NBER WORKING PAPER SERIES

RETIREMENT PLAN TYPE AND EMPLOYEE MOBILITY: THE ROLE OF SELECTION AND INCENTIVE EFFECTS

Gopi Shah Goda
Damon Jones
Colleen Flaherty Manchester

Working Paper 18902
http://www.nber.org/papers/w18902

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2013

We would like to thank Dan Black, John Beshears, Jeff Brown, Kristine Brown, Ezra Golberstein, Jeff Grogger, David Molitor, Aaron Sojourner and participants at the University of Minnesota CHRLS and Population Center, Massachusetts Institute of Technology IWER, and University of Wisconsin-La Crosse research seminars for their helpful comments. This research was supported by the Center for Retirement Research at Boston College pursuant to a grant from the U.S. Social Security Administration through the Steven H. Sandell 2011-2012 Grant Program for Junior Scholars in Retirement Research. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Retirement Plan Type and Employee Mobility: The Role of Selection and Incentive Effects
Gopi Shah Goda, Damon Jones, and Colleen Flaherty Manchester
NBER Working Paper No. 18902
March 2013
JEL No. H0,J26,J32

ABSTRACT

Employer-provided pension plans may affect employee mobility both through an "incentive effect," where the bundle of benefit characteristics such as vesting rules, pension wealth accrual, risk, and liquidity affect turnover directly, and a "selection effect," where employees with different underlying mobility tendencies select across plans or across firms with different types of plans. In this paper, we quantify the role of selection by exploiting a natural experiment at a single employer in which an employee's probability of transitioning from a defined benefit (DB) to a defined contribution (DC) pension plan was exogenously affected by default rules. Using regression discontinuity as well as differences-in-regression-discontinuities (DRD) methods, we find evidence that employees with higher mobility tendencies self-select into the DC plan. Our results suggest that selection likely contributes to the observed positive relationship between the transition from DB to DC plans and employee mobility in settings where employees sort into plans or employers. Counter to conventional wisdom, we find a negative direct effect of the DC plan on turnover relative to the DB plan, which underscores the multi-dimensional difference between these plans.

Gopi Shah Goda
Stanford University
SIEPR
366 Galvez St.
Stanford, CA 94305
and NBER
gopi@stanford.edu

Damon Jones
Harris School of Public Policy
University of Chicago
1155 East 60th Street
Chicago, IL 60637
and NBER
damonjones@uchicago.edu

Colleen Flaherty Manchester
Work and Organizations
University of Minnesota
Room 3-300R
321 - 19th Avenue South
Minneapolis, MN 55455
cmanch@umn.edu

1 Introduction

Among private sector employees in the U.S. with an employer-provided pension plan, the fraction covered solely by a defined contribution (DC) plan more than tripled between 1980 and 2003, while those covered solely by a defined benefit (DB) plan declined by over eighty percent (Buessing and Soto, 2006). At the same time, there has been evidence of increased employee mobility (Munnell, Haverstick and Sanzenbacher, 2006; Farber, 2007). Because DB and DC plans typically differ in how employee tenure relates to pension wealth, it is commonly thought that the increase in DC plans led to this increase in mobility. Understanding the causal link between mobility and pension plan type is an important labor market issue given the large number of firms and employees affected by the transition.

On the one hand, DC plans may indeed cause greater mobility relative to DB plans given that the benefit structure of DB plans often rewards employees who spend their entire career with a single employer through longer vesting requirements and back-loaded pension wealth accrual. These plan features act to reduce portability (Mitchell, 1982; Lazear, 1990), while the wealth accrual in DC plans is generally age-neutral. On the other hand, DB plans and DC plans differ in multiple dimensions, such as control of financial decision-making, access to liquidity, and the transparency of wealth accrual. If employees find these features desirable, DC plans may actually increase retention. In fact, studies have found that, similar to DB plans, workers in DC plans have lower mobility relative to uncovered workers (Gustman and Steinmeier, 1993; Andrietti and Hilderband, 2004). While uncovered workers are not the relevant comparison when evaluating the effect of plan *type* on mobility, this finding does question conventional wisdom that only DB-plan features reduce mobility.

In addition to differences in plan features, the selection of workers across plans may drive part of any observed relationship between mobility patterns and pension plan type. Understanding the causal effect of pension plan type on turnover requires estimating the direct effect of plan features on employee turnover, which we refer to as an *incentive effect*, separate from the *selection effect*, defined as differences in turnover that stem from the underlying relationship between mobility tendencies and preferences for plan characteristics. However, disentangling the incentive effect from the selection effect has typically been challenging because it requires comparing mobility

across workers who are enrolled in different plans but are otherwise similar.

This paper identifies the role of selection in the relationship between employee mobility and pension plan type by exploiting a natural experiment at a single employer in which existing employees faced a one-time, irrevocable option to transition from a DB plan to a DC plan. We exploit exogenous variation in the probability of switching to the DC plan caused by the default rule that governed the plan transition. In particular, existing employees who were under age 45 at the time of the transition were assigned the DC plan as the default plan, while employees age 45 or older were assigned the DB plan as a default. Defaults have been shown to have dramatic effects on DC enrollment (Madrian and Shea, 2001), and this result holds across a variety of private employment contexts (Choi et al., 2004) as well as in public sector pension plans (Cronqvist and Thaler, 2004).

The features of the default rule in our context allow us to use a fuzzy regression discontinuity (RD) approach to estimate the exogenous effect of changing to the DC plan from the DB plan on employee mobility. To improve the precision of our estimates, we combine our data in the year of the policy change with data from previous years in a fashion similar to a differences-in-differences (DD) framework. We hereafter refer to this approach as a differences-in-regression-discontinuities (DRD) estimator. We quantify the role of selection by comparing the DRD effect to OLS estimates, which are confounded by employee selection. We find evidence that employees with higher mobility tendencies select into the DC plan in that the DRD estimates of the effect of the DC plan on turnover are significantly less than the OLS estimates.

This paper contributes to the literature by providing a new source of identification with which to quantify the role of selection into pension plans based on mobility. Prior studies have generally addressed this selection by using selection-correction models or cross-sectional data that includes heterogeneous firms and plans (Allen, Clark and McDermed, 1993; Gustman and Steinmeier, 1993; Rabe, 2007). Other studies have used plausibly exogenous variation from tax reforms (Andrietti and Hilderband, 2004) or plan offerings (Disney and Emmerson, 2004; Manchester, 2010) to identify the consequences of pension plan type for mobility. The regression discontinuity approach we use relies on credible and testable assumptions, namely that unobservable determinants of mobility rates for affected employees relative to non-affected (but otherwise similar) employees did not change discontinuously at the age governing the default plan. Our identification assumption passes falsification tests which show no evidence of a discontinuous change in mobility at alternative age thresholds or

in years prior to the policy change. In addition, we do not detect a discontinuous change in other, predetermined observables at the age threshold.

We contribute to the literature in two additional ways. First, we develop a conceptual framework for evaluating the effect of introducing a new benefit on mobility that allows for heterogeneity in preferences over the benefit, costs of switching, and mobility costs. We show that the resulting relationship between benefit enrollment and mobility depends on the joint distribution of this multi-dimensional heterogeneity as well as the choice environment in which the new benefit is offered. In particular, whether employees have the opportunity to self-select into the new benefit as compared to being forced to enroll has different implications for mobility over and above the inter-relationship between the different sources of heterogeneity. We use both of these insights to generate testable predictions for our estimated parameters and to provide a richer interpretation of our empirical evidence.

This framework sheds new light on previous findings of pension plans and employee mobility. In particular, previous evidence has shown that both DB and DC plans may reduce employee mobility (e.g. Gustman and Steinmeier, 1993; Ippolito, 2002). It has been hypothesized that this result is due to compensation premiums for employees with a pension plan relative to those without (Gustman and Steinmeier, 1993), and the possibility that the retention effect is driven by preferential treatment of savers by employers (Ippolito, 2002). Our framework implies that the overall effect of plan type on mobility depends on the sign and magnitude of the incentive and selection effects.

Applying this framework to our setting, we find that the selection effect tends to induce a positive relationship between mobility and DC plan enrollment, although this is offset in our context by a negative incentive effect of DC plans. Therefore, our results are consistent with the possibility that the bundle of DC plan features, including increased control, transparent wealth accrual, and loan and withdrawal provisions, are desirable relative to those of the DB plan (as measured by higher retention), in line with previous work that finds a low perceived benefit of additional DB benefits (Fitzpatrick, 2011; Brown et al., 2011). Of course, our results are in part specific to our context and the specific features of the DB and DC plan under consideration.[1]

[1]In particular, the DB plan in our setting is not backloaded, as we discuss in Section 3. This means that our estimate of the positive selection effect may be understated, and our estimate of the negative incentive effect may be overstated, relative to standard, backloaded DB plans.

Our second, additional contribution is that we are able to evaluate both the short-term and longer-term effects of DC plans on mobility as our data extends to three years beyond the DC plan introduction. With the exception of Allen, Clark and McDermed (1993), most studies evaluating the relationship between pension plan and mobility use a one-year time frame (Gustman and Steinmeier, 1993; Andrietti and Hilderband, 2004; Disney and Emmerson, 2004; Rabe, 2007). We find that the DC plan had an immediate and relatively large negative effect on mobility rates for those exogenously moved to the plan over a one-year time horizon. However, the effect eventually deteriorates as additional years are included in the analysis. These findings suggest that our estimates may represent a temporary change, rather than a longer-run retention of employees. In other words, affected employees appear to delay their exit, but only for a limited period of time.

The remainder of the paper proceeds as follows. Section 2 describes the conceptual framework that motivates our empirical approach and examines what our results may reveal about the relationship between mobility tendencies and pension plan preferences. Section 3 provides details regarding the natural experiment we exploit in our empirical application. We outline our RD and DRD empirical strategies in Section 4 and present our results along with robustness checks in Section 5. Section 6 explores the implications of our results and concludes.

2 Model of New Benefit Enrollment and Mobility

We construct a conceptual framework for interpreting observational and quasi-experimental estimates of the relationship between mobility patterns and employee benefit enrollment in the presence of unobservable heterogeneity. To do this, we first propose a basic framework that governs individual decisions regarding enrollment in the newly-offered benefit and subsequent turnover. Second, we evaluate this framework in two distinct choice scenarios for benefit enrollment. Finally, we show how comparing the effect of new benefit enrollment on turnover in these two scenarios provides insight into the selection effect, the relationship between underlying mobility tendencies and preferences for the new benefit.

We model the discrete decision between a new employer-provided benefit and an existing one, and the subsequent decision to leave or stay with one's current employer. An employee in our model, indexed by i, has three sources of individual-level heterogeneity: ϕ_i, which determines her

relative valuation of the new employee benefit over the old option; $c_i > 0$, which represents the employee's cost of switching to the new employee benefit; and m_i, which dictates the mobility tendencies associated with switching to a new employer. These three sources of heterogeneity are governed by a joint distribution with CDF $F(\cdot) : \mathbb{R}^3 \to [0, 1]$.

We define B_i to be a binary variable indicating enrollment in the new benefit at one's current employer and $Leave_i$ to be a binary variable indicating departure from the current employer. For example, in our setting $B_i = 1$ indicates that an employee is observed enrolled in the DC retirement plan rather than the DB plan, while $Leave_i = 1$ indicates that an individual has subsequently left the firm within one year of being initially observed. An employee maximizes her expected utility, $\mathbb{E}[V_i(w_i, B_i)]$ which, among other things, depends on the employee's wage w_i, the status of her benefit participation B_i, and her choice of employer.

We begin with the benefit enrollment decision. The parameter ϕ_i, which captures the net utility change of enrolling in the new benefit, is defined as follows:

$$\phi_i \equiv \mathbb{E}[V_i(w_i, 1)] - \mathbb{E}[V_i(w_i, 0)]. \tag{1}$$

Employees with a higher ϕ_i place a higher value on the new benefit. In our context, such employees may prefer a DC plan to a DB plan for a number of reasons, including the net present value, the risk profile of the retirement plan, transparency, portability, control over investment, etc.

In order to realize this utility change, the employee must pay a cost of switching to the new benefit, $c_i > 0$. This may include such costs as time, informational requirements or administrative hurdles associated with switching benefits. If afforded the choice, it follows that the employee will use the following decision rule for adoption of the new benefit:

$$B_i = \begin{cases} 1 & \text{if } \phi_i \geq c_i \\ 0 & \text{if } \phi_i < c_i. \end{cases} \tag{2}$$

We now turn to the decision of whether or not to leave the firm. Denote $V_i^o(w_i^o, B_i^o)$ as the value of working at an outside firm and η_i as a cost of switching employers. We define m_i as the net benefit of leaving the current employer for an outside employer, conditional on having the old

benefit:

$$m_i \equiv \mathbb{E}\left[V_i^o\left(w_i^o, B_i^o\right)\right] - \mathbb{E}\left[V_i(w_i, 0)\right] - \eta_i,$$

where η_i is a parameter that captures the cost of switching employers. Thus, individuals with a higher m_i are more "mobile," in that their outside options tend to be better relative to the current employer and/or they tend to have lower switching costs across employers. The decision to leave the firm can be characterized as follows:

$$Leave_i = \begin{cases} 1 & \text{if } \phi_i \cdot B_i < m_i \\ 0 & \text{if } \phi_i \cdot B_i \geq m_i. \end{cases} \tag{3}$$

We now consider two choice scenarios. In the first case, B_i is endogenously determined by the employees. In the second case, B_i is exogenously determined. In each case, we discuss the association between benefit enrollment and observed mobility and how these relationships may be informative about the joint distribution of (ϕ, m, c). In particular, we are interested in the co-movement of preferences for the new benefit, ϕ, and mobility, m.

In the endogenous case, the employer introduces a new benefit and allows employees to select into this benefit according to the rule in Equation 2. Subsequently, employees make a decision on whether or not to leave the firm according to the rule in Equation 3. Consider a comparison of the subsequent leave probabilities among those enrolled and those not enrolled. We define this difference as:

$$\beta_{Endo} \equiv \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Endo}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Endo}\right], \tag{4}$$

where the "Endo" explicitly indicates that B_i is endogenously determined by the employee.

First, note that those who have chosen to enroll must have a positive value of ϕ_i, given Equation 2 and the assumption that $c_i > 0$. Focusing just on the left-hand sides of the inequalities in Equation 3, those now enrolled have less of a reason to leave the firm relative to those not enrolled, all other things equal. That is, $\phi_i \cdot B_i > 0$ for enrollees. We define this direct effect of the new benefit on the likelihood of leaving as the "incentive effect."

In our context, a negative incentive effect means that the DC plan reduces turnover relative to the DB plan among those choosing to enroll in the new DC benefit. This may seem counterintuitive

given that DC plans are typically more portable. However, recall that the parameter ϕ captures preferences for the multi-dimensional differences between a DC plan and DB plan. All things equal, those who value the DC plan more receive higher utility in the job now that it has a DC plan and are therefore less likely to leave it. Note that, particularly in the context of DB and DC plans, there is a possibility that B_i also directly affects m_i. We return to this case later in the section as an extension of the model.

We now turn to the right-hand sides of the inequalities in Equation 3. The difference in leave probabilities between enrollees and non-enrollees will depend on differences in the distribution of m_i across the two groups. We define the difference in leave probabilities due to differences in the distribution of m_i between enrollees and non-enrollees as the "selection effect." In particular, the sign of the selection effect depends on the following baseline difference in leave probabilities absent the new benefit:

$$\beta_{Selection} \equiv \mathbb{E}\left[Leave_i | \, B_i = 0, \phi_i > c_i\right] - \mathbb{E}\left[Leave_i | \, B_i = 0, \phi_i \leq c_i\right] \lesseqgtr 0. \tag{5}$$

To explore the role of selection, fix $c_i = c$. Conditional on c, if ϕ_i and m_i are independent, then Equation 5 is zero and there would be no selection effect on leave probabilities. In this no-selection case, the incentive effect ensures that leave probabilities are lower for those employees who endogenously enroll in the benefit. Alternatively, assume that, conditional on c, Equation 5 is negative, i.e. there is a negative selection effect. Then the selection effect reinforces the incentive effect, and we would again expect to see lower leave probabilities for enrolled employees. Finally, if Equation 5 is positive, conditional on c, then the selection effect works in the opposite direction of the incentive effect. In this case, the leave probabilities for enrollees may be lower, equal or higher than those of non-enrollees, depending on whether the selection effect only mitigates, neutralizes, or dominates the incentive effect.[2]

Now consider the second choice scenario. In the exogenous case an employer forces all employees to enroll in the new benefit.[3] Imagine comparing the probability of leaving the firm under the new benefit regime as compared to under the original regime. The decision to leave the firm is still dictated by the decision rule in Equation 3. However, now that employees are not self-selecting

[2] We formalize this idea with a Lemma 1 in Appendix A.

[3] In the exogenous case the cost of enrollment c is removed; therefore, the influence of ϕ and c can be separated.

into the new benefit, we no longer have a selection effect since plan enrollment is independent of m. Furthermore, because there is no endogenous enrollment into B_i, it is no longer the case that $\phi_i \cdot B_i > 0$ for all enrollees. Instead, the incentive effect will vary across employees, decreasing the likelihood of leaving among those who have a positive ϕ and increasing the likelihood of leaving for those with a negative ϕ. A comparison of leave probabilities under the new relative to the old benefit regime identifies the average incentive effect of B_i among all employees, or a treatment effect defined as:

$$\beta_{Exog} \equiv \mathbb{E}\left[Leave_i | \, B_i = 1, \text{Exog}\right] - \mathbb{E}\left[Leave_i | \, B_i = 0, \text{Exog}\right], \tag{6}$$

where the "Exog" explicitly indicates that benefit enrollment is exogenously determined. The net change in leave probabilities depends on the number of employees now induced to stay with the firm, i.e. those with m_i and ϕ_i such that $0 < m_i \leq \phi_i$, relative those who are now induced to leave the firm, i.e. those with m_i and ϕ_i such that $0 \geq m_i > \phi_i$, because of the new benefit.

Now that we have defined the estimates for the endogenous and exogenous cases, we can show how the characteristics of the (ϕ, m, c)-distribution manifest in the relative magnitude of the estimates. Fixing $c_i = c$, suppose that m and ϕ are independent, meaning there is no selection effect. This means that the distribution of m does not differ among those who choose to enroll in the new benefit under the endogenous case and under the exogenous case. If there is no selection effect, then we would expect to find a larger reduction in leave probabilities under the endogenous case than the exogenous case (i.e. $\beta_{Endo} < \beta_{Exog}$). This is because those who self-select into the new benefit have relatively higher values for the benefit, and therefore experience larger reductions in the probability of leaving due to the incentive effect, all things equal.[4] Now, suppose that the selection effect is negative. This scenario would further reduce β_{Endo} relative to β_{Exog} because the negative selection effect would reinforce the negative incentive effect present in the endogenous case, again implying $\beta_{Endo} < \beta_{Exog}$. Finally, a positive selection effect would offset the difference between the endogenous and exogenous estimates, potentially even reversing the relative magnitude of β_{Endo} and β_{Exog}.

We formalize these relationships in the following Proposition 1, but first we must define two

[4]This is because the distribution of ϕ among enrollees in the endogenous case is a left-truncated version of the distribution of ϕ among all employees under exogenous enrollment.

additional parameters:

Definition 1. *Define β_0 and β_1 as the effect on leave propensity of exogenously enrolling those who would not have enrolled voluntarily (i.e. $\phi < c$) and those who would have enrolled voluntarily (i.e. $\phi \geq c$) respectively. That is:*

$$\begin{aligned} \beta_0 &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \phi_i < c_i\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \phi_i < c_i\right] \\ \beta_1 &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \phi_i \geq c_i\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \phi_i \geq c_i\right]. \end{aligned}$$

Proposition 1. *If the quasi-experimental estimate defined in Equation 6 is positive (i.e. $\beta_{Exog} \geq 0$), or if exogenous benefit enrollment increases leave propensity by more among those who would not have endogenously enrolled relative to those who would have enrolled (i.e. $\beta_0 \geq \beta_1$), then the difference between the endogenous (Equation 4) and exogenous (Equation 6) estimates is bounded from above by the selection effect defined in Equation 5. That is:*

$$\beta_{Endo} - \beta_{Exog} \leq \beta_{Selection}. \tag{7}$$

We provide a proof in Appendix A. The requirement that β_{Exog} be positive is in principle empirically testable, while the alternative sufficient condition (i.e. $\beta_0 \geq \beta_1$) is sensible and more general, but cannot be tested. Namely it requires that employees who would have self-selected into the new benefit on their own are less likely to leave the firm if exogenously enrolled relative to employees who would not have enrolled on their own.

The implications of Proposition 1 are summarized in Table 1, which maps the possible sign of the selection effect to the implied difference between the endogenous and exogenous estimates. Importantly, Table 1 shows that the mapping is asymmetric in that a negative difference (i.e. $\beta_{Endo} \leq \beta_{Exog}$) is not informative about the sign of the selection effect.[5] As shown in Table 1, in the case where the exogenous estimates show a larger reduction in leave probabilities than the endogenous estimates (i.e. $\beta_{Endo} > \beta_{Exog}$), we can rule out a zero or negative selection effect in favor of a positive selection effect. A regression of turnover (i.e. $Leave_i$) on new benefit enrollment

[5]This may seem counterintuitive given the standard approach of signing omitted variable bias. However, we show in Appendix A.3 why this is the case. In short, the standard omitted variable bias intuition does not hold in the presence of heterogeneous treatment effects and selection on treatment.

among employees who can choose their benefit approximates the endogenous case. As shown above, the correlation between $Leave_i$ and B_i in this choice scenario is driven by both the incentive effect and the selection effect. Estimating the effect of new benefit enrollment on leave probabilities when benefit enrollment is randomly assigned approximates the exogenous case. The effect of B_i on $Leave_i$ identifies the average incentive effect.

A couple of points are worth making about our stylized model. First, it may appear that the dynamics are completely suppressed in our model. In particular, we introduce a friction in decision-making by requiring the enrollment decision to be made before the leave decision, and furthermore do not model forward-looking behavior at the enrollment stage. However, the friction is meant to capture uncertainty about the future leave decision, or at least about the time span between enrollment and leaving. In addition, we can allow for the enrollment decision to be correlated with the leave decision directly through a correlation between ϕ and m, which we have thus far left unrestricted.[6]

Second, we have to this point modeled a new benefit that only affects mobility, m, through its effect on $\mathbb{E}\left[V_i\left(w_i, B_i\right)\right]$. However, the new benefit we examine in our context (the DC plan) has the potential to directly affect mobility, for example, by reducing or eliminating the vesting requirement for retirement benefits. This can be modeled by allowing η_i, the employment switching cost, to be a function of B_i. We have abstracted here from that interaction for brevity and ease of exposition. However, we show in Appendix A.2 that Proposition 1 still holds in this case, so long as we still assume that $\beta_0 \geq \beta_1$.

3 Institutional Setting and Data

3.1 Setting

We use data on unionized, non-faculty employees from a large research university. While our data are from a single institution, the jobs represented in the sample are diverse, ranging from those with low skill requirements (e.g., athletic equipment keeper, food service worker) to relatively high-skilled jobs (e.g., life science technician, computer service, audio equipment specialist). These unionized

[6] In fact, if we had not allowed any friction, then our model would generate the unrealistic prediction that no one who enrolls then leaves the firm, as it would not be optimal to pay the cost of enrolling knowing that one would be leaving the firm.

employees underwent a plan transition on September 1, 2002. All existing union employees could elect to continue participating in the DB plan, or choose to move to the DC plan and cease accruing benefits under the DB plan.[7] If no election was made, the employee was enrolled in the default plan. The default plan was heterogeneous and depended on the employee's date of birth. In particular, employees under age 45 as of September 1 were assigned the DC plan as the default, while employees age 45 or older as of September 1 were assigned the DB plan as the default.[8] We exploit this rich variation in our estimation strategy.

The DB plan at the firm offered benefits equal to 2% of an employees average salary, multiplied by the total years of service at the firm. Because the benefit base was the average salary rather than a final average salary based on the 3 or 5 years prior to retirement, DB benefit accruals were not as "back-loaded" as is often the case with DB plans. These benefits were vested for employees with at least 5 years of service. The DC plan offered a 5 percent employer contribution and matching schedule up to an additional 5 percent.[9] Employer contributions were considered vested after 1 year of service.[10]

3.2 Data

We construct an original data set using administrative data from two sources: annual payroll records that include employees present at the university on December 15 of each year from 1999 to 2005 and pension accrual records. The payroll data includes annual information on job, salary, and weekly hours worked as well as demographic characteristics, including exact date of birth, gender, race, and hire date.[11]

Our primary outcome measure is a binary variable that indicates whether an individual we observe in year t is present in the dataset in year $t+1$. As such, it measures the 1-year probability

[7] The choice governed future benefit accruals only, as past benefit accruals were frozen in the DB plan.

[8] In addition, all unionized employees hired after January 1, 2001 started accruing benefits in the DC plan and did not have a choice of plans. Non-union employees were subject to an earlier plan transition on January 1, 1997. However, our data do not span this earlier policy change. All non-union employees hired after this date were enrolled in the DC plan. Faculty and non-union employees in supervisory roles were never offered benefits in a DB plan unless they experienced job changes that resulted in changes in employment group.

[9] If the employee contributed 1, 2, 3, or 4 percent, the employer contributed 1.5, 3, 4, and 5 percent respectively.

[10] In a study which examines the 2002 transition for union employees, Goda and Manchester (2013) show that the certainty equivalents for the two plans under a base set of assumptions are roughly equal on average across the two plans, though the DC plan is of more value to younger employees and the DB plan is of more value to older employees.

[11] Individuals with missing pension or demographic records were dropped from the analysis (12 individuals). Individuals who had DB accruals, but were rehired following the transition were also dropped (7 individuals).

of leaving the firm, either voluntarily or involuntarily. After the transition, the one-year leave probability among employees that remained in the DB plan was 4.3 percent, while it was 5.3 percent among employees who switched to the DC plan. We also examine the relationship between pension plans and two- and three-year mobility.

The main analysis segments the data into two subsamples: (1) the subset of employees who were present in 2002 and eligible for the September transition from the DB plan to the DC plan, where the default provision varied by the age of the employee on September 1, 2002; and (2) the subset of employees who were employed at the firm in the years 1999-2001. These employees were all enrolled in the DB plan during these years, and because it was prior to the policy change, there is no discontinuity in plan enrollment as a function of whether one would be older than 45 or not on September 1, 2002, or whether one is currently over or under age 45 in these years. Our empirical strategy allows us to use this subsample as an additional comparison group.

Table 2 shows summary statistics for different subsamples of the data. Column 1 shows summary statistics for both union and non-union non-faculty employees at the university. Column 2 restricts the sample to just union employees. The table shows that the mobility propensities are lower among union employees relative to non-union, and lower still among employees between the ages of 40 and 50 years old. In addition, the percent female is substantially lower among the unionized employees, while the fraction Hispanic is higher. Column 3 applies the age restriction of 40 to 50 years of age as of September 1, 2002, which is relevant for our fuzzy RD and DRD analysis. Finally, the last two columns further split this sample into the two subsets of data used in the main analysis. Column 4 includes union employees in 2002, while Column 5 includes union employees in 1999-2001. The predetermined characteristics across these two subsamples are very similar.On the other hand, the propensity to leave within one year for the 2002 subsample (Column 4) is low relative to the 1999-2001 subsample (Column 5).

4 Empirical Strategy

For our empirical strategy, we estimate the endogenous and exogenous relationship between enrollment in the DC plan and mobility based on the model outlined in Section 2 albeit with one difference. Rather than true random assignment as described in the model above, we exploit the

discontinuity in DC enrollment produced by the different default plan for employees on either side of age 45 in 2002 using a fuzzy RD and differences-in-regression-discontinuities (DRD) design described below. We conclude this section with a discussion of how these estimates map to our model and Proposition 1.

To execute our strategy, we first use OLS to estimate the effect of DC plans on employee mobility by running the following regression:

$$Leave1_i = \beta_{OLS} \cdot DC_i + f\left(A_i - 45\right) + f\left(A_i - 45\right) \cdot Under45_i + \Gamma X_i + \varepsilon_i, \tag{8}$$

where $Leave1_i$ is a binary variable that equals one if the employee is not with the firm one year later. The variable DC_i is a dummy equal to one if employee i is in a DC plan. The variable A_i denotes the employee's age on September 1, 2002. The variable $Under45_i$ is a binary variable that takes the value 1 if the employee is younger than age 45 on September 1, 2002. The flexible function $f(\cdot)$ controls for age. We use three alternative functions as follows:

$$\begin{aligned} f(x) &= 0 \\ f(x) &= ax \\ f(x) &= ax + bx^2 + cx^3. \end{aligned}$$

Finally, the vector X_i consists of demographic control variables for gender, race, hours, base salary, tenure at the firm and dummies for department.

We then turn to a fuzzy RD estimate using the discontinuity in default rules. In the first stage, we estimate the effect of the default provision on DC participation for those under 45 relative to those over 45 in 2002. In the second stage, we estimate the effect of DC participation on the one-year turnover probability, instrumenting for DC participation using the age-based policy change.

Formally, the first-stage equation is as follows:

$$DC_i = \delta \cdot DCDefault_i + f\left(A_i - 45\right) + f\left(A_i - 45\right) \cdot Under45_i + \Gamma X_i + \varepsilon_i, \tag{9}$$

where DC_i, A_i, $Under45_i$ and X_i are defined as described above, and $DCDefault_i$ is a binary variable that equals 1 if the employee is an employee under the age of 45 in 2002. The coefficient

δ is interpreted as the increase in DC enrollment from the assignment of the DC default.

The second-stage equation is estimated as:

$$Leave1_i = \beta_{RD} \cdot DC_i + f(A_i - 45) + f(A_i - 45) \cdot Under45_i + \Gamma X_i + \varepsilon_i, \tag{10}$$

where DC is instrumented for with the age-based policy as shown above. The estimate β_{RD} identifies the incentive effect of the DC plan relative to the DB plan for compliers, as the two-stage approach helps isolate the effect of enrollment patterns driven by the random variation in the assignment of the default plan on employee mobility.[12] The fuzzy RD estimate approximates the exogenous case under the assumption that, in a small window surrounding the age 45 threshold in 2002, counterfactual mobility rates evolve smoothly as a function of age (Lee and Lemieux, 2010).

Because of the availability of administrative data prior to the policy change, we can augment our analysis by combining our fuzzy RD design with elements of a difference-in-difference. We refer to this methodology as a differences-in-regression-discontinuities (DRD) design. This strategy provides an additional estimate of the incentive effect of pension plans on leave propensities. Formally, we estimate:

$$Leave1_i = \beta_{DRD} \cdot DC_i + f(A_i - 45) + f(A_i - 45) \cdot Under45_i + \tilde{\Gamma}\tilde{X}_i + \varepsilon_i, \tag{11}$$

where DC_i is instrumented for with the age-based policy as shown above. When pooling the data from 2002 with data from years prior to the policy change, both the first and second stage include indicators for the year 2002 and $Under45_i$ in $\tilde{X}_i$. The estimate β_{DRD} compares the difference in mobility between workers under and over 45 in 2002 to that same difference in 1999-2001.[13]

The DRD approach affords three advantages over employing a fuzzy RD using a single cross section. First, it mitigates the typical trade-off between bias and precision in RD designs. The use of age as the forcing variable in the first stage requires controlling for age parametrically to identify the effect of the default provision on plan enrollment. However, as will be seen, we run into problems with statistical power when estimating a smooth function of age with our sample

[12] Specifically, the two stage estimation identifies the local average treatment effect (LATE) among "compliers", whose choice in retirement plan is driven by the default policy (Imbens and Angrist, 1994).

[13] As we explain later, we will use both a cohort-based analysis, where A_i is age in the year 2002, and an age-based analysis, where A_i is age at time of observation.

size. With a cross section of data, one may increase sample size by widening the window about the discontinuity. However, doing so may weaken the assumption that individuals above and below the cutoff are otherwise similar. Our additional years of data afford us the ability to increase our sample size while holding the size of our window constant, thus increasing the efficiency of our estimate. These additional observations contribute to our estimate of the smooth function of the forcing variable, increasing our precision. This refinement is not completely free: we must impose an additional assumption that the relationship between leave probability and age is similar in earlier years.[14]

Second, the additional data allow us to control for flexible functions of age. While the assumptions underlying a fuzzy RD estimator would allow us to estimate everything using just a single cross section, certain methods of controlling or age, such as age dummies, are not possible. However, the inclusion of pre-transition data allow us to add this fourth, nonparametric functional form for our control function in age:[15]

$$f(x) = \sum_{i=-k/2}^{k/2} \gamma_i \cdot \mathbf{1}(x = i),$$

where k is the size of the bandwidth around age 45 to which the analysis is restricted.

Finally, DRD estimation can be used in situations where the relationship between unobservable determinants of leaving and age is not continuous at the threshold, perhaps due to other polices that discontinuously change. In our context, this is not a concern as there are no other policies that involve the age 45 threshold. Instead, we can use the pre-policy data to evaluate the validity of our assumption that there is no discontinuous change in unobservable determinants of leaving at age 45 in the absence of the policy.

Our estimates provide proxies for the relationship between mobility and DC enrollment in the endogenous case (OLS) and the exogenous case (fuzzy RD and DRD) laid out in Section 2.[16] The OLS estimates that compare mobility rates among DC participants and DB participants are driven

[14] We visually inspect the validity of this assumption below.

[15] When we control non-parametrically for age using age dummies, the estimation technique becomes essentially a difference-in-difference estimator in a narrow window around the policy change.

[16] The discontinuity in the default plan by age allows us to identify a local average treatment effect among compliers, i.e. individuals who enroll in the plan that is their default plan, whether it be the new benefit or the old benefit. In the context of our model in Section 2, these are employees for whom $|\phi_i| \leq c_i$. While these employees may not represent the average employee, we can use the same conditions developed earlier to sign the selection effect.

by both the incentive effect and the selection effect. These two forces can, in general, lead to an ambiguous relationship between mobility rates across the two types of plans because the selection effect could reinforce or counteract the incentive effect. Importantly, by Proposition 1, we can rule out both a negative selection effect and no selection effect if we can reject that the OLS estimate is less than the RD estimate.

Recall that Table 1 summarizes the relationship between the relative magnitude of β_{OLS} ($\approx \beta_{Endo}$) and β_{RD} ($\approx \beta_{Exog}$) and the implied sign of the selection effect generated from our model. Here β_{RD} refers either to the fuzzy RD estimate generated from the 2002 data or to the DRD estimate generated from the pooled 1999-2002 data. In order to test for positive selection, we report the results of a test of the null hypothesis that $\beta_{OLS} \leq \beta_{RD}$. We also report first stage results and the results of reduced-form regressions, which replace DC_i in Equations 10 or 11 with $DCDefault_i$.

5 Results

In this section, we first graphically examine the conditions required for a regression discontinuity analysis in our context. Second, we show our main regression results for one-year leave probabilities, followed by analysis using a two- and three-year window. Finally, we present supplemental analyses, including placebo discontinuities and an analysis of the control groups used in our DRD estimator.

5.1 Graphical Analysis

Figure 1 shows the DC plan enrollment rate by one-year age bins in 2002. The figure shows a large discontinuity in enrollment at age 45, which is the causal effect of the default policy on plan enrollment. We next investigate the assumptions needed for a regression discontinuity approach. Figure 2 confirms that the age distribution is smooth across the age 45 cut-off for the 2002 subsample, while Figure 3 shows that observable characteristics do not vary discontinuously at age 45.

The upper panel of Figure 4 depicts the probability of leaving within one year for one-year age bins for employees at the firm in 2002. This graph shows some evidence of a discontinuity in mobility rates on either side of the age 45 cutoff. However, the figure also highlights some difficulties in estimating the effect of pension plans on mobility with these data. Due to limitations in sample

size, there is considerable noise in the mobility rates by age, particularly within a small window surrounding the age 45 threshold.

As discussed previously, the issue of precision can be surmounted by augmenting our analysis with data from the 1999-2001 period, when these workers were not affected by the policy. Namely, we can compare the discontinuity in mobility rates at age 45 in 2002 to the discontinuity in mobility rates for these same workers in prior years (i.e. based on date of birth or "cohort"). The bottom panel of Figure 4 displays the leave probabilities by age in 2002 for both the 2002 and the 1999-2001 subsamples. By showing both plots in the same figure, a clearer mobility pattern emerges in that the 2002 plot is shifted down relative to the 1999-2001 plot for those younger than 45 in 2002, yet the two plots are nearly identical for ages 45 and older.[17]

To better facilitate this comparison, in Figure 5, we show the difference in employee mobility between employees in 2002 and the same cohort of employees in 1999-2001 by one-year bins based on age in 2002. This figure shows evidence of a discontinuity in one-year leave probabilities across the age threshold. The probability of leaving within one year is lower for the younger employees relative to the older employees, suggesting that the DC plan is associated with lower one-year turnover probabilities relative to the DB plan.[18]

Alternatively, we can use the 1999-2001 data to compare the discontinuity in mobility rates at age 45 in 2002 to the discontinuity in mobility rates at age 45 in prior years (i.e. fixed age). While we omit the graphical results for ease of exposition, our empirical results report DRD results for both cohort and age.

These figures give suggestive evidence that employee mobility is related to pension plan enrollment. The following section formalizes the graphical evidence using the regression framework outlined previously in Section 4.

5.2 One-Year Leave Probabilities

We begin by estimating the effect of DC plan enrollment on the probability of leaving the firm within the next calendar year using a fuzzy RD approach. Table 3 reports the results for the sample of

[17] The fact that these curves line up so well to the right of age 45 supports our notion that the pre-transition data provides an accurate counterfactual for the 2002 employees.

[18] Again, the difference is nearly zero for ages above 45 in 2002, suggesting that the pre-transition employees are an appropriate group to use for estimating the baseline relationship between leaving and age.

employees affected by the policy change in 2002. We report results for a sample consisting of observations within a 5-year window around the age of 45.[19] Coefficients from the OLS regression in Equation 8 are reported in the first row. The second row contains the results from the fuzzy RD regression described in Equation 10, and the third row provides the p-values of the test of the null hypothesis that $\beta_{OLS} \leq \beta_{RD}$. Recall that this is the key inequality from Proposition 1. The table also includes first stage and reduced form regression results, the mean one-year leave probability within this sample, and the F-statistic from the first stage regression.

The OLS estimate of the correlation between DC plan enrollment and leaving the firm is not significantly different from zero. The fuzzy RD estimates indicate a negative, though typically insignificant effect of DC enrollment on employee mobility. As we add controls for the running variable (age) we lose a significant amount of power, given our low sample size. This result is demonstrated most readily by moving across the first three columns and comparing the first stage regressions. The F-statistics from our first stage imply that simultaneously estimating a smooth function in age and a jump at age 45 is asking a lot of the data. While the fuzzy RD estimates are always more negative than the OLS estimates, suggesting a positive selection of high-mobility employees into the DC plan, the p-values from our hypothesis test do not consistently allow us to reject the null hypothesis that the OLS estimates are less than the fuzzy RD estimates. Overall, the lack of power in these fuzzy RD estimates mirrors our earlier discussion of Figure 4. We therefore turn to our DRD analysis for increased precision.

We report results from our DRD estimation in Table 4, which mirrors Table 3 in format except that it contains one additional column where age is controlled for non-parametrically. Here, β_{DRD} reflects the difference in mobility rates for those near age 45 in 2002 relative to the same employees in prior years. We again see very little evidence of a correlation between employee mobility and DC plan enrollment in the first row of OLS regression results. However, the DRD estimates are now robust to alternative methods of controlling for the running variable. The p-values from our hypothesis test now consistently allow us to reject the null hypothesis that $\beta_{OLS} \leq \beta_{RD}$ at the 5 percent level. Thus, based on Proposition 1, we conclude that the selection effect is positive, i.e. mobility tendencies are positively related to preferences for the DC plan. We also demonstrate a strong and robust first-stage relationship, which suggests that those just below the age 45 threshold

[19] Results assuming a 10-year window are provided in Appendix B.2.

are about 50 percentage points more likely to enroll in the DC plan.

Table 5 presents results where β_{DRD} reflects the difference in mobility rates for those near age 45 in 2002 relative to those near age 45 in prior years. We find the same pattern of results in that β_{OLS} is greater than β_{DRD} or a positive selection effect, although the finding is now only marginally significant with a p-value of approximately 0.07.

One may be concerned that these results are driven by employees who are vested in the DC plan but not vested in the DB plan due to differences in vesting requirements. When we restrict the analysis to employees vested in both plans (i.e. at least 5 years of service), the positive selection effect remains. This suggests that the multi-dimensional difference between the two plans contributes to the positive relationship between mobility tendencies and preferences for the DC plan rather than differences in vesting alone.

While our focus has been on identifying the role of selection, our results also provide estimates of the incentive effect, which measures the direct effect of the DC plan on mobility relative to the DB plan. The results suggest switching an employee from the DB plan to the DC plan reduces the probability of leaving the firm within the next year by approximately six to ten percentage points depending on which comparison group is used (Table 5 vs Table 4). While conventional wisdom suggests that DC plans ought to increase mobility, due to greater portability, our results suggest that other attributes of the benefit generally make this DC plan more attractive than the DB and increase the likelihood that one remains with the firm in a way that dominates portability.

The magnitude of the incentive effect is large relative to the average leave probability among the sample, which is between four and five percent. One possible explanation is that those induced to enroll in the DC plan due to the default policy (i.e. "compliers") are systematically different from the average employee in our sample. We use a method similar to Autor and Houseman (2005) to estimate the characteristics of the marginal DC enrollee via a DRD and compare average characteristics to the sample mean. The only difference we observe is that compliers are more likely to be White and less likely to be of Hispanic origin relative to the sample mean. These results are provided in Appendix B.1. Another possibility is that these effects only represent intertemporal retiming of behaivior. In Section 5.4, we discuss in the context of longer run effects whether this estimate is more akin to a permanent reduction in mobility or a short-run delay in leaving the firm.

The fact that we find a positive selection effect and a negative incentive effect may appear

counterintuitive, but is readily interpretable in the context of our conceptual model. Consider a simple case with two types of workers, indexed by $j \in \{1, 2\}$ and a constant enrollment cost of c. Assume $\phi_1 > c > \phi_2 > 0$, so that both types prefer the DC plan, but only type 1 enrolls voluntarily. Furthermore, assume the mobility parameter for individual i of type j is $m_{ij} = m_j - \eta - \epsilon_{ij}$, where $m_1 > m_2$ and $\epsilon_{ij} \underset{\text{i.i.d.}}{\sim} F(\cdot)$. In this case we have positive selection, i.e. those who voluntarily choose the DC plan are also generally more likely to leave the firm, even in the absence of the DC plan. Formally, $1 - F(\eta - m_1) > 1 - F(\eta - m_2)$. Nonetheless, the DC plan has a negative incentive effect on the probability of leaving, i.e. $1 - F(\eta + \phi_j - m_j) < 1 - F(\eta - m_j)$. In particular, when the DC plan default removes the enrollment cost for type 2 workers, they enjoy a positive benefit relative to the DB plan, implying higher retention. Finally, the result is robust to a direct effect of DC enrollment on mobility, e.g. $\eta = \overline{\eta} - B_i \triangle\eta$, so long as $\phi_2 > \triangle\eta$.[20]

We can similarly illustrate this example as follows. Suppose that the DC plan is equivalent to the DB plan in terms of retirement wealth and risk, but also allows for low penalty withdrawal pre-retirement. However, assume that this additional feature of the DC plan requires effort to discover. If higher ability workers are more likely to discover this feature, they may be more likely to enroll in the DC plan. Furthermore, assume that higher ability workers tend to receive more frequent outside offers, and, therefore, are more likely to leave the firm. In this case, we have positive selection, in that those who choose the DC plan are also more likely to leave the firm. Finally, assume that once you are enrolled in the DC plan, you costlessly learn about all of its features. Now, randomly enrolling a worker in the DC plan will, on average, reveal the higher value of the benefit, and therefore, reduce turnover, all things equal. Again, this example is consistent with simultaneously finding positive selection and a negative incentive effect.

Therefore, while behavioral economic explanations may be possible, e.g. overestimation of the enrollment cost $\hat{c} > c$, they are not required to explain the pattern of our findings. It is important to note that our findings cannot merely be explained by a tendency for compliers to be complacent or inert because our estimation uses employees who were affected by the default on both sides of the age 45 threshold. Any such explanation would need to highlight why compliers under age 45

[20] Because m involves two components, the value of the outside employment option relative to the current firm and employer switching costs (η), this result places no restriction on the magnitude of the enrollment cost c relative to the switching cost η. However, in the case where the DC plan directly affects the switching cost, our assumptions do imply that c is greater than the *difference* in switching costs between the DC and DB plan, $\triangle\eta$.

are systematically different from compliers who are age 45 or above in a narrow window around this threshold.

5.3 Robustness Checks

In this section, we evaluate the robustness of our results by conducting falsification tests using alternative age thresholds in 2002. We furthermore evaluate the validity of our comparison groups in the DRD analyses by testing for discontinuities in mobility in the pre-period.

5.3.1 Placebo Discontinuities for Plan Enrollment

Our estimates rely on the assumption that those employees just below age 45 are otherwise comparable to those employees above age 45. One advantage of regression discontinuity designs is that specification checks that help to test the validity of our identification are readily available (Lee and Lemieux, 2010). In particular, we can redo our analysis at placebo discontinuities, where we know there is no sharp change in the DC enrollment rate. Our identifying assumptions imply that we should find no discontinuous change in our outcome variables at these alternative thresholds. To employ this method, we conduct our analysis with ages 42.5 and 47.5 as our placebo thresholds. In both cases, we examine the sample in 5-year windows surrounding these ages: ages 40 to 45 and ages 45 to 50, respectively. Within these samples, all employees receive the same retirement plan default.

Table 6 contains the reduced form results for one-year leave probabilities at these alternative thresholds as well as the first-stage F-statistic using the 2002 sample and pooled 1999-2002 sample used for the two DRD analyses. As expected, the F-statistics from the first stage are very small given there is no discontinuity in plan enrollment at these placebo thresholds. We therefore focus on the reduced form estimates at these alternative thresholds. They are similarly noisy and almost always indistinguishable from zero. Thus, in order for our results to be confounded by age patterns, there has to be an unobservable difference in underlying employee mobility between those just above and below age 45 that quickly vanishes when comparing those just above and under either age 42.5 or 47.5.

5.3.2 Placebo Discontinuities in 1999-2001

Our DRD approach compares differences in leave propensities for workers on either side of age 45 in 2002 relative to differences in leave propensities for the same cohorts in 1999 to 2001 and relative to differences in leave probabilities around the age 45 cutoff in 1999 to 2001. Therefore, the difference in leave probabilities in 1999-2001 is the counterfactual we use in our analysis. This group, which was not affected by our policy, should not exhibit any jumps in leave probabilities around the relevant threshold if our assumption that there is no discontinuous change in unobservable determinants of leaving at age 45 in the absence of the policy is correct.

In Table 7 we assess this condition by estimating the reduced form discontinuity in one-year leave probabilities in each of the years 1999-2001. The top panel estimates the discontinuity for the same cohorts that were subject to the differential default policy in 2002, while the bottom panel estimates the discontinuity at the fixed age 45 threshold in prior years. In each case, we do not find evidence of a discontinuity nor do we find any trend. In the final column, we estimate the average difference pooling the years 1999 to 2001, which represents the counterfactuals we use when pooling the data among employees in 1999-2002 in Tables 4 and 5. Here, we find no statistically significant evidence that turnover probabilities differ at the threshold age in the absence of the policy, which suggests that the 1999-2001 employees serve as an appropriate control group.

5.4 Two- and Three-Year Leave Probabilities

We extend the analysis from the previous section to longer-run turnover outcomes, in particular, the likelihood of leaving the firm within two or three years. Table 8 reports fuzzy RD estimates for the 2002 sample and results from the two DRD analyses for various samples and specifications. Our sample is slightly smaller than that used in our main set of estimates in order to eliminate employees in the pre-period whose two- or three-year horizons cross the 2002 introduction of the new pension plan. We have omitted results from the first stage because, other than the difference in sample size, they remain identical to those contained in the previous tables.

The first three columns of Table 8 show the results from the fuzzy RD, which are sensitive to functional form as we saw in the one-year outcome. Columns (4) to (6) report the DRD estimates when using age in 2002 as the forcing variable. The point estimates show a slightly more pronounced

effect of DC enrollment on turnover. Enrollment in the DC plan generates nearly a 17 percentage point reduction in the probability of leaving the firm within two calendar years, relative to a baseline of 9 percent. When using the comparison across age 45 in prior years, the results are similar, but lower in magnitude and thus not significant at conventional levels. This pattern suggests that there is an additional reduction in employees leaving in the second year, albeit a smaller effect as a proportion of the baseline leave probability.[21]

Looking at results of pension plan type on mobility over a three-year horizon, we see that there is still a negative effect of DC plans on turnover probabilities, though the effect is not consistently significant. In fact, the magnitude of the three-year turnover effect is less than the effect on two-year turnover. There are at least two ways to interpret this result. First, the fact that the three-year effect is less significant and sometimes smaller than the two-year effect may suggest that the leave probabilities are beginning to converge between those just over and just under 45 years of age. This relationship may be indicative of a short-run effect of the policy that eventually fades. In other words, employees who were defaulted into the DC plan initially stay with the firm some time longer, but ultimately leave the firm anyway. This finding would suggest that the large magnitude of our one-year results may be due to the fact that intertemporal adjustments tend to be much larger than permanent behavioral responses. Second, this finding may be due to the smaller sample and reduced precision resulting from measuring a three-year leave probability and therefore having to drop more observations who span the the pre- and post-periods.

6 Conclusion

The effect of a widespread transition in the employer-provided pension plan landscape from DB to DC plans on employee mobility has been a subject of interest among policymakers and academics because of the large number of firms and employees affected. Since DB pension wealth is typically tied more closely to tenure as compared to DC plans, conventional wisdom supports the idea that DC plans will induce higher mobility. However, this conclusion is complicated by the potential role of selection into employers and plan offerings by employees with differing underlying mobility tendencies. The effect of plan type on mobility is further confounded by the multi-dimensional

[21] Theoretically, the effect would eventually have to slow down, since it is bounded below by negative one.

difference between DB and DC plans, including features, such as individual control, liquidity, and transparency, that may make DC plans desirable enough to increase retention at firms with these plans.

In this paper, we exploit a natural experiment that created random variation in pension plan enrollment, in order to study the effects of pension plan type on employee mobility. Our identification strategy combines elements of a difference-in-difference and a fuzzy RD estimator, a DRD estimation approach, relying on weaker assumptions relative to the previous literature. We develop an empirical model that helps us interpret the results from our analysis in the context of separate, and possibly countervailing, incentive and selection effects. This framework provides predictions regarding the different effects of endogenous and exogenous pension plan enrollment as they relate to the role of selection on mobility tendencies. Our empirical results combined with insights from our model indicate that preferences for DC plans are positively related to unobservable mobility tendencies.

While the natural experiment we examine provides plausibly exogenous variation, extrapolating from a single employer warrants caution. However, our findings have some implications for mobility and the transition from DC to DB plans more generally. First, our results provide evidence of positive selection into DC plans based on mobility tendencies, implying that at least part of the relationship between the transition and increased job mobility is due to selection, and not fully caused by differences in portability or accrual patterns across plan type. Second, because the transition we examine takes place within an employer among a set of covered workers, we can rule out the possibility that the differences in mobility we find are driven by compensation premiums, which have been used to explain a potentially large part of the mobility differences between covered and uncovered workers (Gustman and Steinmeier, 1993).

Third, we find evidence that, counter to conventional wisdom, DC plans may reduce mobility relative to DB plans. Thus we should not simply characterize the difference in plan features between DB and DC plans in terms of portability and accrual; rather, the differences are multi-dimensional, including differences in risk exposure, liquidity, and transparency, for example. Finally, we find that the incentive and selection effects work in opposite directions in our context. This has implications for choice architecture in that the presence of nontrivial transaction costs of switching from the DB to the DC benefit ensures that only those who value the DC plan the most will switch into it. For

these individuals, the relatively high value they place on the DC plan will tend to offset certain features of the DC plan, such as a shorter vesting period, that may generate higher turnover. For an employer contemplating a transition from a DB to a DC plan, the trade-off between higher DC enrollment and lower turnover may be mitigated by transaction costs, implying that the optimal level of switching costs may be nonzero.

References

Allen, Steven G., Robert L. Clark, and Ann A. McDermed. 1993. "Pensions, Bonding, and Lifetime Jobs." *Journal of Human Resources*, 28(3): 463–481.

Andrietti, Vincenzo, and Vincent Hilderband. 2004. "Evaluating Pension Portability Reforms: The Tax Reform Act of 1986 as a Natural Experiment." *Working Paper 04-52.*

Autor, David H., and Susan Houseman. 2005. "Do Temporary Help Jobs Improve Labor Market Outcomes for Low-Skilled Workers? Evidence from 'Work First'." *NBER Working Paper 11743.*

Brown, Jeffrey R., Arie Kapteyn, Erzo F. P. Luttmer, and Olivia S. Mitchell. 2011. "Do Consumers Know How to Value Annuities? Complexity as a Barrier to Annuitization." *Working Paper WR-924-SSA.*

Buessing, Marric, and Mauricio Soto. 2006. "Getting to the Top of Mind: How Reminders Increase Saving." Center for Retirement Research at Boston College.

Choi, James J, David Laibson, Brigitte C Madrian, and Andrew Metrick. 2004. "For Better or for Worse: Default Effects and 401(k) Savings Behavior." In *Perspectives on the Economics of Aging.* , ed. David A. Wise, 81–126. University of Chicago Press.

Cronqvist, Henrik, and Richard H Thaler. 2004. "Design Choices in Privatized Social-Security Systems: Learning from the Swedish Experience." *American Economic Review*, 94(2): 424–428.

Disney, Richard, and Carl Emmerson. 2004. "Choice of Pension Plan and Job Mobility in Britain." *Discussion Paper 04-04.*

Farber, Henry S. 2007. "Is the Company Man an Anachronism? Trends in Long Term Employment in the U.S., 1973-2006." *Working Paper No. 518.*

Fitzpatrick, Maria D. 2011. "How Much do Public School Teachers Value Their Retirement Benefits?" *Working Paper.*

Goda, Gopi Shah, and Colleen Flaherty Manchester. 2013. "Incorporating Employee Heterogeneity into Default Options for Retirement Plan Selection." *Journal of Human Resources*, 48(1): 200–237.

Gustman, Alan L., and Thomas L. Steinmeier. 1993. "Pension Portability and Labor Mobility: Evidence from the Survey of Income and Program Participation." *Journal of Public Economics*, 50: 299–323.

Imbens, G W, and J D Angrist. 1994. "Identification and Estimation of Local Averate Treatment Effects." *Econometrica*, 62(3): 467–475.

Ippolito, Richard A. 2002. "Stayers as "Workers" and "Savers": Toward Reconciling the Pension-Quit Literature." *Journal of Human Resources*, 37(2): 275–308.

Lazear, Edward P. 1990. "Pensions and Deferred Benefits as Strategic Compensation." *Industral Relations*, 29(2): 263–281.

Lee, David S, and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature*, 48(2): 281–355.

Madrian, Brigitte C, and Dennis F Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *The Quarterly Journal of Economics*, 116(4): 1149–1187.

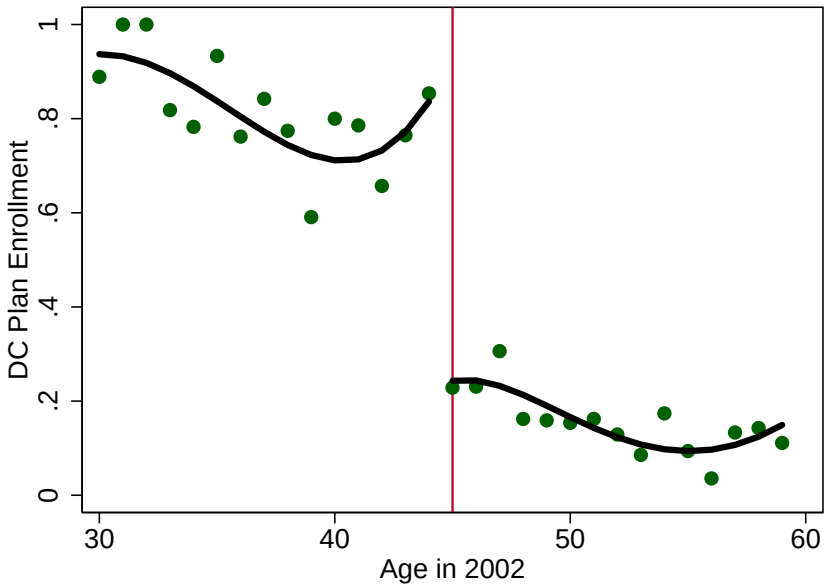
Manchester, Colleen Flaherty. 2010. "The Effect of Pension Plan Type on Retirement Age: Distinguishing Plan Incentives from Career Length Preferences." *Southern Economic Journal*, 77(1): 104–125.

Mitchell, Olivia S. 1982. "Fringe Benefits and Labor Mobility." *Journal of Human Resources*, 17(2): 286–298.

Munnell, Alicia H., Kelly Haverstick, and Geoffrey Sanzenbacher. 2006. "Job Tenure and Pension Coverage." *Working Paper 2006-18*.

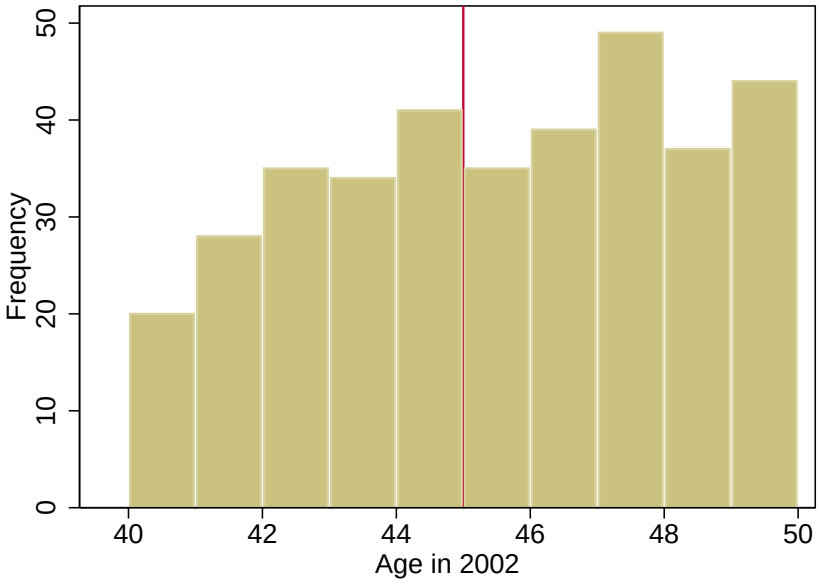
Rabe, Birgitta. 2007. "Occupational Pension, Wages, and Job Mobility in Germany." *Scottish Journal of Political Economy*, 54(4): 531–552.

Figure 1: DC Plan Enrollment by Age in 2002



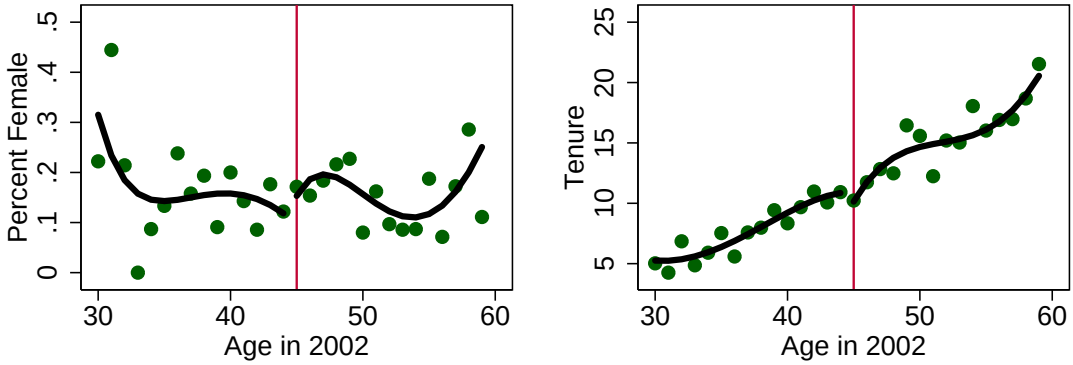
Notes: Dots represent DC enrollment rate for one-year age bins.

Figure 2: Distribution of Employee Age for Employees in 2002



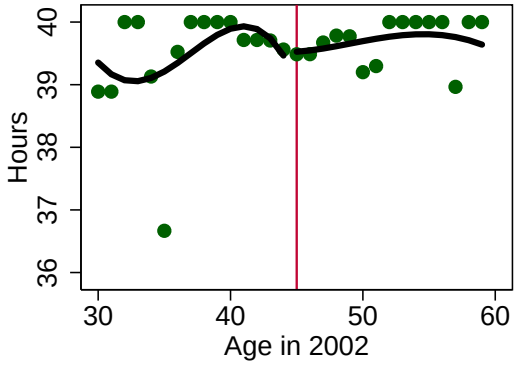
Notes: Histogram of employee age as of September 1, 2002 using one-year bins.

Figure 3: Average Value of Covariates by Age in 2002

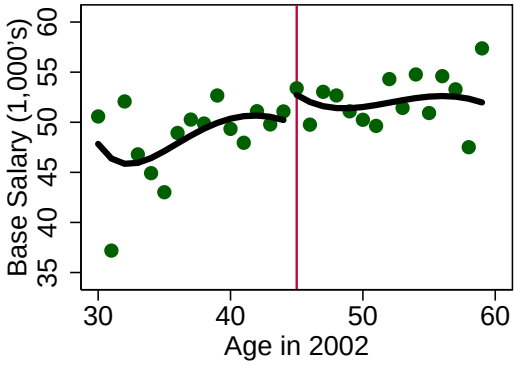


(a) Proportion Female

(b) Tenure at Firm



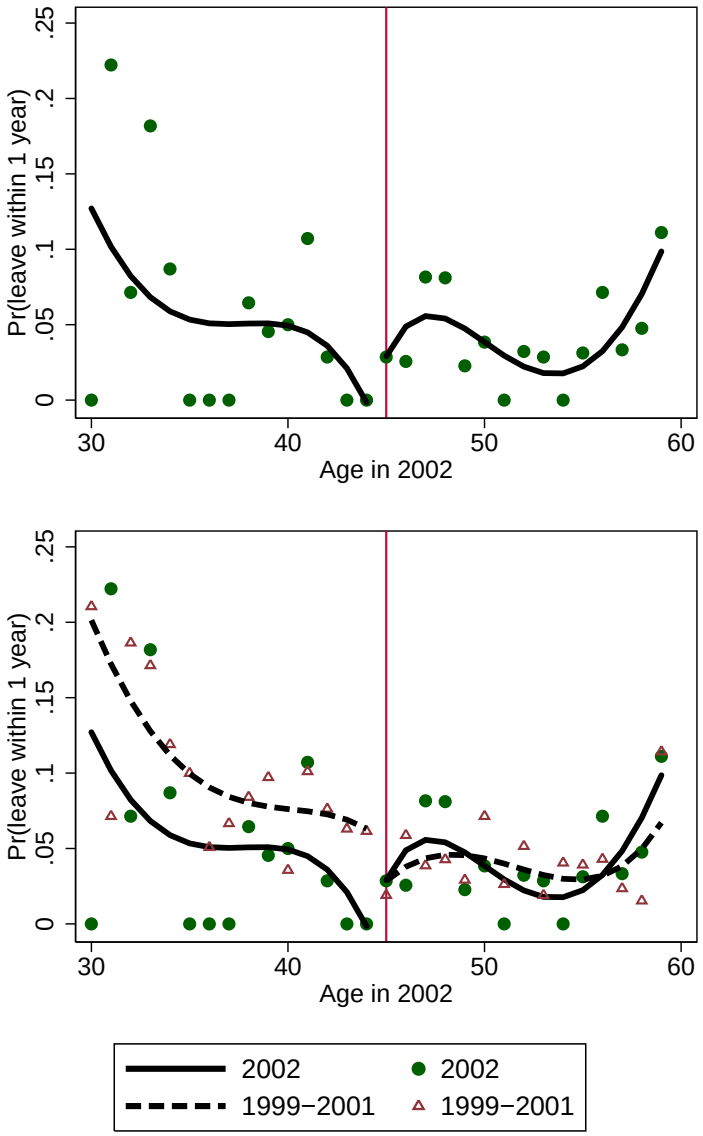
(c) Average Hours per Week



(d) Average Annual Salary

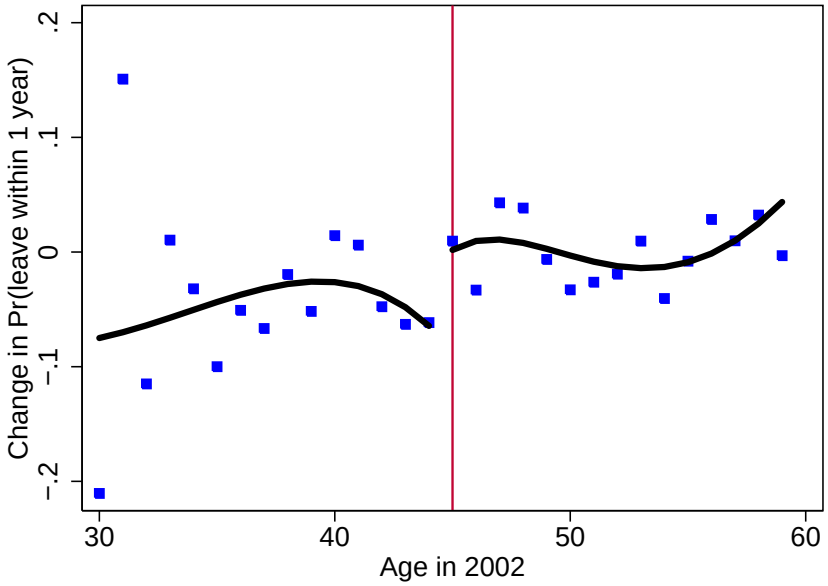
Notes: Panels used to verify no discontinuity at age 45 for other observable characteristics; vertical line marks age 45.

Figure 4: Probability of Leaving within One Year: 1999-2002



Notes: The top panel plots the average leave probability for one-year age bins for the 2002 sample, while the bottom panel separately plots these probabilities for the 2002 and 1999-2001 samples.

Figure 5: Probability of Leaving within One Year: Difference between 2002 and 1999-2001



Notes: Markers plot the difference in average leave probability by one-year age bins for the year 2002 relative to years 1999-2001.

Table 1: Implications of Selection Effect for Empirical Estimates

Empirical Observation	No Selection Effect	Negative Selection Effect	Positive Selection Effect
$\beta_{Endo} \leq \beta_{Exog}$	X	X	X
$\beta_{Endo} > \beta_{Exog}$			X

Note: Selection refers to the relationship between mobility tendencies and preferences for the new benefit, defined by Equation 5, while β_{Endo} and β_{Exog} are defined by Equations 4 and 6. In practice, we estimate an OLS coefficient $\beta_{OLS} \approx \beta_{Endo}$ and an RD (DRD) effect $\beta_{RD} \approx \beta_{Exog}$

Table 2: Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)
	Union & Non-Union All Ages 1999 to 2002	Union All Ages 1999 to 2002	Union 40 to 50 1999 to 2002	Union 40 to 50 2002	Union 40 to 50 1999 to 2001
Leave within 1 year	0.138	0.070	0.049	0.041	0.052
	(0.345)	(0.256)	(0.217)	(0.200)	(0.222)
Age on Sept. 1, 2002	42.89	47.34	45.49	45.49	45.49
	(12.14)	(10.88)	(2.732)	(2.741)	(2.731)
Female = 1	0.504	0.161	0.167	0.169	0.167
	(0.500)	(0.368)	(0.373)	(0.375)	(0.373)
Black	0.0941	0.106	0.107	0.105	0.107
	(0.292)	(0.308)	(0.309)	(0.307)	(0.310)
Hispanic	0.181	0.280	0.296	0.298	0.296
	(0.385)	(0.449)	(0.457)	(0.458)	(0.456)
Other Non-White	0.193	0.157	0.143	0.144	0.142
	(0.395)	(0.364)	(0.350)	(0.351)	(0.350)
Weekly hours	38.57	39.55	39.71	39.67	39.72
	(4.576)	(2.651)	(2.257)	(1.857)	(2.371)
Salary	$41,414	$46,573	$47,597	$51,133	$46,472
	(11914.8)	(12999.3)	(12534.7)	(12961.0)	(12188.2)
N	8,981	4,217	14,99	362	1,137

Notes: Sample mean listed above; standard deviation in parentheses

Table 3: OLS and Fuzzy RD Estimates of Effect of DC Plan on One-Year Leave Probability, 2002

	(1)	(2)	(3)
β_{OLS}	0.017	0.046	0.048
	(0.022)	(0.030)	(0.039)
β_{RD}	-0.114**	-0.040	-0.149
	(0.057)	(0.081)	(0.112)
H_0: $\beta_{OLS} \leq \beta_{RD}$	0.027	0.189	0.042
First Stage	0.455***	0.612***	0.623***
	(0.076)	(0.142)	(0.236)
Reduced Form	-0.052**	-0.025	-0.093
	(0.026)	(0.055)	(0.074)
$\mathbb{E}[Leave_i]$	0.026	0.026	0.026
Bandwidth	5	5	5
$f(Age)$	None	Linear	Cubic
N	196	196	196
First Stage F-stat	35.5	18.5	6.98

Note: Sample includes employees in the year 2002. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 (i.e. "Treatment" is DC plan default). P-value for H_0 reported for evaluating implication of Proposition 1. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 4: OLS and DRD Estimates of Effect of DC Plan on One-Year Leave Probability, 1999-2002, Cohort Comparison

	(1)	(2)	(3)	(4)
β_{OLS}	0.015	0.014	0.013	0.015
	(0.027)	(0.027)	(0.027)	(0.027)
β_{DRD}	-0.108**	-0.109**	-0.107**	-0.109**
	(0.054)	(0.054)	(0.053)	(0.054)
H_0: $\beta_{OLS} \leq \beta_{DRD}$	0.024	0.025	0.027	0.024
First Stage	0.519***	0.518***	0.519***	0.518***
	(0.062)	(0.062)	(0.062)	(0.062)
Reduced Form	-0.056**	-0.056**	-0.056**	-0.056**
	(0.027)	(0.027)	(0.027)	(0.027)
$\mathbb{E}[Leave_i]$	0.045	0.045	0.045	0.045
Bandwidth	5	5	5	5
$f(Age)$	None	Linear	Cubic	Non-Par
N	815	815	815	815
First Stage F-stat	70	69.8	69.6	69.6

Note: Sample includes employees in the years 1999-2002. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 in 2002 (i.e. "Treatment" is DC plan default). Comparison group consists of same cohorts of workers in 1999-2001. P-value for H_0 reported for evaluating implication of Proposition 1. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 5: OLS and DRD Estimates of Effect of DC Plan on One-Year Leave Probability, 1999-2002, Age 45 Comparison

	(1)	(2)	(3)	(4)
β_{OLS}	0.024	0.025	0.025	0.025
	(0.026)	(0.026)	(0.026)	(0.026)
β_{DRD}	-0.067	-0.064	-0.067	-0.062
	(0.054)	(0.053)	(0.054)	(0.052)
H_0: $\beta_{OLS} \leq \beta_{DRD}$	0.069	0.073	0.069	0.076
First Stage	0.513***	0.514***	0.515***	0.514***
	(0.062)	(0.061)	(0.061)	(0.061)
Reduced Form	-0.034	-0.033	-0.034	-0.032
	(0.028)	(0.028)	(0.028)	(0.027)
$\mathbb{E}[Leave_i]$	0.039	0.039	0.039	0.039
Bandwidth	5	5	5	5
$f(Age)$	None	Linear	Cubic	Non-Par
N	818	818	818	818
First Stage F-stat	68.9	70.4	70.6	70.3

Note: Sample includes employees in the years 1999 - 2002. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 (i.e. "Treatment" is DC plan default). Comparison group consists of workers around age 45 threshold in 1999-2001. P-value for H_0 reported for evaluating implication of Proposition 1. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 6: Placebo Effect – Reduced Form Estimates of Alternative Age Thresholds on One-Year Leave Probability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Placebo Threshold: 42.5				Placebo Threshold: 47.5			
2002	0.085**	0.106	0.085		-0.041	-0.066	0.047	
	(0.039)	(0.070)	(0.132)		(0.038)	(0.069)	(0.089)	
First Stage F-stat	0.33	0.07	1.68		0.71	0.01	1.38	
N	158	158	158		204	204	204	
1999-2002: Cohort Control	0.052	0.054	0.052	0.055	-0.014	-0.014	-0.014	-0.014
	(0.041)	(0.041)	(0.041)	(0.041)	(0.034)	(0.034)	(0.035)	(0.035)
First Stage F-stat	0.03	0.03	0.05	0.03	0.57	0.58	0.55	0.58
N	661	661	661	661	837	837	837	837
1999-2002: Age Control	0.044	0.041	0.041	0.043	-0.031	-0.035	-0.034	-0.037
	(0.039)	(0.038)	(0.039)	(0.039)	(0.035)	(0.035)	(0.035)	(0.034)
First Stage F-stat	0.09	0.07	0.09	0.01	0.55	0.40	0.44	0.41
N	736	736	736	736	796	796	796	796
Bandwidth	5	5	5	5	5	5	5	5
$f(Age)$	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par

Note: Reduced form results reported for 2002 and 1999-2002 samples. DC is instrumented for using the placebo thresholds of 42.5 or 47.5 in 2002 and F-statistic from first stage is reported. Fuzzy RD and DRD estimates not reported due to weak first stage. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 7: Placebo Effect: Reduced Form Estimates of Age 45 Threshold on One-Year Leave Probability in Pre-Policy Years

	(1)	(2)	(3)	(4)
	1999	2000	2001	1999-2001
Cohort Control	-0.017	0.016	-0.007	-0.002
	(0.036)	(0.045)	(0.040)	(0.032)
$N = 622$				
Age Control	0.001	-0.020	-0.009	-0.010
	(0.043)	(0.046)	(0.040)	(0.036)
$N = 622$				
Bandwidth	5	5	5	5

Note: Sample includes employees in the years 1999 - 2001. Cohort Control reports effect of age 45 in 2002 threshold on mobility in prior years, while Age Control reports the effect of a fixed age 45 threshold on mobility in prior years (estimated jointly across years). Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 8: RD and DRD Estimates of Effect of DC Plan on Two- and Three-Year Leave Probabilities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2002			1999-2002: Cohort Control				1999-2002: Age Control			
2 Year	-0.103	0.019	0.113	-0.167*	-0.168*	-0.165*	-0.167*	-0.122	-0.123	-0.130	-0.120
	(0.069)	(0.106)	(0.222)	(0.087)	(0.087)	(0.086)	(0.086)	(0.086)	(0.088)	(0.087)	(0.087)
$\mathbb{E}[Leave_i]$	0.046	0.046	0.046	0.088	0.088	0.088	0.088	0.077	0.077	0.077	0.077
N	196	196	196	611	611	611	611	614	614	614	614
3 Year	-0.080	-0.036	0.176	-0.141	-0.141	-0.139	-0.140	-0.039	-0.032	-0.025	-0.009
	(0.089)	(0.136)	(0.290)	(0.114)	(0.115)	(0.113)	(0.114)	(0.126)	(0.126)	(0.121)	(0.125)
$\mathbb{E}[Leave_i]$	0.087	0.087	0.087	0.113	0.113	0.113	0.113	0.101	0.101	0.101	0.101
N	196	196	196	397	397	397	397	398	398	398	398
Bandwidth	5	5	5	5	5	5	5	5	5	5	5
$f(Age)$	None	Linear	Cubic	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par

Note: Sample includes employees in 2002 and 1999-2002 samples. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 in 2002 (i.e. "Treatment" is DC plan default). Exogenous, or incentive, effect of DC plan on two- and three-year turnover outcomes is reported for fuzzy RD (columns 1 to 3) and two DRD analyses: by cohort (columns 4 to 6) and by age (columns 7 to 9). Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Appendix A: Model Discussion

A.1 Proof of Propositions 1

Before proving Proposition 1, we establish a useful lemma:

Lemma 1 (Selection and Observational Correlations)**.** *The observed difference in leave probabilities by benefit type (B_i) defined in (4) is bounded from above by the selection effect defined in (5). That is:*

$$\beta_{Endo} \leq \beta_{Selection} \tag{A.1}$$

The implication of Lemma 1 is that if we observe a positive correlation between the probability of the leaving the firm and endogenous enrollment in the new benefit (i.e. $\beta_{Endo} > 0$), then we can sign the selection effect as positive (i.e. $\beta_{Selection} > 0$). This result is asymmetric, in that a negative correlation (i.e. $\beta_{Endo} \leq 0$) is not informative about the sign of the selection effect.

Proof.

$$\begin{aligned}
\beta_{Endo} &\equiv \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Endog}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Endog}\right] \\
&= \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&\leq \Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&\equiv \beta_{Selection}
\end{aligned}$$

□

where in the third line, we have used the fact that by assumption $c_i \geq 0$ and therefore $m_i > \phi_i \geq c_i \geq 0$. Thus, this effect is bounded above by the selection effect. It follows that a necessary condition for observing a positive β_{Endo} is a positive selection effect.

We now prove Proposition 1:

Proof. First note that:

$$
\begin{aligned}
\beta_{Exog} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Exog}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Exog}\right] \\
&= \Pr\left(m_i > \phi_i\right) - \Pr\left(m_i > 0\right) \\
&= \Pr\left(\phi_i \geq c_i\right) \cdot \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) + \Pr\left(\phi_i < c_i\right) \cdot \Pr\left(m_i > \phi_i \middle| \phi_i < c_i\right) \\
&\quad - \Pr\left(\phi_i \geq c_i\right) \cdot \Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \Pr\left(\phi_i < c_i\right) \cdot \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&= \left[1 - \Pr\left(\phi_i < c_i\right)\right] \cdot \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) + \Pr\left(\phi_i < c_i\right) \cdot \Pr\left(m_i > \phi_i \middle| \phi_i < c_i\right) \\
&\quad - \Pr\left(\phi_i \geq c_i\right) \cdot \Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \left[1 - \Pr\left(\phi_i \geq c_i\right)\right] \cdot \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&= \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&\quad - \Pr\left(\phi_i \geq c_i\right) \cdot \left[\Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right)\right] \\
&\quad - \Pr\left(\phi_i < c_i\right) \cdot \left[\Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > \phi_i \middle| \phi_i < c_i\right)\right] \\
&= \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&\quad - \left[1 - \Pr\left(\phi_i < c_i\right)\right] \cdot \left[\Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right)\right] \\
&\quad - \Pr\left(\phi_i < c_i\right) \cdot \left[\Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > \phi_i \middle| \phi_i < c_i\right)\right] \\
&= \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&\quad - \left[\Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right)\right] \\
&\quad - \Pr\left(\phi_i < c_i\right) \cdot \left[\Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right)\right] \\
&\quad + \Pr\left(\phi_i < c_i\right) \cdot \left[\Pr\left(m_i > \phi_i \middle| \phi_i < c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right)\right] \\
&= \beta_{Endo} - \beta_{Selection} - \Pr\left(\phi_i < c_i\right) \cdot \left[\beta_1 - \beta_0\right], \qquad \text{(A.2)}
\end{aligned}
$$

where we have used the definitions of the treatment effects for two subpopulations:

$$
\begin{aligned}
\beta_0 &= \Pr\left(m_i > \phi_i \middle| \phi_i < c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\
&= \mathbb{E}\left[Leave_i \middle| B_i = 1, \phi_i < c_i\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \phi_i < c_i\right] \\
\beta_1 &= \Pr\left(m_i > \phi_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) \\
&= \mathbb{E}\left[Leave_i \middle| B_i = 1, \phi_i \geq c_i\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \phi_i \geq c_i\right]
\end{aligned}
$$

These two parameters capture the effect of exogenously switching from the old to the new benefit

on the probability of leaving, for those who would not choose the new benefit when given the choice (β_0) and those who would choose the new benefit when given the choice (β_1), respectively. Thus, the difference between β_{Endo} and β_{Exog} gives:

$$\beta_{Endo} - \beta_{Exog} = \beta_{Selection} + \Pr(\phi_i < c_i) \cdot [\beta_1 - \beta_0]$$

If the second term in brackets, $[\beta_1 - \beta_0]$, is negative, then the results follows. We have focused on two sufficient conditions for this term to be negative. First, note that if $\beta_{Exog} > 0$, then we have:

$$\begin{aligned} 0 &\leq \beta_{Exog} \\ &= \beta_{Endo} - \beta_{Selection} - \Pr(\phi_i < c_i) \cdot [\beta_1 - \beta_0] \\ &\leq -\Pr(\phi_i < c_i) \cdot [\beta_1 - \beta_0] \\ \implies [\beta_1 - \beta_0] &\leq 0 \end{aligned}$$

where in the third line we have used Lemma 1. Alternatively, we can just assume that $[\beta_1 - \beta_0]$ is negative. In either case, the result follows. □

The assumption that $[\beta_1 - \beta_0]$ is negative will in general be true if the new benefit is less likely to make those who would choose the benefit leave the firm than those *would not* choose the benefit if given the choice. It makes sense that those for whom values of ϕ_i are high are less likely to have $m_i > \phi_i$, which is how this condition is represented in our model. However, this is not guaranteed to be negative and one could construct counter examples. When this assumption is true, we have the result and a necessary condition for $\beta_{Endo} - \beta_{Exog} \geq 0$ is that $\beta_{Selection} \geq 0$.

A.2 Allowing for a Direct Effect of Benefit Enrollment on Mobility

In the previous section, we restricted the effect of the new benefit on m to an effect on $\mathbb{E}[V_i(w_i, B_i)]$. We now show that an amended version of Proposition 1 still holds once this restriction is relaxed. We now define a new "mobility" parameter, $\tilde{m}$, as the value of mobility, net the switching cost

$$\tilde{m_i} \equiv \mathbb{E}[V_i^o(w_i^o, B_i^o)] - \mathbb{E}[V_i(w_i, 0)].$$

Furthermore, we now allow the employment switching cost to be a function of benefit enrollment, B_i. Without loss of generality, we normalize the switching cost to zero in the absence of the new benefit and define this new function $\tilde{\eta}(B_i)$ as follows:

$$\tilde{\eta_i} \equiv B_i \cdot \eta_i$$

It follows that the net benefit of mobility is now:

$$m_i \equiv \tilde{m}_i - \tilde{\eta}_i,$$

and the decision to leave is now made according to the following rule:

$$Leave_i = \begin{cases} 1 & \text{if } (\phi_i + \eta_i) \cdot B_i < \tilde{m}_i \\ 0 & \text{if } (\phi_i + \eta_i) \cdot B_i \geq \tilde{m}_i. \end{cases}$$

Heterogeneity is now captured by the quadruplet $(\phi, c, \tilde{m}, \eta)$. The incentive effect is now $\phi + \eta$, and without any further restrictions on η, Lemma 1 no longer holds. In particular, notice that the when $\eta_i < 0$, the benefit enrollment may increase the likelihood of leaving the firm. This is the case, for example, when the new benefit does not have as demanding a vesting a requirement. However, if we define the new treatment effect on each subpopulation as:

$$\begin{aligned} \tilde{\beta}_0 &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \phi_i < c_i\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \phi_i < c_i\right] \\ &= \Pr\left(m_i > \phi_i + \eta_i \middle| \phi_i < c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \\ \tilde{\beta}_1 &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \phi_i \geq c_i\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \phi_i \geq c_i\right] \\ &= \Pr\left(m_i > \phi_i + \eta_i \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right), \end{aligned}$$

the following, amended version of Proposition 1 is obtained:

Proposition 1a (Selection, Observational Correlations and Quasi-Experimental Estimates with Direct Mobility Effects)**.** *If exogenous benefit enrollment increases leave propensity by more among those who would not have endogenously enroll relative to those who would have enrolled (i.e. $\tilde{\beta}_0 \geq \tilde{\beta}_1$), then the difference between the endogenous (Equation (4)) and exogenous (Equation*

(6)) estimates is bounded from above by the selection effect defined in Equation (5). That is:

$$\beta_{Endo} - \beta_{Exog} \leq \beta_{Selection}$$

Proof. To prove this, we use the same steps as in (A.2) above. However, we substitute $m_i > \phi_i + \eta_i$ everywhere for the expression $m_i > \phi_i$. It then follows that:

$$\begin{aligned}
\beta_{Exog} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Exog}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Exog}\right] \\
&= \Pr\left(m_i > \phi_i + \eta_i\right) - \Pr\left(m_i > 0\right) \\
&= \beta_{Endo} - \beta_{Selection} - \Pr\left(\phi_i < c_i\right) \cdot \left[\tilde{\beta}_1 - \tilde{\beta}_0\right]
\end{aligned}$$

Rearranging terms, we again have:

$$\beta_{Endo} - \beta_{Exog} = \beta_{Selection} + \Pr\left(\phi_i < c_i\right) \cdot \left[\tilde{\beta}_1 - \tilde{\beta}_0\right],$$

and the result follows. □

A.3 Deviation from Traditional Omitted Variable Bias Intuition

In our model, we arrive at the result that when comparing OLS to an exogenous estimate of the treatment effect, we only learn about unobservable differences in leave probabilities when the exogenous effect is more negative than the OLS estimate. A traditional omitted variable bias calculation would suggest that the opposite observation would be equally informative about the selection. However, this notion only holds when there is a constant treatment effect. We relax that assumption in our case. To illustrate this, consider a case where all individuals have the same effect of new benefit enrollment on the probability of leaving: β^*. That is, the probability of leaving is characterized by the following equation:

$$Leave_i = \beta^* \cdot B_i + \varepsilon_i$$

The OLS regression of $Leave_i$ on B_i among employees who endogenously choose benefit enrollment recovers the following:

$$\begin{aligned}\beta_{Endo} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Endo}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Endo}\right] \\ &= \beta^* + \mathbb{E}\left[\varepsilon_i \middle| B_i = 1\right] - \mathbb{E}\left[\varepsilon_i \middle| B_i = 0\right]\end{aligned}$$

On the other hand, the effect of benefit enrollment when exogenously assigned recovers:

$$\begin{aligned}\beta_{Exog} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Exog}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Exog}\right] \\ &= \beta^*\end{aligned}$$

Thus, comparing the OLS estimate to an estimate based on exogenous variation is information about the selection effect: $\mathbb{E}\left[\varepsilon_i \middle| B_i = 1\right] - \mathbb{E}\left[\varepsilon_i \middle| B_i = 0\right]$. In our case, however, we relax the assumption of a constant treatment effect. Assume that the effect of benefit enrollment on leaving for those that endogenously choose to enroll is β_1 and the effect among those that do not enroll is $\beta_0 > \beta_1$. This approximates our model above, where those who have a high value of the benefit will experience a greater reduction in the probability of leaving when enrolled. Now, the OLS estimate among employees who choose their enrollment recovers:

$$\begin{aligned}\beta_{Endo} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Endo}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Endo}\right] \\ &= \beta_1 + \mathbb{E}\left[\varepsilon_i \middle| B_i = 1, \text{Endo}\right] - \mathbb{E}\left[\varepsilon_i \middle| B_i = 0, \text{Endo}\right]\end{aligned}$$

On the other hand, an estimate of the effect of benefit enrollment on leaving using exogenous variation recovers:

$$\begin{aligned}\beta_{Exog} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Exog}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Exog}\right] \\ &= \pi\beta_1 + (1-\pi)\beta_0\end{aligned}$$

where π is the share of employees that would choose the benefit voluntarily, and $(1-\pi)$ is the share

of employees who would not enroll. Since we have assumed that $\beta_0 > \beta_1$, it follows that:

$$\beta_1 < \pi\beta_1 + (1-\pi)\beta_0$$

Thus, observing $\beta_{Endo} < \beta_{Exog}$ is consistent with a positive or negative selection effect. However, $\beta_{Endo} > \beta_{Exog}$ is only consistent with a positive selection effect, i.e.

$$\mathbb{E}\left[\varepsilon_i | \, B_i = 1, \text{Endo}\right] - \mathbb{E}\left[\varepsilon_i | \, B_i = 0, \text{Endo}\right] > 0$$

The heterogeneity in treatment effects, then, prevents us from relying on the traditional intuition regarding omitted variable bias, and we can only sign the selection effect when it is positive enough to overcome the direct effect of benefit enrollment on leaving.

Appendix B: Supplemental Results

B.1 Characterizing the Marginal DC Enrollee

Table B.1 attempts to look deeper into the DRD results from Table (4). Column (1) reports the means of various observable characteristics among the sample of union employees in a 5-year window around age 45 in 2002. The final characteristic, predicted leave, is an estimated leave probability. Column (2) reports the mean characteristics among DC participants, and Column (3) reports the estimated average characteristic of the "compliers" in the DRD context. Here, we define compliers as those individuals who would not have enrolled in the DC plan were it not for the fact that they were defaulted into the DC plan. We use a method similar to Autor and Houseman (2005) to estimate the characteristics of the marginal DC enrollee via a DRD. In all cases, the estimates are regression-adjusted for age.

In general, we do not detect significant differences between the marginal DC enrollee and the average employee in our sample, though our power is limited. The exception is that we find that compliers are more likely to be White and less likely to be Hispanic.

Table B.1: Complier Characteristics (2sls)

	(1) Sample Mean	(2) DC Mean	(3) Compier Mean
Female	0.153	0.186	0.095
	(0.020)	(0.040)	(0.076)
White	0.459	0.361	0.285*
	(0.025)	(0.049)	(0.094)
Black	0.107	0.113	0.146
	(0.016)	(0.032)	(0.062)
Hispanic	0.270	0.340	0.475**
	(0.024)	(0.048)	(0.094)
Other Race	0.163	0.186	0.095
	(0.020)	(0.040)	(0.076)
Tenure	11.069	10.195	10.593
	(0.314)	(0.628)	(1.195)
Hours/Wk	39.6	39.5	39.8
	(0.1)	(0.3)	(0.5)
Base Wage	50,864	49,638	49,127
	(651)	(1,309)	(2,482)
Leader	0.252	0.246	0.167
	(0.031)	(0.054)	(0.104)
Problem Solving	46.811	46.523	48.138
	(0.735)	(1.255)	(2.422)
Critical Thinking	54.748	54.446	55.450
	(0.640)	(1.093)	(2.112)
Economics/Acct	21.784	21.800	19.093
	(0.690)	(1.178)	(2.317)
Predicted Leave	0.053	0.060	0.058
	(0.003)	(0.006)	(0.012)
Bandwidth $f(Age)$	196	196	196

Note: Sample includes union employees in the years 2002. DC and Stay are instrumented for using discontinuity in default retirement benefit at the age of 45 in 2002. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

B.2 Results with a 10-year Bandwidth

Table B.2: OLS and Fuzzy RD Estimates of Effect of DC Plan on One-Year Leave Probability: 2002

	(1)	(2)	(3)
β_{OLS}	-0.016	-0.016	-0.007
	(0.028)	(0.034)	(0.037)
β_{RD}	-0.045	-0.140*	-0.028
	(0.043)	(0.080)	(0.060)
H_0: $\beta_{OLS} \leq \beta_{RD}$	0.271	0.083	0.392
First Stage	0.496***	0.492***	0.768***
	(0.050)	(0.091)	(0.158)
Reduced Form	-0.022	-0.069*	-0.021
	(0.022)	(0.038)	(0.049)
$\mathbb{E}[Leave_i]$	0.041	0.041	0.041
Bandwidth	10	10	10
$f(Age)$	None	Linear	Cubic
N	362	362	362
First Stage F-stat	99.5	29	23.5

Note: Sample includes employees in the year 2002. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 (i.e. "Treatment" is DC plan default). P-value for H_0 reported for evaluating implication of Proposition 1. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table B.3: OLS and DRD Estimates of Effect of DC Plan on One-Year Leave Probability: 1999-2002, Cohort Comparison

	(1)	(2)	(3)	(4)
β_{OLS}	-0.012	-0.011	-0.012	-0.011
	(0.025)	(0.025)	(0.025)	(0.025)
β_{DRD}	-0.077*	-0.077*	-0.077*	-0.076*
	(0.045)	(0.044)	(0.044)	(0.044)
H_0: $\beta_{OLS} \leq \beta_{DRD}$	0.084	0.080	0.085	0.084
First Stage	0.543***	0.543***	0.543***	0.543***
	(0.045)	(0.045)	(0.045)	(0.045)
Reduced Form	-0.042*	-0.042*	-0.042*	-0.041*
	(0.024)	(0.024)	(0.024)	(0.024)
$\mathbb{E}[Leave_i]$	0.049	0.049	0.049	0.049
Bandwidth	10	10	10	10
$f(Age)$	None	Linear	Cubic	Non-Par
N	1,498	1,498	1,498	1,498
First Stage F-stat	148	148	147	147

Note: Sample includes employees in the years 1999-2002. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 in 2002 (i.e. "Treatment" is DC plan default). Comparison group consists of same cohorts of workers in 1999-2001. P-value for H_0 reported for evaluating implication of Proposition 1. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table B.4: OLS and DRD Estimates of Effect of DC Plan on One-Year Leave Probability: 1999-2002, Age Comparison

	(1)	(2)	(3)	(4)
β_{OLS}	-0.003	-0.002	-0.002	-0.003
	(0.025)	(0.024)	(0.025)	(0.025)
β_{DRD}	-0.046	-0.046	-0.047	-0.046
	(0.044)	(0.045)	(0.044)	(0.044)
H_0: $\beta_{OLS} \leq \beta_{DRD}$	0.172	0.175	0.168	0.180
First Stage	0.541***	0.539***	0.540***	0.541***
	(0.045)	(0.045)	(0.044)	(0.044)
Reduced Form	-0.025	-0.025	-0.025	-0.025
	(0.024)	(0.024)	(0.024)	(0.024)
$\mathbb{E}[Leave_i]$	0.044	0.044	0.044	0.044
Bandwidth	10	10	10	10
$f(Age)$	None	Linear	Cubic	Non-Par
N	1,532	1,532	1,532	1,532
First Stage F-stat	147	145	148	148

Note: Sample includes employees in the years 1999 - 2002. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 (i.e. "Treatment" is DC plan default). Comparison group consists of workers around age 45 threshold in 1999-2001. P-value for H_0 reported for evaluating implication of Proposition 1. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table B.5: RD and DRD Estimates of Effect of DC Plan on Two- and Three-Year Leave Probabilities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2002			1999-2002: Cohort Control				1999-2002: Age Control			
2 Year	-0.067	-0.160	0.047	-0.147**	-0.147**	-0.147**	-0.145**	-0.090	-0.090	-0.090	-0.087
	(0.050)	(0.101)	(0.113)	(0.067)	(0.067)	(0.067)	(0.067)	(0.063)	(0.064)	(0.063)	(0.064)
$\mathbb{E}[Leave_i]$	0.058	0.058	0.058	0.092	0.092	0.092	0.092	0.082	0.082	0.082	0.082
N	362	362	362	1,129	1,129	1,129	1,129	1,156	1,156	1,156	1,156
3 Year	-0.068	-0.072	0.024	-0.165*	-0.166*	-0.162*	-0.160*	-0.044	-0.045	-0.044	-0.024
	(0.061)	(0.124)	(0.160)	(0.086)	(0.086)	(0.086)	(0.086)	(0.083)	(0.084)	(0.083)	(0.083)
$\mathbb{E}[Leave_i]$	0.094	0.094	0.094	0.119	0.119	0.119	0.119	0.110	0.110	0.110	0.110
N	362	362	362	739	739	739	739	748	748	748	748
Bandwidth	10	10	10	10	10	10	10	10	10	10	10
$f(Age)$	None	Linear	Cubic	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par

Note: Sample includes employees in 2002 and 1999-2002 samples. DC is instrumented for using the discontinuity in default pension plan type at the age of 45 in 2002 (i.e. "Treatment" is DC plan default). Exogenous, or incentive, effect of DC plan on two- and three-year turnover outcomes is reported for fuzzy RD (columns 1 to 3) and two DRD analyses: by cohort (columns 4 to 6) and by age (columns 7 to 9). Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust and clustered at the employee level. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.