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