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# Long term savings decisions: Financial reform, peer effects and ethnicity<sup>☆</sup>

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## ABSTRACT

In 2005, a drastic reform in the Israeli capital market shifted the power to choose savings vehicles from employers to individuals. Using a unique dataset from a large employer, this event provides us a rare window into individuals' savings decisions and the effect of their social environment. In the first year following the reform's implementation, 7% of the employees switched out of the fund in which they all previously saved. Choice of fund was not associated with observable measures of fund performance, but was strongly affected by the employees' social environment. Exploiting within-department variation in peer groups, we find that savings decisions were strongly influenced by the choices of co-workers from the same ethnic group. Interviews also point to the influence of non-professional colleagues.

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## 1. Introduction

Imagine a country in which long term savings instruments are provided solely through the employer. Then a reform is enacted and the choice is given to the individual. How will individuals respond? Which financial instruments will they choose? Will they improve on the choices made by the employer? How will they decide? These are not trivial questions. Population aging and the financial crisis have raised concerns regarding the process by which individuals make savings decisions and allocate their savings among different investment vehicles. Governments around the world are implementing various reforms aimed at improving the quality of long-term savings decisions, promoting competition in the financial sector and addressing fiscal imbalances. According to [Holzmann \(2012\)](#), between 1988 and 2008 twenty-nine countries introduced

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pension system reforms. An important input to the design of such reforms is a better understanding of the consequences of transferring greater responsibility to the individual.

This paper studies decisions following a substantial regulatory change in Israel. Prior to the reform, employees were required to contribute to a default saving plan (provident fund) chosen by the employer. Following the reform, employees were given a choice from over two hundred different provident funds. We use a unique proprietary dataset from a large employer in Israel, which contains detailed information about employees' savings decisions following the reform. This offers a rare window into these decisions. How many employees actually switch out of the default? Who are the early switchers? Which funds do they choose? Are these funds distinguishable from other funds on observable measures? Furthermore, the richness of the data also allows us to investigate how the social environment of employees – and in particular the choices of coworkers – affect their decisions and the outcome of these decisions.<sup>1</sup>

Four features of the data are particularly important. The first is that we observe behavior immediately following a shock to the regulatory environment which affected all employees. The reform was substantial and highly publicized throughout the country: employees were exogenously moved from a no-choice environment to one in which they could choose which fund to save in. As detailed below, the reform affected only the choice of fund and not the amount of savings invested in the fund (which is effectively determined by fixed contributions by the employee and employer). This helps us to isolate an important dimension of savings decisions, namely the choice of the fund provider. It also provides a natural starting point for examining peer effects in the choice of savings vehicles.

A second feature of our dataset is that it contains information on the timing of the decision. Thus, beyond examining the association between choices made simultaneously by different employees – where it may be hard to know who affected whom – we can focus on the association of decisions by peers in an early period with decisions made in a later period. Third, we know not only whether the employee switched out of the default, but also the particular fund chosen. This allows us to investigate whether observable fund characteristics, emphasized in the finance literature, play an important role in savers' decisions. It also helps address some of the challenges involved in identifying peer effects.

Finally, the dataset is rich in terms of information about employees. We know the department in which the decision-makers are employed; several department characteristics (location, size); and employees' personal characteristics (education, family status, etc.). Importantly, we also know employees' detailed ethnic background. That is, we know the country of birth of each employee's paternal grandfather (most Israeli Jews either immigrated themselves or are descendants of people who immigrated to Israel during the past century, hence, paternal grandfather can serve as a proxy for ethnicity). While ethnicity is often unobserved to outsiders (and, importantly, cannot be used by fund providers to target specific groups), it frequently affects an Israeli individual's social circle. Maman (1991) documents that in Israel ethnicity plays an important role in one's social network and hence may serve as a good proxy for communication channels.<sup>2</sup> We can thus examine the association of one's choices not only with the choices of other members of the department in which she is employed, but also with the choices of co-ethnics within that department.

The proprietary data obtained from the employer are augmented by data from the Israeli Ministry of Finance on management fees and net inflows for each provident fund that operated in Israel during the period of investigation. This enables us to compare some of our results to the provident fund industry's total activity during the period we investigated. Specifically, we can check whether the net inflow patterns, resulting from the switch made by savers in the organization we studied, are consistent with the general trend in the provident fund industry.

Given the existing evidence in behavioral finance, the first natural question to investigate following such reform concerns inertia.<sup>3</sup> Indeed, we find that despite the drastic and well-publicized reform, 93% of employees did not switch funds and stayed with the default. However, unlike a common emphasis in the literature on the apparent irrationality of such behavior, in our case staying with the default does not represent an obviously inferior decision. The default fund offered low management fees at the time of the investigation, and in retrospect performed better than most of the funds that employees switched into. We thus focus our attention on the factors that affect the decision to switch out of the default fund.

A first interesting finding is that by far the most popular fund chosen by those employees who switched did not stand out in terms of performance (returns, Sharpe Ratios), transaction costs or services as compared to hundreds of other available funds. The popularity of this fund – which we will call “Fund X” – does not seem to be a peculiar feature of our data. The net inflow of this particular fund in 2007 was close to eighteen per cent of the provident funds management industry (Israeli

<sup>1</sup> Our analysis of the last point builds on the extensive literature on peer effects. Peer effects have been studied in several contexts, including student outcomes and choice of major in college (Sacerdote, 2001; Cipollone and Rosolia, 2007; Ammermueller and Pischke, 2009; De Giorgi et al., 2010; Lavy and Schlosser, 2011; Lavy et al., 2012); health plan choice (Sorensen, 2006); criminal behavior (Bayer et al., 2009); unethical practices (Gould and Kaplan, 2011); mutual funds proxy voting (Matvos and Ostrovsky, 2010) and stock market investment decisions (Hong et al., 2004; Brown et al., 2008). With respect to long term savings, Duflo and Saez (2003), Beshears et al. (2011) and Kast et al. (2012) study how peers influence the magnitude of savings and Chalmers et al. (2012) and Brown and Laschever (2012), document that peer effects are an important determinant of individual retirement dates. A well-known challenge in this literature is the fact that a correlation in behavior (e.g. savings decisions) within peer groups cannot automatically be attributed to a direct influence of group members on one another (Manski, 1993). For example, workers in the same department face the same organizational environment and may respond to common shocks. Further, workers in a given department may share similar unobserved characteristics which lead to similar choices. The richness of our data allows us to plausibly address such concerns.

<sup>2</sup> According to Maman 77% of the “social network” of those whose family origins are from Asia, Africa or the Middle East, are from that same group. 68% of the social network of those whose family origins are European, are from the same origin.

<sup>3</sup> See e.g., Madrian and Shea (2001) on inertia in 401(k) savings behavior.

Ministry of Finance data).<sup>4</sup> It is also interesting that the choice to switch out of the default does not seem to be associated with either education in general or education in economics.

We find a substantial association between an employee's choice of fund and the decisions previously made by peers in her department. This association may be due to any combination of common unobserved attributes of employees in a given department; various department-level shocks (e.g. ongoing marketing campaigns, seminars, educational efforts); and peer effects. However, we find that even within a given department, employees are more likely to choose a particular fund the higher the proportion of co-ethnics from that department who previously chose that fund. That is, above and beyond any general tendencies in some departments to choose certain funds (due to marketing, selection, etc.) an individual's choice is strongly affected by peers from her ethnic group. This relates to the literature studying variation of savings behavior across ethnic groups.<sup>5</sup> Our contribution to this literature is in further examining how saving choices spread within ethnic groups.

To complement our empirical investigation we also conduct a set of structured interviews with a sample (77 employees) of our investigated population. While employees say that choosing a savings fund is an important decision – and point to funds' returns as the important criterion in choosing funds – their actual knowledge of their fund's rate of return is rather poor. Further, when asked whom they consulted before deciding to switch to their current fund, more than half of the interviewees who opted out of the default mention the recommendation of co-workers. The responses are consistent with our main findings: these savings decisions do not appear to be strongly grounded in observable fund performance and appear to be strongly influenced by (non-professional) peers.

It is important to note that the tendency to switch to funds with high management fees does not appear to be unique to the particular organization and funds we study. During 2007, the overall net inflow to Fund X (high management fees) was positive and the net inflow to the default fund (low management fees) was negative. Indeed, data from the Israeli Ministry of Finance indicate a similar general tendency to switch to high management fees funds during the year 2007.<sup>6</sup> It is perhaps also worth noting that one of the reform's declared aims was to increase competition in the provident funds industry in Israel. Yet, the average management fees in the industry actually increased following the reform from 0.52% and 0.59% in 2005 and 2006 respectively to 0.77% and 0.82% in 2008 and 2009 respectively.

The paper is organized as follows. Section 2 provides institutional details on the market structure and a brief introduction to ethnicity in Israel. Section 3 describes the data and Section 4 examines the performance of the funds chosen by the employees. Section 5 discusses our strategy for identifying peer effects and Section 6 presents the results. Section 7 presents qualitative results from interviews. Section 8 concludes.

## 2. The setting

This section describes the institutional and social setting in which our empirical investigation takes place. We first describe the financial reform, then turn to the structure of the long term saving plan under investigation (provident funds) and finally we briefly discuss ethnicity in Israel.

### 2.1. The 2005 reform in financial markets in Israel

Prior to the reform, three banks dominated the financial intermediation industry in Israel in general and the provident funds industry in particular.<sup>7</sup> Despite being regulated by the Bank of Israel and the Israel Securities Authority, the market was characterized by potential conflicts of interests and excessive concentration. Hence, a series of drastic regulatory changes, also known as the “Bachar Committee Reform”, were implemented. The intent was to boost competition in the banking sector, as well as to address conflicts of interest among financial intermediaries.

The Bachar Committee stated that investors should be free to select their provident funds as they see fit, unbound by employer recommendations or interests. Moreover, the reform forced banks to divest of their provident funds.<sup>8</sup> As a result, the reform encouraged investment houses to offer the public various provident funds that varied in their risk exposure and investment policies. Indeed, the number of provident funds offered in Israel grew from 168 in 2005 to 226 in 2008.

<sup>4</sup> One particular aspect that characterizes this fund (but not only this fund) is that it is managed by an independent investment bank specializing in investment unlike many other funds that are managed by institutions also involved with other businesses such as insurance.

<sup>5</sup> For instance, African Americans accumulate less financial wealth (Altonji et al., 2000), as well as less human capital (Neal and Johnson, 1996; Fryer et al., 2004), and are less likely to invest in the stock market (Hurst et al., 1998).

<sup>6</sup> A simple way to see this is to divide the entire provident funds population into funds that charged management fees above the median and to those that charged management fees below the median in 2007. 46% of the funds that were below median had positive net inflows while 83% of the funds above the median had positive net inflows. It is thus not surprising that we find a positive and significant correlation between net inflow and management fees during 2007: funds with higher management fees received higher net inflows.

<sup>7</sup> Structural reform in the capital market, inter-ministerial committee report. The Israeli Ministry of Finance, 2004. [http://ozar.mof.gov.il/bachar/pdf/bachar\\_English.pdf](http://ozar.mof.gov.il/bachar/pdf/bachar_English.pdf).

<sup>8</sup> Alternative solutions, such as ownerships sale and retention of management, or management outsourcing while retaining ownerships, were deemed insufficient to eliminate the potential conflict of interest. The committee also included recommendations about underwriting, marketing, advisory and adviser's compensation that have been implemented gradually over time.

The reform was enacted into law in 2005 and implemented during the ensuing years. By the end of 2006, employees in our organization could switch to non-default funds, and hence we focus on the period January–December 2007.<sup>9</sup>

## 2.2. Provident funds

A traditional provident savings plan in Israel consists of two components: an employer contribution of up to 7.5% of the employee's salary into this plan, and an employee contribution of up to 2.5%. In most workplaces the plan can be liquidated tax free at any time after six years from the initial deposit. In fact, not all employees withdraw their savings when the funds become liquid. At the end of 2007, 53% of the savings held in provident funds were liquid (Israel Ministry of Finance aggregate data). There is usually a one year waiting period for eligibility to the provident plan benefit. In addition, there exist tax advantages to investing in provident funds up to a certain threshold. In practice, this means that the amount of savings is determined by the employer and not the employees (who almost always participate when eligible).

Following the financial reform described above, employees could choose between dozens of different vendors who offer provident funds. A vendor may provide more than one fund, and the funds can differ by their investment strategy. These funds may include money-market funds, various bonds and stocks (both Israeli and foreign). In our case, employees who take no action are automatically enrolled in a default fund (the same fund all employees were enrolled in up to 2006 before the reform).<sup>10</sup>

## 2.3. Ethnicity in Israel

Israel is a country of immigrants. Israeli Jews are often grouped into two major groups: “Sephardic” whose ancestors come mostly from North Africa (e.g. Tunisia, Morocco) and Asia (e.g. Iran, Iraq); and “Ashkenazi” whose ancestors come mostly from Europe and North America.<sup>11</sup> Studies consistently show significant differences in social and economic performance across these groups.<sup>12</sup> They also document that the different ethnic groups in Israel are associated with different stereotypes that can affect economic activity (see [Freshtman and Gneezy, 2001](#) for discussion). However, these groupings mask significant heterogeneity. For example, the wage gap between Asian Sephardic and Ashkenazi Jews is narrower than the gap between North-African Sephardic and Ashkenazi Jews ([Cohen, 2002](#)). In most of our empirical investigation, we use a finer classification of the Jewish ethnic groups (in addition to the non-Jewish group) according to the paternal grandfather's country of birth. As most Israeli Jews either immigrated themselves or are descendants of people who immigrated to Israel during the past century, paternal grandfather's country of birth serves as a good proxy for ethnicity.

## 3. Data

Our main source of data is a proprietary dataset from a large employer in Israel. The employer provides benefits to its employees through a traditional provident savings plan, which offers the same terms for all workers. The data cover 10,723 employees in 103 separate departments, who continued to work in the same department for the entire period between January 2007 and December 2007.<sup>13</sup> The median department has 49 employees, but there are thirteen departments with over 200 employees. In most of the analysis below we exclude five departments (seven employees) with less than three employees in a department.

In addition to an employee's department, the dataset contains a rich set of demographic variables including age, gender, marital status, number of children, place of residence, years of education, type of institution granting highest diploma (university, college or other), academic profession, whether the employee is tenured and department location (urban or remote).<sup>14</sup> As emphasized above, a unique feature of the dataset is that it includes information on employees' ethnicity based on paternal grandfather's country of birth. Of the 10,723 employees 24.4% are Jews originating from North African origin; 23.3% from other Middle Eastern countries; 14.2% from the Former Soviet Union; and 22.2% from Europe and North America. The rest include non-Jews, and Jews originating from Sub-Saharan Africa and Latin America.

In addition to the administrative data, we obtained additional information by conducting face to face interviews with a subsample of 77 employees. The sampling method and questionnaire are detailed in Section 7.

To compare our analysis with the general activity in the provident fund industry, we also obtained data from the Israeli Ministry of Finance about the net inflow and management fees of each of the provident funds operating in 2007. Additional

<sup>9</sup> Three employees in our data switched out of the default already in late 2006. They are included among the early switchers.

<sup>10</sup> Some procedural details: in order to opt out, an employee had to contact the specific provident fund vendor directly. After this initial contact the vendor had to send the employee forms to be filled out and submitted to the chosen fund and to the employee's Human Resources department (in person or by fax). Overall, the opting out process was time consuming and could not be completed by one phone conversation or on the internet.

<sup>11</sup> “Sepharad” means Spain or Iberia in mediaeval Hebrew while “Ashkenaz” means Germany.

<sup>12</sup> As [Rubinstein and Brenner \(2010\)](#) recently put it, “By the late 1990s, half a century after the establishment of the state of Israel, the Sephardic–Ashkenazi wage gaps had become as large as the black–white wage gaps in the United States”.

<sup>13</sup> Given the sensitivity of the information, we were only able to obtain personnel data for 2007. This is however an appealing period to study the effects of the reform as it immediately follows the reform's implementation and precedes the 2008 financial crisis.

<sup>14</sup> While these variables are used in our regression analysis, we are not permitted to disclose any detailed descriptive statistics.

**Table 1**  
Summary of fund choices.

	Period 1, 1–8/07	Period 2, 9–12/07	Two periods pooled	Mean of within-department proportions; two periods pooled
Opting out of the default	280 [0.026]	457 [0.043]	737 [0.069]	0.045 {0.056}
Enrollment into Fund X	161 [0.015] (57%)	312 [0.029] (68%)	473 [0.044] (64%)	0.026 {0.047}
Enrollment into vendor 5 funds	11 [0.001] (4%)	30 [0.003] (7%)	41 [0.004] (6%)	0.003 {0.012}
Enrollment into vendor 6 funds	28 [0.003] (10%)	9 [0.001] (2%)	37 [0.003] (5%)	0.001 {0.004}
Enrollment into vendor 7 funds	46 [0.004] (16%)	27 [0.003] (6%)	73 [0.007] (10%)	0.006 {0.015}
Enrollment into vendor 8 funds	18 [0.002] (6%)	6 [0.001] (1%)	24 [0.002] (3%)	0.003 {0.013}
Enrollment into other funds	16 [0.002] (6%)	73 [0.007] (16%)	89 [0.008] (12%)	0.006 {0.018}

*Note:* The first three columns show the number of employees who opt out of the default by the end of each period and their proportion in the population [in brackets]. The first row shows the total number of switchers and the other rows show their distribution across funds, with percentage out of the total number of switchers (in parentheses). The last column shows the mean across departments of the within-department proportion of employees who opted out or chose a particular fund, with standard deviation {in braces}.

information is gathered from the government portal “Gemel-Net” which contains comparable information about the funds’ performance measures (returns, management fees and Sharpe Ratios). The “Gemel-Net” information is publically available.

#### 4. Which funds did employees choose?

Table 1 shows summary statistics on individual fund choices made during 2007 following the implementation of the reform. The first row shows all non-default choices. We split the data into two periods so that as close as possible to 50% of the switchers move in each period.<sup>15</sup> During the first eight months of 2007 (period 1), 2.6% of workers switched out of the default, and during the remaining four months (period 2) another 4.3% switched, for a total of 6.9% of employees who switched during the entire year.<sup>16</sup> Some switching out of the default occurred in 65 departments (out of the 103 departments in our sample). The employees who did switch funds following the reform, opted for 58 different non-default funds out of more than two hundred possibilities. We concentrate on the five largest vendors, who attracted over 88% of those employees who opted out of the default fund. We denote these vendors as vendors X, 5, 6, 7, and 8. Vendor X offered only one fund, which we denote “Fund X”.

In itself, the relatively small proportion of non-default choices is perhaps not surprising. It is consistent with an effect known in the literature as “inertia”: for various reasons, people choose to stay in their current situation.<sup>17</sup> However, in our case (and possibly in many typical situations) this observed inertia does not necessarily represent an inferior choice. The default fund charged significantly lower fees than the typical industry rates (less than 0.3% compared to typical fees of between 1% and 1.5% in funds that were available to switchers).<sup>18</sup> Further, examining the overall performance of the funds for 12/2006–12/2010 measured by their Sharpe Ratio, the default fund turned out to have performed somewhat better than the other funds shown in Table 1.

As can be seen from the table, Fund X is by far the most popular fund among switchers, and was chosen by 64% of the switchers. This tendency to choose Fund X is not peculiar to a few isolated departments, and occurred in 44 departments (in

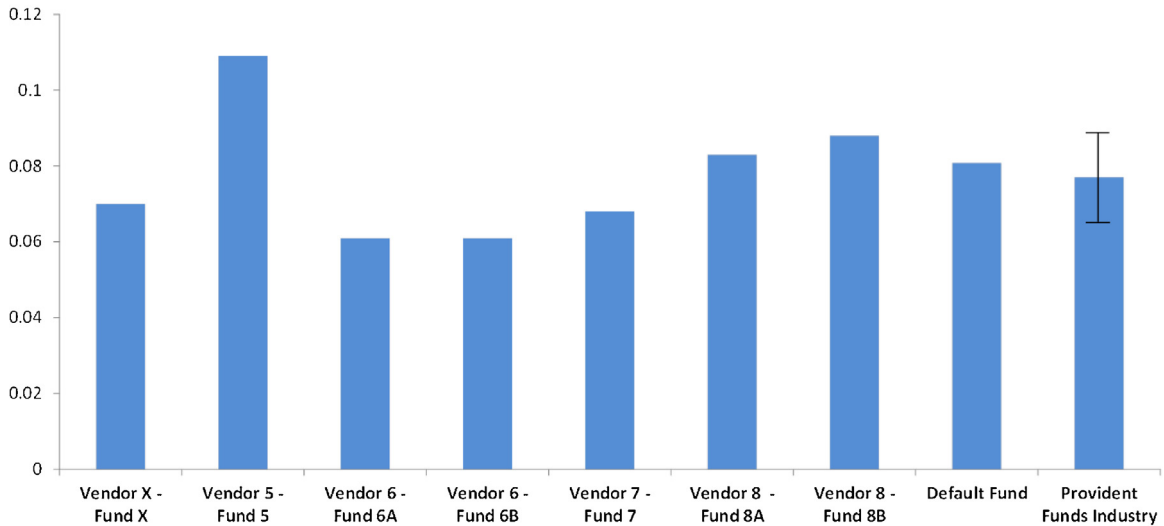
<sup>15</sup> A relatively large number of employees switched during September 2007. Since we only have monthly data this necessarily leads to an unequal number of switchers in the two periods.

<sup>16</sup> This number is relatively low compared with the documentation in [Palme et al. \(2007\)](#) and [Massa et al. \(2006\)](#), regarding the 2000 reform in Sweden. However, there are notable differences between the relevant market micro-structure and the choice process in the Israeli reform compared to the Swedish reform.

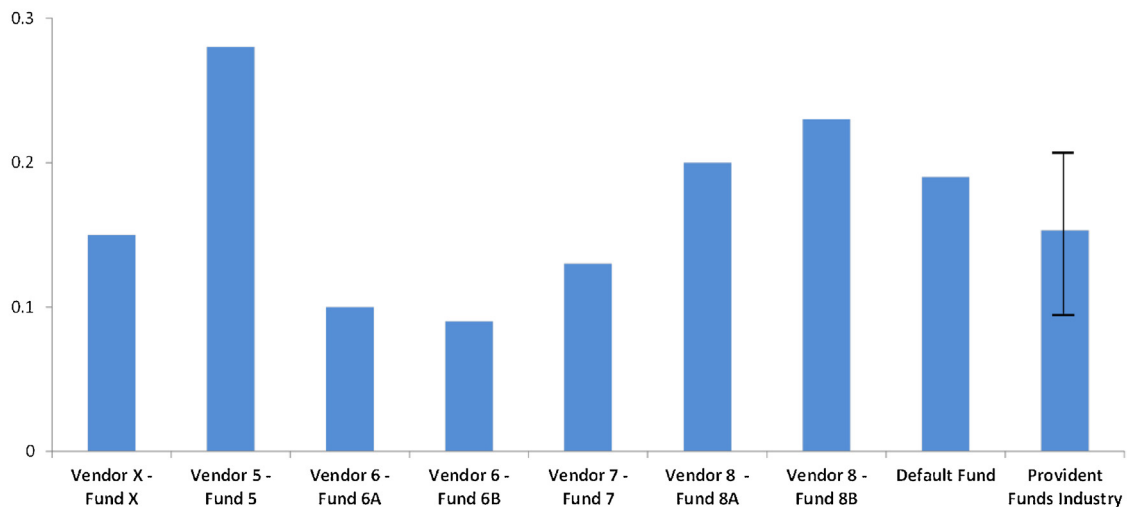
<sup>17</sup> There is a growing academic literature that investigates inertia and savings’ automatic enrolment. For example, [Madrian and Shea \(2001\)](#) document that the 401k participation rate of the cohort at 3–15 months of tenure whose default choice was not to enroll was 37%, which is less than half the 86% participation among the new cohort whose default was to enroll. In [Choi et al. \(2004\)](#) 35% of self reported under-savers express an intention to increase their savings rate in the next few months, but 86% of these well-intended savers have made no changes to their plan months later.

<sup>18</sup> A survey we conducted among a sample of the employees (discussed in greater detail in Section 7) indicates that 35% of the employees consider fund’s management fees in their fund choices.

### Panel A: Rate of Returns



### Panel B: Sharpe Ratios



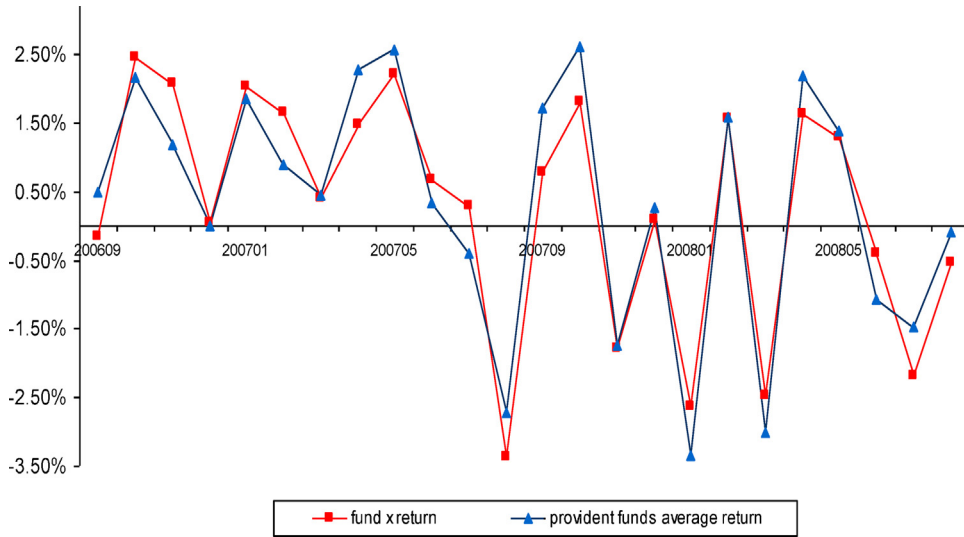
**Fig. 1.** Performance of the funds chosen by employees, 2007. (Means, yearly data. Capped ranges indicate 95% confidence intervals.) This figure shows the rates of return and Sharpe Ratios of provident funds chosen by most employees who opted out of the default fund. The data were publicly available at the Israeli Ministry of Finance website (GemelNet). The returns were calculated by GemelNet for the period 12/2006–12/2007. The Provident Funds Industry category shows the simple mean (and 95% confidence interval) across all provident funds that existed the entire year and whose performance is available from GemelNet (200 provident funds).

33 departments during period 1). We next examine the performance of this fund relative to that of other provident funds in the industry.

Fig. 1 compares the performance of the main funds chosen to the default fund and the mean performance among provident funds in 2007, using publicly available data provided by the Israel Ministry of Finance.<sup>19</sup> Panel A shows rates of returns and panel B shows Sharpe Ratios.<sup>20</sup> In both figures, the rightmost point shows the average performance in the industry together with the 95% confidence interval. As the figure plainly shows, Fund X did not show outstanding financial performance

<sup>19</sup> For related evidence that investors have a tendency to use past performance as an indicator of future performance, see for example Daniel et al. (2002).

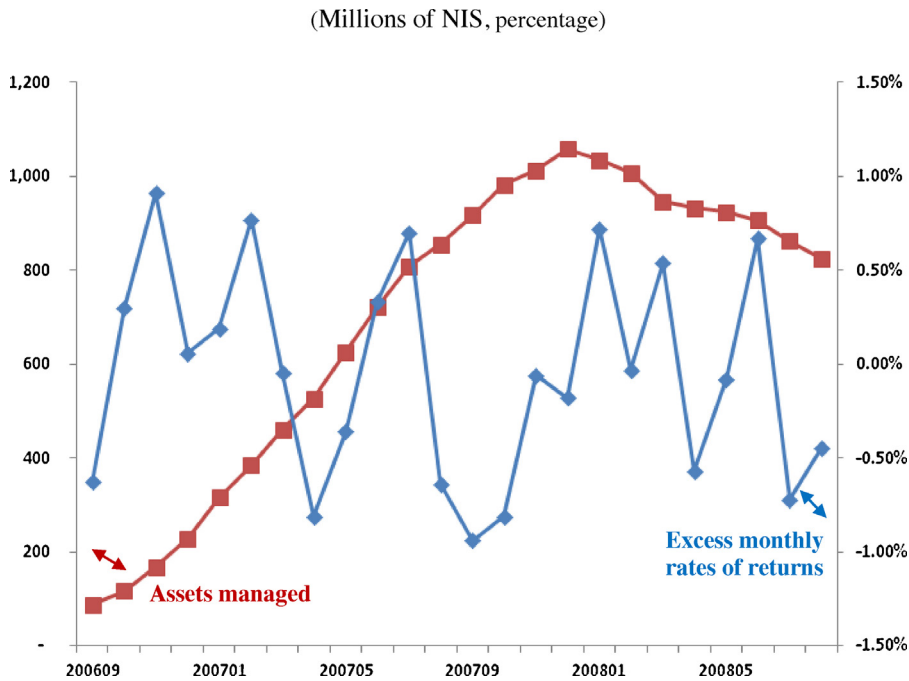
<sup>20</sup> Data on manager's Alpha were not publicly available for the investigated period.



**Fig. 2.** Fund X and the average provident funds returns, 9/2006–8/2008. (Monthly returns, percent.) This graph illustrates the monthly rates of returns for the Fund X and the average return of the Provident Funds Industry during the period 9/2006–8/2008. The data was obtained from GemelNet (Israeli Ministry of Finance website). The average Provident Funds Industry variable was calculated by GemelNet as a simple mean of the total available population of provident funds during each particular month. The Y axis represents the return value in the percentage for the given calendar month.

compared to the provident funds' industry, nor did most other funds that were chosen by switchers (except perhaps vendor 5, which attracted 6% of the switchers).

Fig. 2 compares monthly returns of Fund X to the average across funds in the industry. Again, Fund X is not exceptional: its monthly returns are both similar and highly correlated with those of the industry. Fig. 3 shows the accumulated assets managed by Fund X and its excess monthly rates of returns.



**Fig. 3.** Assets managed by Fund X versus its excess monthly rates of returns, 9/2006–8/2008. (Millions of NIS, percentage.) This graph illustrates two variables. The first variable is the Fund X excess monthly rates of returns, calculated as a simple difference between Fund X monthly returns in percentage and average Provident Funds Industry monthly returns in percentage, during the period 9/2006–8/2008. The right Y axis represents this difference for the given calendar month. The second variable is assets managed by Fund X. This variable is calculated as cumulative amount of assets managed by Fund X at the end of calendar month (represented at axis X), during the period 9/2006–8/2008. The left Y axis represents this value in millions of NIS for the given month. The graph does not suggest that the change of assets managed by Fund X could be explained by excess monthly rates of returns.

Overall, publicly available information does not indicate any meaningful difference in the performance of Fund X relative to other funds. This seems to suggest that past fund performance was not the key factor in employees' decisions to join this fund. Beyond performance, Fund X also charged similar fees as the typical provident funds during this period (between 1% and 1.5%).<sup>21</sup> As it turned out, the performance of the fund during the period 2006–2010, at least as measured by the Sharpe Ratio, was also not exceptional: some funds did worse (in particular vendors 6 and 7) and some did better (vendor 8).

To sum up, observable economic indicators do not seem to be sufficient to explain the switchers' behavior – and in particular their preference for Fund X even though this decision has meaningful financial implications.<sup>22</sup> We next turn to some of the social aspects of this behavior.

## 5. Identification of peer effects

The identification of peer effects poses several challenges, highlighted by Manski (1993) and the ensuing literature (Blume et al., 2011 provide a review). In this section we detail our strategy for addressing these challenges.

Consider estimating a linear equation of the form:

$$y_{ij} = \alpha + \beta y_{j-i} + X_i' \gamma + D_j' \delta + \varepsilon_{ij} \quad (1)$$

where  $y_{ij}$  is the outcome (e.g. choice of a particular fund) for individual  $i$  in department  $j$ ;  $y_{j-i}$  is the mean outcome in department  $j$  excluding individual  $i$ ;  $X_i$  is a vector of individual  $i$ 's characteristics; and  $D_j$  is a vector of department  $j$  characteristics.

Our main interest in this section is in the effect of one's peers on one's savings decision, or, more precisely, on the extent to which employee  $i$  is influenced by the average behavior of other employees in her department. Notice however, that a positive estimate of  $\beta$  in Eq. (1) might not be an accurate measure of such a causal effect. A first issue is the so called *reflection problem*:  $\beta$  represents not only the effect of  $y_{j-i}$  on  $y_{ij}$ , but also the possible effect of  $y_{ij}$  on  $y_{j-i}$ . Second, the relationship between  $i$ 's behavior and her peers' behavior may be due to common unobserved characteristics. It is quite possible that people with similar characteristics either self-select or are selected by the employer into the same departments. Thus, an observed similarity in choices may be due to *selection effects*. Third, a correlation in choices within departments may be due to employees in the same department facing the same institutional environment or being exposed to similar marketing campaigns by various funds. We will call these *correlated effects*.<sup>23</sup> Notice that even if we had random variation in the assignment of employees into departments (which could have helped overcoming selection effects), correlated effects could still be present.

Our identification strategy seeks to address these issues one by one. First, we exploit the fact that we have some information on the timing of the decision. Thus, we can restrict attention to the association of decisions taken by peers in an early period (January–August 2007) with individual decisions in a later period (September–December 2007). Since it is unlikely that the group is affected by an individual's future decision, this plausibly alleviates part of the reflection problem.<sup>24</sup>

A more difficult issue to address when trying to isolate peer effects is the possibility of selection effects. If we examine the decision to stay in the default versus switching out, then selection is a potentially important factor. It seems reasonable to suspect that people who stick to the default tend to have different personal characteristics from people who opt out. Such characteristics may also affect the particular department in which one is employed. We address this concern in two ways. First, we exploit the fact that we have information not only about the decision whether or not to opt out of the default, but also about the choice of a specific fund out of the many that are available to the decision maker. When considering *this* choice, selection effects are less likely to be important. As we have seen in Section 4 above, the fund most often chosen by people opting out of the default (Fund X) is hardly exceptional. It is difficult to think of unobserved personal characteristics leading to the choice of this specific fund over other similar funds. That said, we clearly cannot rule out the possibility that some funds *do* appeal to specific personalities. Our main method of addressing this issue is to rely on variation in peer behavior within departments, keeping constant any general tendencies of people in a given department. This also helps address the possibility of correlated effects.

<sup>21</sup> The effective rate was negotiable to some extent, up to 0.2–0.3% below the official rate.

<sup>22</sup> Under the assumption of no difference in the future returns of the two funds, and simplified conservative assumptions about the timing of the cash flows, we can evaluate the cost associated with switching from the default to Fund X for the average salary employee. If employees save for 42 years without withdrawing they lose the equivalent of 13 months of salary. If they decide to withdraw the money every six years (which is possible) they lose three months of salary. Regardless, this choice is meaningful in monetary terms for the employees in the investigated organization.

<sup>23</sup> Manski (1993) includes in his definition of “correlated effects” both these and the selection effects. For our purposes it is useful to distinguish the two. Manski also discusses the possibility that the characteristics of one's peers have a direct effect on one's decision (these are termed contextual effects). Such effects are not particularly relevant to the present context. In any case, whether or not we control for the average characteristics of one's peers does not significantly alter our estimation results.

<sup>24</sup> We note, however, that this may not fully solve the reflection problem. Some individuals may delay the implementation of their decisions. Further, even if people who move in the late group cannot affect people in the early group, people in the late group could still affect other people in the late group. That is, there could still be peer effects occurring simultaneously for the late group. This means that our estimates of the size of the effect should be interpreted with care. Nonetheless, as we show below the association between late and early decisions is not significantly different from the association when pooling together the entire period (Table 2). This seems to suggest that reflection is not a major concern in our setting.



An association between the choices of specific funds within a department might not indicate peer effects but rather the effects of the common environment faced by employees in a given department. For example, one might worry that different vendors target different departments, leading to an association in the choices of employees within department. Note first that by focusing on the association of decisions taken at different points in time, we reduce the possibility of spurious correlation due to temporary (but not persistent) shocks such as marketing campaigns that affect all employees at a given department. More importantly, we exploit the information we have on employees' ethnicity. Specifically, we examine whether employee  $i$ 's choices are affected by the (preceding) choices of peers from her ethnic group, beyond any effects that operate at the department level.<sup>25</sup> While marketing efforts may well vary by departments, they are highly unlikely to vary by ethnicity within departments.

The idea of comparing correlations in behavior within and across sub-groups to help identify peer effects was developed by [Munshi \(2004\)](#) and employed by [Duflo and Saez \(2002\)](#). A possible advantage of our approach is that it employs a variable – ethnicity – which is not easily observable to outsiders, yet, is plausibly known to insiders. This helps alleviate concerns that vendors may target specific sub-groups according to these characteristics.<sup>26</sup> Of particular importance is the fact that vendors can make special offers to particular groups (e.g. engineers). If vendors also focus their marketing campaigns on specific departments, this can generate within-demographic-and-department correlation in choices. Such special offers cannot be made based on ethnicity.

Formally, the use of ethnic-specific peer groups allows us to use department fixed effects, thereby controlling for unobserved department-specific factors. That is, our estimate of peer effects is identified from variations in the choices of savings funds across ethnic groups *within* departments.

In principle, one might still worry that even an association within department and ethnic group is due to correlated rather than causal effects of the behavior of co-ethnic peers. This can happen if certain funds are for some reason more attractive to some ethnic groups than to others. If different funds target different departments, and vary in their attractiveness to different ethnic groups, then this can generate correlations within departments and ethnic groups. To capture such ethnic-specific attractiveness of funds we also include ethnic fixed effects.

To sum up, our strategy for identifying peer effects consists of estimating an equation of the form:

$$y_{ijet} = \alpha + \beta y_{j,e-i,t-1} + X_i' \gamma + \theta_j + \eta_e + \varepsilon_{ijet} \quad (2)$$

where  $y_{ijet}$  is the choice of a particular fund by individual  $i$  in department  $j$  and ethnicity  $e$  at time  $t$  (the second period);  $y_{j,e-i,t-1}$  is the mean choice by members of ethnic group  $e$  (excluding  $i$ ) in department  $j$  at time  $t-1$  (the first period) i.e.,  $\sum_{k \neq i} y_{kjet-1} / (n_{jet-1} - 1)$ ;  $X_i$  is a vector of individual characteristics,  $\theta_j$  is a department fixed effect; and  $\eta_e$  is an ethnicity fixed effect.  $\varepsilon_{ijet}$  is an error term clustered at the department level.<sup>27</sup> Our identification assumption is that, beyond any differences across departments and across ethnic groups, and once individual observables are controlled for, there are no systematic differences across department-by-ethnicity cells in factors that may affect individual savings choices in both periods. We assess this assumption below.

## 6. Results

We begin with a series of descriptive regressions examining the characteristics of employees who switched out of the default, and the within-department correlation in making this choice ([Table 2](#)). We then look more closely at the choice of particular funds or vendors ([Table 3](#)). Finally, in [Table 4](#) we turn to the estimation of peer effects using Eq. (2). For ease of interpretation and comparison to the existing literature, for the most part we report OLS estimates.<sup>28</sup> In most regressions, we control for the following individual characteristics: age, gender, marital status, number of children, place of residence, years of schooling, type of institution granting highest diploma (university, college or other), academic profession, whether the employee is Jewish or not and whether the employee is tenured. In those regressions that do not include department fixed effects we also control for department location (urban or rural) and size. A description of the variables used is in [Table A1](#).

<sup>25</sup> [Bertrand et al. \(2000\)](#) use geography and ethnicity to investigate peer effects in welfare participation.

<sup>26</sup> In the previous studies, the subgroups were formed using characteristics that can be relatively easily observed by outsiders (gender, tenure, age, position in the organization). In contrast, it is often hard for an outsider to determine a person's ethnic origin (within the Jewish population), and this opacity vis-à-vis outsiders makes it hard to target a sub-group based on ethnic origin.

<sup>27</sup> Since our treatment is at the department level, we allow for correlations at that level, which may be possible even with department fixed effects. Notice however that the clustering problem is not central in our setting since the main explanatory variable of interest  $y_{j,e-i,t-1}$  varies within departments.

<sup>28</sup> For specifications with a binary dependent variable, results are qualitatively similar when using either a linear probability model (estimated by OLS), logit, or probit models. In particular, our main coefficients of interest (capturing the effect of peer choices) have the same sign and similar levels of statistical significance across all three estimation techniques. The precise magnitudes of the estimated marginal effects from probit or logit estimations are, however, sensitive to the point in the distribution at which marginal effects are evaluated. For example,  $y_{j,e-i,t-1}$  from Eq. (2) has a highly asymmetric distribution, with a large mass at zero. It is not clear whether evaluating marginal effects at the mean of this variable is the most informative marginal effect to consider. An additional issue with logit or probit is that certain dummy variables perfectly predict outcomes. Most importantly, many departments have no employees switching to Fund X in the second period. Maximum likelihood estimation of the fixed effects specification is thus not possible without dropping these departments. See [Table 6](#).

**Table 2**  
Opting out of the default.

	Decision to opt out of the default in period 1	Decision to opt out of the default (two periods pooled)		Decision to opt out of the default in period 2	
	(1)	(2)	(3)	(4)	(5)
Share of peers who opted out			0.643 <sup>***</sup> (0.072)		
Share of peers who opted out in period 1				0.980 <sup>***</sup> (0.186)	0.739 <sup>***</sup> (0.210)
Tenured (indicator)	0.043 <sup>***</sup> (0.007)	0.073 <sup>***</sup> (0.010)	0.073 <sup>***</sup> (0.010)		0.033 <sup>***</sup> (0.008)
Age	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)		0.001 (0.001)
Married	-0.002 (0.006)	0.026 <sup>***</sup> (0.008)	0.025 <sup>***</sup> (0.008)		0.027 <sup>***</sup> (0.006)
Education	0.001 (0.002)	0.004 (0.002)	0.001 (0.003)		0.002 (0.002)
Male	0.013 <sup>**</sup> (0.004)	0.026 <sup>**</sup> (0.006)	0.022 <sup>***</sup> (0.007)		0.012 <sup>*</sup> (0.005)
Economist (indicator)	0.016 (0.022)	-0.002 (0.029)	-0.020 (0.027)		-0.022 <sup>*</sup> (0.013)
Engineer (indicator)	0.006 (0.004)	0.015 (0.015)	0.002 (0.013)		0.004 (0.013)
Industrial engineer (indicator)	-0.001 (0.011)	-0.037 <sup>***</sup> (0.013)	-0.029 <sup>**</sup> (0.012)		-0.029 <sup>***</sup> (0.010)
MD (indicator)	0.008 (0.007)	0.016 (0.011)	0.011 (0.011)		0.006 (0.008)
Urban department location (indicator)	0.020 <sup>***</sup> (0.007)	0.017 (0.013)	0.000 (0.007)		-0.021 (0.013)
Jewish (indicator)	0.004 (0.006)	0.028 <sup>***</sup> (0.011)	0.015 (0.011)		0.018 <sup>**</sup> (0.007)
Ethnic origin North Africa	0.016 <sup>**</sup> (0.005)	0.020 <sup>**</sup> (0.010)	0.017 <sup>*</sup> (0.010)		0.003 (0.008)
Ethnic origin Europe	0.014 <sup>*</sup> (0.006)	0.024 <sup>*</sup> (0.011)	0.023 <sup>**</sup> (0.011)		0.011 (0.008)
Ethnic origin Former SU	0.008 <sup>*</sup> (0.004)	0.023 <sup>**</sup> (0.011)	0.016 <sup>*</sup> (0.009)		0.013 (0.009)
Ethnic origin Middle East	0.012 <sup>**</sup> (0.005)	0.021 <sup>*</sup> (0.011)	0.017 (0.011)		0.009 (0.008)
Other individual and department controls	Yes	Yes	Yes	No	Yes
Observations	10,716	10,716	10,716	10,436	10,436
R <sup>2</sup>	0.024	0.055	0.068	0.012	0.045

Note: OLS, standard errors clustered at the department level in parentheses. Dependent variable is an indicator variable for opting out of the default fund. Main explanatory variable is the share of *i*'s department members (excluding *i*) who opted out of the default fund. Columns 4 and 5 exclude employees who have already opted out of the default fund in period 1. Unreported controls include number of children, place of residence, type of institution granting highest diploma (university, college or other) and department size. All regressions exclude departments with less than 3 workers and include 98 departments.

\* Significant at the 10% level.

\*\* Significant at the 5% level.

\*\*\* Significant at the 1% level.

### 6.1. Opting out

Consider first the decision whether or not to opt out of the default. Recall from Table 1 that more than 90% of the employees in our sample did not switch fund and stayed with the default. Table 2 examines the decision to opt out. Column 1 looks at the demographic correlates of what may be called the early switchers – those employees who opted out of the default during the first eight months of 2007.<sup>29</sup> Column 2 looks at switchers during the entire period under study. The patterns are broadly similar. Consistent with previous studies, gender and tenure seem to matter for savings decisions (e.g. Huberman et al., 2007). In particular, we find that males and tenured employees are more likely to opt out. Employees in urban departments,

<sup>29</sup> There is a growing marketing and social network academic literature that suggests several types of “early switchers”. One group is “innovators” or “early adopters” who are intrinsically interested in new products hence willing to adopt them without the need for social approval of others (for a discussion see Rogers, 1995). From a social network perspective, another potential group is “social hubs” (Goldenberg et al., 2009). Social hubs are connected to many people, and therefore get the information early, and, in turn spread it to other individuals. A third early switchers group is called “market mavens”, who deal with monitoring and screening the information in the social system (Feick and Price, 1987). The information that we have is not sufficient to determine which group is dominant among our early switchers. Nonetheless, as we shall see, early adopters appear to influence the decisions of their peers and hence it may be of interest to examine their demographic characteristics. For a review of the three main types of social influence in the marketing literature – word of mouth, signals, and network externalities – see Muller et al. (2009).

**Table 3**  
Choosing savings funds (all funds chosen by more than ten employees in period 1).

	Fund X	Vendor 5	Vendor 6	Vendor 7	Vendor 8	Other funds
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Linear probability model (six independent regressions)</i>						
Share of peers who chose Fund X in period 1	0.801*** (0.206)	0.035 (0.036)	−0.002 (0.008)	0.024 (0.021)	0.000 (0.008)	−0.100 (0.081)
Share of peers who chose vendor 5 in period 1	1.789 (1.898)	0.610 (0.424)	−0.039 (0.052)	−0.039 (0.061)	−0.007 (0.024)	−0.193 (0.287)
Share of peers who chose vendor 6 in period 1	0.730 (0.855)	0.183 (0.201)	0.111 (0.113)	−0.135 (0.132)	−0.015 (0.097)	0.444 (0.514)
Share of peers who chose vendor 7 in period 1	−0.132 (0.497)	0.181 (0.155)	0.065 (0.070)	0.215 (0.126)	−0.035 (0.042)	0.364* (0.194)
Share of peers who chose vendor 8 in period 1	−0.221 (0.148)	−0.042 (0.046)	−0.020 (0.016)	0.025 (0.085)	0.023 (0.028)	−0.092 (0.085)
Share of peers who chose other funds in period 1	−0.235 (0.710)	−0.004 (0.074)	−0.072* (0.042)	0.158 (0.128)	0.024 (0.047)	−0.181 (0.318)
Observations	10,436	10,436	10,436	10,436	10,436	10,436
R <sup>2</sup>	0.038	0.010	0.006	0.009	0.004	0.011
<i>Multinomial logistic model (joint regression)</i>						
Share of peers who chose Fund X in period 1	<b>24.102**</b> <b>(4.924)</b>	12.123 (8.868)	−9.512 (14.307)	12.996* (7.391)	−2.844 (19.202)	−19.896 (21.483)
Share of peers who chose vendor 5 in period 1	58.016** (25.796)	<b>82.360**</b> <b>(30.979)</b>	−1.001 (37.302)	−8.470 (51.555)	19.264 (25.959)	−32.694 (121.459)
Share of peers who chose vendor 6 in period 1	40.039 (26.339)	96.265 (73.781)	<b>133.198**</b> <b>(64.875)</b>	−59.067 (50.095)	2.958 (109.844)	77.250 (50.543)
Share of peers who chose vendor 7 in period 1	9.944 (17.932)	33.366 (29.833)	31.824 (24.340)	<b>52.148*</b> <b>(23.177)</b>	−11.668 (45.432)	46.361** (19.796)
Share of peers who chose vendor 8 in period 1	−28.309 (25.937)	−49.544 (73.194)	−116.458** (56.814)	10.991 (14.776)	<b>6.934</b> <b>(13.704)</b>	−13.943 (30.418)
Share of peers who chose other funds in period 1	−7.556 (38.431)	10.771 (46.353)	−257.626 (157.255)	42.875 (29.497)	58.808 (52.158)	−28.562 (85.181)
Ethnic origin North Africa	0.386 (0.321)	−0.455 (0.685)	−0.641 (1.373)	0.813 (1.275)		−0.399 (0.410)
Ethnic origin Europe	0.327 (0.315)	0.358 (0.804)	−0.602 (1.405)	1.016 (1.408)	−1.234 (1.225)	0.295 (0.362)
Ethnic origin Former SU	0.506 (0.373)	−0.591 (1.043)	−0.339 (1.533)	1.096 (1.334)	−0.654 (1.461)	0.364 (0.360)
Ethnic origin Middle East	0.329 (0.341)	0.009 (0.777)	−0.026 (1.340)	0.663 (1.162)	0.019 (1.366)	0.256 (0.432)
Observations	10,436					
Pseudo R <sup>2</sup>	0.179					

Note: OLS regressions are reported in the top panel, where each of the six columns represents an independent regression with an indicator dependent variable for choice of the specific fund or vendor in the column title. A multinomial logistic regression is reported in the bottom panel. The categorical dependent variable includes the categories in the column titles and the base outcome is the default fund (the most frequent outcome). Robust standard errors, clustered at the department level are in parentheses. All regressions control for Jewish indicator, ethnicity indicators, age, gender, marital status, number of children, place of residence, years of education, type of institution granting highest diploma (university, college or other), academic profession, whether the employee is tenured, department location (urban or rural) and department size; and exclude employees who have already opted out of the default fund in period 1. All regressions exclude departments with less than 3 workers and include 98 departments.

\* Significant at the 10% level.

\*\* Significant at the 5% level.

\*\*\* Significant at the 1% level.

certain professions and certain ethnic groups are also somewhat more likely to opt out. Interestingly, neither education in general nor an economics education is associated with this decision.<sup>30</sup>

Column 3 estimates the overall association between an employee's decision to opt out and the proportion of other employees in her department who opt out during the entire year, controlling for individual and department characteristics (Eq. (1)). Columns 4 and 5 show the association between individual decisions in the second period and decisions taken by peers in the first period (with and without demographic and department controls). The results suggest very strong within-department correlations. For example, when looking at the pooled regression in column 3, a percentage point increase in the proportion of co-workers who opt out is associated with a 0.64 percentage points increase in the individual's probability

<sup>30</sup> The same is true with respect to type of institution granting higher diploma, included in the unreported controls.

**Table 4**

Peer effects (dependent variable: indicator for choice in period 2).

	Ethnicity FE				Ethnicity FE and department FE			
	Opt out		Fund X		Opt out		Fund X	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of peers who chose opt out/Fund X in period 1	0.739*** (0.210)	1.138** (0.519)	0.828*** (0.239)	1.261** (0.614)				
Squared share of peers who chose opt out/Fund X in period 1		-4.642 (4.483)		-5.168 (5.613)				
Share of co-ethnic peers who chose opt out/Fund X in period 1					0.184*** (0.065)	0.281*** (0.080)	0.317*** (0.079)	0.417*** (0.100)
Squared share of co-ethnic peers who chose opt out/Fund X in period 1						-0.308*** (0.092)		-0.463 (0.432)
Ethnic origin North Africa	0.003 (0.008)	0.003 (0.008)	0.007 (0.006)	0.007 (0.006)	0.002 (0.008)	0.000 (0.008)	0.003 (0.006)	0.002 (0.006)
Ethnic origin Europe	0.011 (0.008)	0.011 (0.008)	0.007 (0.005)	0.007 (0.005)	0.009 (0.008)	0.007 (0.008)	0.005 (0.005)	0.004 (0.005)
Ethnic origin Former SU	0.013 (0.009)	0.013 (0.009)	0.009 (0.007)	0.009 (0.007)	0.010 (0.008)	0.010 (0.009)	0.007 (0.006)	0.007 (0.006)
Ethnic origin Middle East	0.009 (0.008)	0.009 (0.008)	0.005 (0.006)	0.005 (0.006)	0.006 (0.008)	0.005 (0.008)	0.002 (0.006)	0.001 (0.006)
Observations	10,436	10,436	10,436	10,436	10,345	10,345	10,345	10,345
R <sup>2</sup> /pseudo R <sup>2</sup>	0.045	0.046	0.037	0.037	0.065	0.065	0.060	0.060
Department fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department controls	Yes	Yes	Yes	Yes	No	No	No	No

Note: OLS, standard errors clustered at the department level in parentheses. Dependent variable is an indicator variable for choosing Fund X or opting out of the default fund (indicated in the Column titles) in period 2. Unreported individual controls include Jewish indicator, age, gender, marital status, number of children, place of residence, years of education, type of institution granting highest diploma (university, college or other), academic profession, whether the employee is tenured. Unreported department controls include location and size. All regressions exclude employees who have already opted out of the default fund in period 1 and departments with less than 3 workers and include 98 departments.

\*\* Significant at the 5% level.

\*\*\* Significant at the 1% level.

to opt out. The association between an employee's choice and that of her peers in the *previous* period is, if anything, slightly stronger.<sup>31</sup>

## 6.2. Choosing particular funds

As discussed above, these within-department correlations in choices may be partly due to common unobserved characteristics (selection effects). However, it seems plausible that, since observed differences between non-default funds are small, common unobserved personal characteristics are less likely to explain correlation in choices of specific funds. Table 3 examines the choice between the default fund, the five largest vendors described in Table 1, and the rest of the funds. The top panel shows separate OLS regressions for each fund/vendor. These regressions are similar to those reported in column 5 of Table 2, but with the choice of a specific fund/vendor as dependant variable and with the share of peers choosing each of the vendors as explanatory variables. The results for Fund X indicate significant within department correlation in choice. The estimated effects for the other (smaller) funds are mostly insignificant. It is noteworthy, however, that the largest point estimates for vendors 5, 6 and 7 are for the share of peers choosing those specific vendors.

The bottom panel presents an analysis of the full (multinomial) choice of the employee. Specifically, we estimate a multinomial logit model where the dependent variable is a categorical variable taking seven values according to whether the employee stayed in the default (the base outcome) or chose one of the six alternatives indicated in the table columns. The results are instructive. For all but the smallest vendor,<sup>32</sup> we find a positive and statistically significant association between the likelihood of choosing that specific fund and the share of one's co-workers who chose that fund in the previous period (coefficients shown in bold type). There is also some suggestive indication of spillover effects between the choices of Fund X and vendor 5. Finally, it is important to note that, as seen from the coefficients on the ethnicity indicators, none

<sup>31</sup> As a way of checking how likely it is to obtain such within-department correlations by chance, we did the following. We randomly assigned each employee a fictitious department using the empirical distribution of department membership and then re-ran the regression from column 5 in Table 2. We repeated this procedure 1000 times. Out of 1000 iterations, the highest estimated coefficient was 0.489 (compared to the estimate of 0.739 obtained in the original dataset).

<sup>32</sup> Recall from Table 1 (third column) that by far the most popular fund in this period is Fund X, followed by vendor 7 and vendors 5 and 6.

of the funds/vendors appears to be more or less attractive to any of the major ethnic groups (the same is true also without department controls). While we will not be able to perform a similar analysis using within-department variation across ethnic groups (there are simply not enough people who switched to each of the non-X funds), the results in Table 3 do seem to be consistent with peer influence not only at the opting out level but at the level of choice of particular funds.

### 6.3. Peer effects

Of course, even if the estimated correlations in Table 3 are not due to selection effects, they could be driven, at least partly, by common shocks at the department level (e.g. a marketing campaign). We therefore examine whether an employee's decision to join a specific fund is associated with the choices of peers from her ethnic group, beyond any effects that may operate at the department level.

Before we present the results, however, we ought to examine whether members of a given ethnic group in a given department differ systematically from members of that same ethnic group in other departments, controlling for the overall differences across departments. To explore this possibility, we examine whether such differences exist in observable personal characteristics. Specifically, we regress individual characteristics (such as years of schooling or age) on the set of ethnicity fixed effects (some ethnic groups may be more educated than others), department fixed effects (some departments may attract more educated people) and the interactions between the department and ethnicity fixed effects. Our main interest is in the latter. Since there are over 500 such interactions per regression, a convenient way to summarize the results is the distribution of the *t*-statistics of the interaction coefficients. These are presented in Fig. 4. While some ethnic groups in some departments do turn out to be different in some particular characteristic, most of these coefficients are statistically indistinguishable from zero. More precisely, between 91% and 98% of the interaction coefficients are statistically insignificant at the 95% level (see note to the figure).

We now come to some of our key results, presented in Table 4. We examine first the decision to opt out and then focus on the choice of the largest fund (there is very little within-department variation in the choices of the smaller funds). Columns 1–4 are presented primarily for reference. The first two columns show the overall association between the likelihood of an employee opting out and the proportion of peers who have opted out, controlling for ethnicity fixed effects, individual and departmental characteristics. The only difference between the columns is that in column 2 we allow for non-linear effects by including a quadratic term. Columns 3 and 4 do the same with respect to the choice of Fund X. It is, of course, hard to say whether and to what extent these associations are due to peer effects and how much of them reflect department-level shocks.

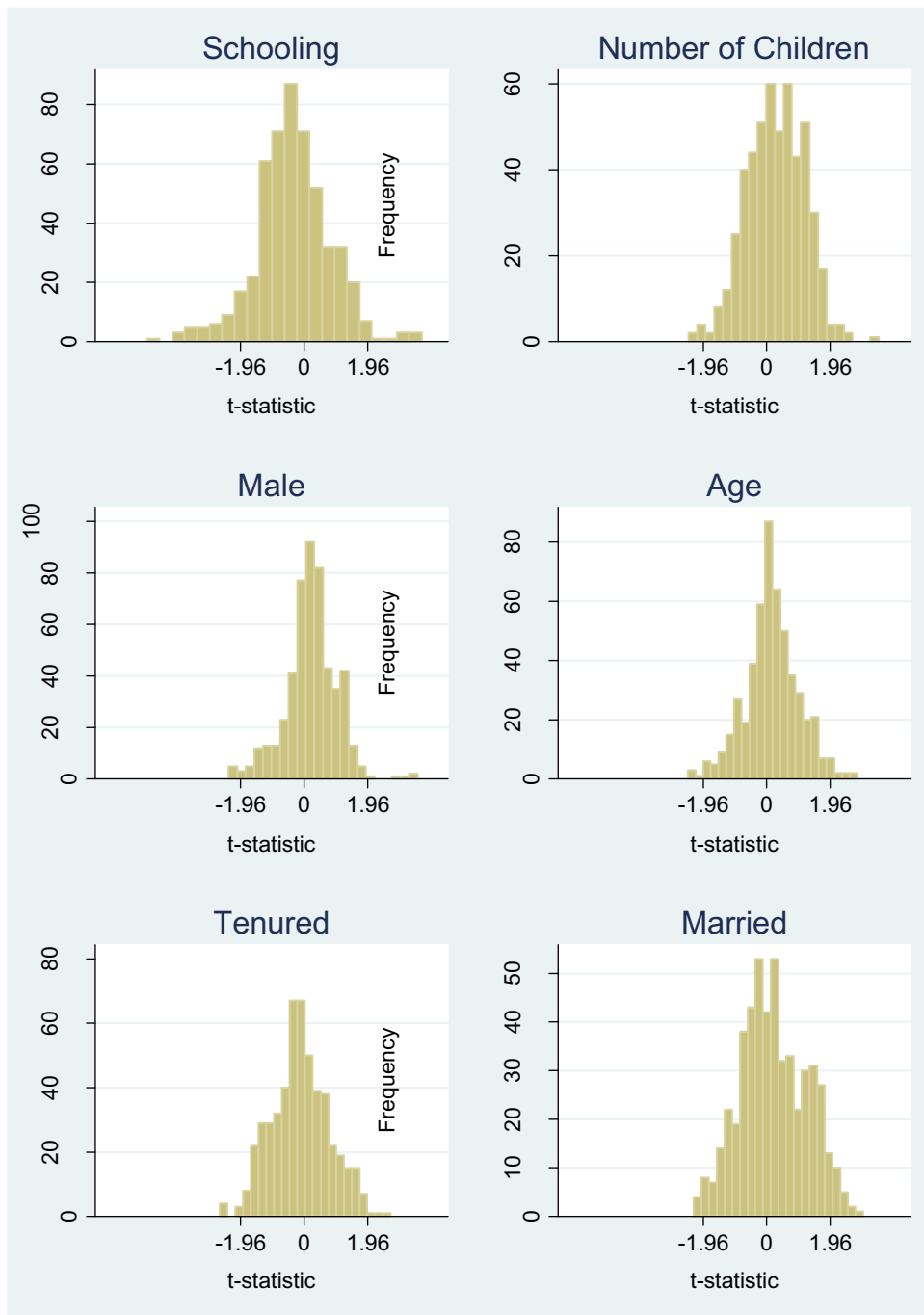
In columns 5–8 we estimate the effect of co-ethnic peers controlling for department fixed effects (Eq. (2)). That is, we now examine whether individuals are more likely to opt out, or to choose a particular fund, when a higher proportion of members of their ethnic group have made that choice – above and beyond any general tendencies of certain departments to make these choices due to selection, marketing or other factors related to the common institutional environment. The results are striking. Consider column 7. Employees in the *same* department are significantly more likely to choose Fund X the higher the proportion of co-ethnics in that department who chose that fund. The estimates suggest that a one percentage point increase in the share of co-ethnic peers who chose Fund X in the first period is associated with a 0.317 percentage point increase in the likelihood of choosing that fund in the second period – controlling for department and ethnicity fixed effects. A similar pattern is seen with respect to opting out (column 5).

As another way to gauge the significance of within-department ethnic peer effects, we performed the following two simulation exercises. First, we randomly assigned employees to (fictitious) departments drawing from the empirical distribution of department membership. We then re-ran the regression from column 7 of Table 4, examining the association between the decision to choose Fund X in period 2 and the share of co-ethnic peers (in the fictitious department) who chose the same fund in period 1. Repeating this procedure 1000 times, none of the iterations yielded a coefficient at least as high as the coefficient in the original data (the highest estimate was 0.247 as compared with 0.317 in the original dataset). Second, we randomly assigned employees to ethnic groups (using the empirical distribution) keeping the true department membership, and estimated the same regression. Out of 1000 iterations, only two yielded a coefficient at least as high as the coefficient in the original data (the highest estimate was 0.331).

Returning to Table 4, columns 6 and 8 explore non-linear effects. The estimates appear consistent with decreasing marginal effects (although the coefficients are not precisely estimated for Fund X). That is, the effect of the first employees who choose to opt out on the choices of their peers appears to be larger than the effect of additional employees making this choice.

Finally, as seen from the estimated coefficients on the ethnicity indicators, and consistent with the results in Table 3, the four main ethnic groups do not differ in their overall propensity to either opt out or choose Fund X. In other words, it does not appear to be the case that Fund X is more attractive to one ethnic group than to another. The most plausible interpretation of these results is thus the existence of strong peer effects.

To complete the picture, in Table 5 we also show cross-group correlations for the four large ethnic groups. That is, we regress an individual's choice of Fund X on the proportion of peers in a given ethnic group (not necessarily her own) who previously chose that fund, controlling for individual and department characteristics. For example, in the third cell



**Fig. 4.** Balancing tests. The figures show the results from six separate regressions of an individual characteristic (years of schooling; number of children; male indicator; tenured indicator; married indicator; and age) on the full set of department fixed effects, ethnicity fixed effects, and all interactions between them. Each histogram shows the distribution of the  $t$ -statistics of the 509 interaction coefficients in one of these regressions. The interaction coefficients for schooling, number of children, proportion male, tenured, married and age are statistically insignificantly different from zero (at the 95% significance level) in 91%, 97%, 98%, 98%, 94% and 97% of the interactions, respectively.

of the first column we regress the choices of individuals of North-African descent on the earlier choices of their peers of European descent. Since the proportion of peers from a given ethnic group who chose  $X$  does not vary within department, we cannot use department fixed effects and the coefficients may capture selection, correlated, and peer effects. Nonetheless, consistent with the results in Table 4 and with sociological accounts, the association of an employee's decision with the decisions of her own ethnic group tends to be significantly stronger than the association with members

**Table 5**

Cross ethnic groups regressions (dependent variable: indicator for choice of Fund X in period 2).

	Ethnic group subsamples			
	Ethnic origin North Africa (1)	Ethnic origin Middle East (2)	Ethnic origin Europe (3)	Ethnic origin Former SU (4)
Share of North Africa Ethnic origin peers who chose Fund X in period 1	0.480*** (0.136) {95} [2533]	0.634*** (0.184) {95} [2420]	0.128 (0.115) {92} [2291]	0.452 <sup>†</sup> (0.254) {93} [1479]
Share of Middle East Ethnic origin peers who chose Fund X in period 1	0.108 (0.123) {95} [2530]	0.689** (0.221) {93} [2420]	0.158 (0.159) {93} [2297]	0.683** (0.339) {93} [1476]
Share of Europe ethnic origin peers who chose Fund X in period 1	0.0145 (0.268) {92} [2511]	0.252 (0.220) {93} [2407]	0.614*** (0.213) {90} [2296]	0.047 (0.230) {91} [1478]
Share of Former SU ethnic origin peers who chose Fund X in period 1	-0.050 (0.052) {93} [2521]	0.336 (0.161) {93} [2417]	0.004 (0.091) {91} [2292]	0.398 <sup>†</sup> (0.220) {84} [1471]

Note: Each cell represents a separate OLS regression where the dependent variable is individual  $i$ 's choice of Fund X in period 2 and the sample is restricted to the ethnic group indicated in the column title. The table reports the estimated coefficients on the average enrollment to Fund X in a particular ethnic group indicated in the row (excluding  $i$ ). This independent variable differs from row to row. Robust standard errors, clustered at the department level are in (parentheses). Number of departments is in {braces} and number of observations is in [brackets]. All regressions control for Jewish indicator, age, gender, marital status, number of children, place of residence, years of education, type of institution granting highest diploma (university, college or other), academic profession, whether the employee is tenured, department location (urban or rural) and department size; and exclude employees who have already opted out of the default fund in period 1. All regressions exclude departments with less than 3 workers.

<sup>†</sup> Significant at the 10% level.

\*\* Significant at the 5% level.

\*\*\* Significant at the 1% level.

of other ethnic groups. The main exception is the group of immigrants from the Former Soviet Union (the smallest group).<sup>33</sup>

Finally, Table 6 restricts attention to two specific subsamples using identical specifications as in columns 7 and 8 in Table 4. First, columns 1 and 2 report the results of logit estimation. This requires the exclusion of observations where the outcome is perfectly predicted by one of the dummy variables included. Most importantly, this means that we exclude departments in which no employee switched to Fund X in the second period. Despite the smaller sample, the results are qualitatively similar to those in Table 4: choice of Fund X is strongly associated with the choices of that fund by co-ethnic peers. OLS estimates on this sub-sample are almost identical to Table 4 columns 7 and 8. Results are also similar when limiting the analysis to the 33 departments that had some switching to Fund X in the first period (results not shown).

Second, about half of the employees in our sample are concentrated in thirteen large departments, i.e. departments with over 200 employees. One might therefore worry that the results are driven by what happened in a small number of departments. In columns 3 and 4 we exclude these large departments. The results are again similar to the ones obtained in Table 4. The estimated peer effects are stronger when restricting attention to even smaller departments (less than 100 employees; results not shown). This is not surprising: it seems plausible that department-level peer effects are stronger in smaller units where employees are more familiar with each other.

## 7. Qualitative evidence from interviews

To complement our empirical results we conducted structured interviews with a sample of our investigated population. Conducting interviews (rather than sending out questionnaires) allows us to validate the true identity of the person answering the questions and the time and effort spent in answering them. We use a closed fixed response interview, in which all interviewees are asked the same set of questions with the same set of multiple-choice answers.<sup>34</sup>

A total of 150 employees were randomly sampled from three different strata as follows. Forty employees from the group of employees who remained in the default fund, sixty employees from the group who chose Fund-X; and fifty employees

<sup>33</sup> As mentioned, Maman (1991) documents that in Israel ethnicity plays an important role in one's social network. As seen in Table 5, decisions of Jews originating from North Africa (the largest group in our sample) are statistically significantly associated only with decisions of peers with the same origin. The same is true for Jews originating from Europe. Jews originating from the Middle East are commonly grouped together with Jews from North Africa, and indeed the second column indicates a strong association with the latter group as well. Similar patterns also show up when looking at opting out decisions (not shown).

<sup>34</sup> A detailed analysis of the interviews appears in Mugerman (2010). In this section we focus on those questions most relevant to the present study.

**Table 6**

Robustness checks (dependent variable: indicator for choice of Fund X in period 2).

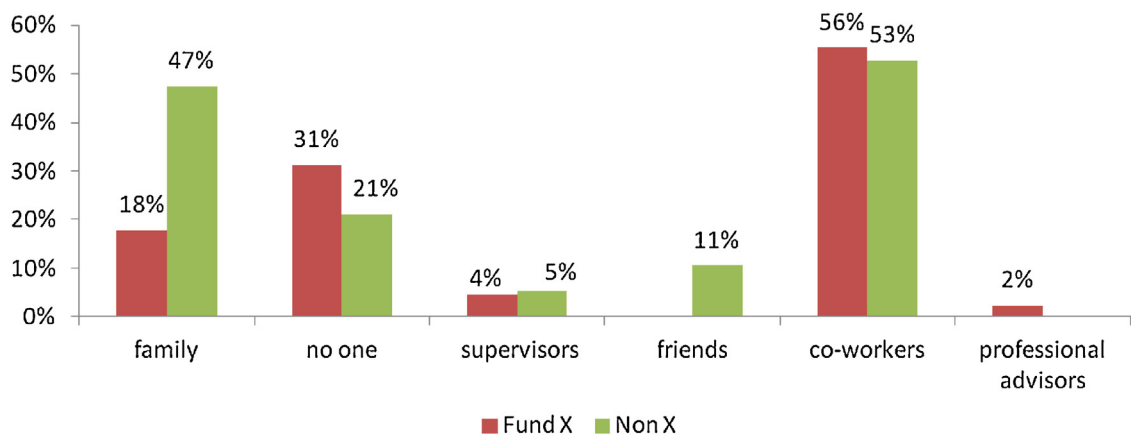
	Ethnicity FE and department FE			
	Logit		OLS excluding large depts.	
	(1)	(2)	(3)	(4)
Share of co-ethnic peers who chose Fund X in period 1	6.147 <sup>***</sup> (1.788)	9.366 <sup>**</sup> (3.528)	0.324 <sup>**</sup> (0.086)	0.532 <sup>***</sup> (0.089)
Sq. share of co-ethnic peers who chose Fund X in period 1		-11.383 (9.541)		-0.836 <sup>**</sup> (0.482)
Ethnic origin North Africa	0.210 (0.302)	0.123 (0.329)	-0.006 (0.006)	-0.009 <sup>†</sup> (0.005)
Ethnic origin Europe	0.188 (0.244)	0.130 (0.250)	0.001 (0.005)	0.000 (0.005)
Ethnic origin Former SU	0.386 (0.311)	0.355 (0.316)	0.002 (0.006)	0.001 (0.006)
Ethnic origin Middle East	0.197 (0.292)	0.148 (0.294)	0.001 (0.005)	-0.001 (0.005)
Departments	42	42	85	85
Observations	6921	6921	5125	5,125
R <sup>2</sup> /pseudo R <sup>2</sup>	0.138	139	0.054	0.055
Department fixed effects	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Department controls	No	No	No	No

Note: Each column represents a different regression. Robust standard errors in parentheses are clustered at the department level. Unreported individual controls include Jewish indicator, age, gender, marital status, number of children, place of residence, years of education, type of institution granting highest diploma (university, college or other), academic profession, whether the employee is tenured. Unreported department controls include location and size. Logit model regressions are reported in columns 1 and 2. Columns 3 and 4 report OLS estimations of the *restricted sample* (excluding departments with over 200 employees). All regressions exclude employees who have already opted out of the default fund in period 1 and departments with less than 3 workers.

<sup>†</sup> Significant at the 10% level.

<sup>\*\*</sup> Significant at the 5% level.

<sup>\*\*\*</sup> Significant at the 1% level.



**Fig. 5.** Who did you consult with before deciding to switch to your current fund? Responses to structured interviews conducted with a subsample of the employees who switched out of the default ( $N = 64$ ).

who switched to non-X funds. Of these, 13, 45 and 19 (respectively) agreed to participate and completed the interviews, for a total of  $N = 77$ .<sup>35</sup> The interviews were conducted during 2008. Each interview was conducted at the employee's workplace. Those that agreed to be interviewed received a small reward (thermos cup) for their participation.

Most of the employees in our sample think that their fund choice is an important decision. When asked to rank the importance of five different important decisions – buying home furniture; buying a car; choosing a career path; choosing which area to live in; and choice of savings fund – 75% rank the choice of savings fund as one of the three most important decisions (10% ranked it last). In addition, when asked how frequently they check their fund's performance, 21% of the employees interviewed claim they do it monthly, while 69% of them claim that they do it quarterly. Only 10% say they never check it or check it annually.

<sup>35</sup> The low participation rate of default-individuals may in itself indicate their relative lack of interest in this issue, relative to the other two groups.



Survey participants were also asked which indicators, if any, out of several publicly available indicators they examine to evaluate fund performance. 74% of the employees pointed to the fund's past rate of return; 35% mentioned the fund's management fees; 35% mentioned comparative returns relative to other funds; while only 4% mentioned risk measurement. The relatively low number of respondents who mentioned management fees is interesting, given that this is an important, predictable, and easily understood indicator. However, when asked what the return of their fund was in the previous year, 64% said that they did not know. Further, out of those who claimed to know the fund's return, one half gave a number, which is wrong by more than one percentage point.

Perhaps the most important question asks "Whom did you consult with before deciding to switch to your current fund?" The options were: (a) bank consultants; (b) co-workers; (c) supervisors; (d) friends outside of work; (e) family; (f) others; and (g) I did not consult with anyone. The results are in Fig. 5. By far the most important factor mentioned was co-workers (55%) followed by no-one (28%) and family (27%).<sup>36</sup> This is consistent with our interpretation of the empirical findings as indicating peer effects in actual choice of funds.

## 8. Conclusion

Savings decisions are among the most important financial decisions most individuals make. Yet these decisions are not fully understood. This complicates the design of policy reforms to improve the quality of financial decisions. This paper exploits a large and detailed dataset that allows us to shed some light on how these decisions are made. Our first finding is that a drastic and highly publicized regulatory reform allowing savers to switch saving plans did not result in many savers actually switching. Most employees remained with the default option, and perhaps justifiably so. Furthermore, while the reform was followed by an increase in the number of funds, the increased choice given to consumers did not result in clearly improved terms. Data from the Israeli Ministry of Finance indicates that if anything, management fees have increased in the years following the reform. The average management fee in the provident fund industry in 1999–2004, just prior to the enactment of the reform, hovered between 0.45% and 0.52%. In 2007, the period we study, it was 0.65% and by 2009 it was over 0.8%.<sup>37</sup> This pattern appears consistent with results from Mexico's privatized pension system launched in 1997. Fees in the newly launched system were remarkably high and firms did not seem to compete fiercely on management fees. Rather, they engaged in advertising to increase brand loyalty and actually reduce demand elasticity (Hastings et al., 2013a).

Beyond documenting the overall low number of switches across funds, our data allows for a deeper investigation into the details of individual savings decisions. We find that even when individuals choose to opt out of the default, the considerations guiding their decisions seem to be quite different from what one would expect given observable measures of performance (risk, returns and management fees). Rather, individuals seem to be strongly influenced by their peers, who are not necessarily experts in the subject matter. Indeed, individuals seem to be more strongly influenced by those peers who happen to share their ethnic background.

There are several potential explanations for this behavior. One possibility is "keeping up with the Joneses". In Gomez et al. (2009) the Joneses preferences depend on peers' wealth and DeMarzo et al. (2004) agents hedge against price fluctuations of a local good which is in short supply. In our context, we find it less appealing to argue that one ethnic group in a particular department is hedging different local goods than the others in the same department. Another possibility is social identification: individuals tend to follow the norms or prototypical behavior of members of groups they identify with (Akerlof and Kranton, 2000; Shayo, 2009). This could help explain why our employees follow co-ethnics more than other co-workers. The final explanation emphasizes information and observational learning (the seminal papers here are Bikhchandani et al., 1992; Banerjee, 1992). However, as recently emphasized by Eyster and Rabin (2011), rational observational learning does not necessarily lead to the imitation of predecessors and can in fact lead individuals to contradict the behavior they observe. But even if observational learning is an important component underlying our findings, our results highlight the importance of understanding who it is that people learn from and imitate. The most knowledgeable may not be the most influential.

Many countries are in the process of reforming saving and pensions systems. The patterns documented in this study may have important implications for the design of effective financial reforms. Promoting more choice for savers, even if this is accompanied by an increased number of funds competing in the marketplace, does not guarantee improvements in savings choices. One possibility for improvement is to incorporate workplace seminars and financial education programs as integral parts of the reform. While the literature investigating whether financial literacy programs improve economic decision-making at the individual or household level finds mixed results and some of the studies are subject to methodological criticism (for recent surveys see Gale et al., 2012; Hastings et al., 2013b; Fernandes et al., 2014 provide a meta-analysis), most of the research on employee-targeted financial education at the workplace suggests significant positive effects on savings behavior (e.g. Bernheim and Garrett, 2003; Lusardi, 2002). Another possibility is to provide employees with impartial

<sup>36</sup> The results concerning co-workers are in line with Benartzi and Thaler (2007) who report that investors tend to consult with peers and friends who don't necessarily qualify as financial experts. Consulting with family is consistent with Hvide and Ostberg (2012) that document that one's family influences an individual's choice regarding which stocks to invest in.

<sup>37</sup> The average management fees for the period 1999–2005 was 0.49% while the average fees for the period 2006–2011 was 0.74%. There are many potential causes for such an increase. This issue is beyond the scope of this paper. For our purposes it is interesting to note that in the years before the reform management fees were stable, whereas following the reform we observe a substantial increase.

professional counseling paid for by the employer (see Willis, 2011 for discussion). We are also currently investigating how improving the availability and framing of information on personal finances and investment options affect saving decisions. A final possibility is to design incentives that help consumers overcome behavioral biases like availability heuristics and procrastination. Regardless of which of these policies is most effective, our results suggest that early campaigns may have substantial long-term effects by affecting early savings decisions and harnessing the power of peer effects.

## Appendix A.

**Table A1**

Variables description.

Variable	Description
Choice of fund	There are 59 chosen options, including default selection (among over 200 potential options)
Residence	There are 6 regions: Dan district (Tel Aviv), Jerusalem district, North region, South region, Haifa district, Other
Ethnic origin	The country of birth of paternal grandfather.
Academic profession	Economists, Engineers, Industrial Engineers, MDs, other
Education	Years of schooling
Department	103 autonomous departments
Department size	Number of employees in the departments

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