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EMPLOYEE MOBILITY AND EMPLOYER-PROVIDED RETIREMENT PLANS

Gopi Shah Goda, Damon Jones, and Colleen Flaherty Manchester

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Center for Retirement Research at Boston College
Hovey House
140 Commonwealth Avenue
Chestnut Hill, MA 02467
Tel: 617-552-1762 Fax: 617-552-0191
http://crr.bc.edu

Gopi Shah Goda is a senior research scholar at the Stanford Institute for Economic Policy Research. Damon Jones is an assistant professor at the University of Chicago. Colleen Flaherty Manchester is an assistant professor at the University of Minnesota. The research reported herein was pursuant to a grant from the U.S. Social Security Administration (SSA), funded as part of the Retirement Research Consortium (RRC). The findings and conclusions expressed are solely those of the authors and do not represent the views of SSA, any agency of the federal government, the RRC, the Stanford Institute of Economic Policy Research, the University of Chicago, the University of Minnesota, or Boston College.

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Center for Retirement Research at Boston College
Hovey House
140 Commonwealth Avenue
Chestnut Hill, MA 02467
phone: 617-552-1762 fax: 617-552-0191
e-mail: crr@bc.edu
crr.bc.edu

Abstract

This paper provides new insights into the effect of the widespread transition from defined benefit (DB) to defined contribution (DC) pension plans on employee mobility. Pension plans may affect employee mobility both through an “incentive effect,” where the bundle of benefit characteristics, such as vesting rules, relative liquidity and the risk/return tradeoff affect turnover directly, and a “selection effect,” where employees with different underlying mobility tendencies select into firms with different types of pension 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 DB to a DC plan was exogenously affected by the default provisions of the transition. Using a differences-in-regression-discontinuities (DRD) estimator, we find evidence that employees with higher mobility tendencies self-select into the DC plan. Furthermore, we find a negative direct effect of DC enrollment on turnover that takes place within one year. Our results suggest that selection likely contributes to an observed positive relationship between the transition from DB to DC plans and employee mobility in settings where employees choose plans or employers.

1 Introduction

Among American employees 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). Reasons given for this dramatic shift are many, including increased regulatory costs for DB plans, changes in technology that reduced the benefit of tenure (Friedberg and Owyang 2004) and the fact that 401(k) plans were popularized in the 1980s, which coincided with a strong equity market (Munnell, Haverstick and Sanzenbacher 2006). In addition, higher employee mobility is thought to have played a key role in the transition given that the benefit structure of DB plans often rewards employees who spend their entire career with a single employer. However, was it the change in pension plan landscape that led to increased mobility, or did pension plan coverage adapt to the greater mobility preferences of employees and/or firms? Due to the large number of employees affected by the plan transition, this question plays an important role in understanding labor market dynamics.

One reason this question has attracted attention is that it is commonly thought that employee mobility also experienced a dramatic shift during this same time period, although empirical evidence has been scarce. Recent work shows evidence of reduced tenure and greater mobility among younger workers starting in the 1990s (e.g., Schrager 2009, Farber 2007), while Munnell et al. (2006) find increased mobility among older workers during this time period. Furthermore, Munnell et al. argue that the change in pension coverage from DB-only to DC-only explains the increase in mobility over time. While the finding that mobility did not change until the 1990s suggests that the change in pension plan landscape likely contributed to the change in employee mobility, the results from Munnell et al. do not address possible selection of employees across employers based on mobility tendencies, which precludes causal conclusions.

This paper identifies the role of selection in explaining 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 that was 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. The features of the default rule allow, along with repeated cross sections of data, 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. Furthermore, we combine our date in the year of the policy change with data from previous cross sections in a fashion similar to a differences-in-differences (DD) framework. We hereafter refer to this approach, which affords us greater precision, 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 of positive selection into the DC plan based on employee mobility tendencies in that the DRD estimates of the DC plan effect on turnover are significantly less than the OLS estimates.

This paper contributes to the literature by providing a new source of identification to quantify the role of selection into retirement plan type based on preferences for mobility. Prior studies have generally addressed this selection by using selection-correction models that rely on functional form assumptions or cross-sectional data that includes heterogeneous firms and plans (Allen and Clark 1993; Gustman and Steinmeier 1993; Rabe 2007). Other studies have used plausibly exogenous variation from tax reform (Andrietti and Hilderband 2004) or plan offerings (Disney and Emmerson 2004; Manchester 2010) to identify consequences of pension plans for mobility. The regression discontinuity approach we use relies on fewer assumptions, namely that mobility rates for affected employees relative to non-affected (but otherwise similar) employees did not change discontinuously at the age where the default enrollment plan changed.

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 environment in which benefits are 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 provide a richer interpretation of our estimate for how the introduction of the DC plan affected mobility in our specific context.

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 reduce employee mobility (e.g., Gustman and Steinmeier 1993; Ippolito 2002) and have 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 employee selection is also a factor. Our results are also consistent with the possibility that employees who find the features of DC plans desirable, such as shorter vesting periods, increased control, and loan and withdrawal provisions, or those who place a low value on DB benefits (Fitzpatrick 2011), have lower mobility tendencies.

Finally, 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. We find that the DC plan had an immediate and large effect on mobility rates for those exogenously moved to the plan. 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 more long-run retention of employees. In other words, affected employees appear to delay their exit, but only for a short period of time.

The remainder of the paper proceeds as follows. Section 2 describes the conceptual framework that motivates our empirical approach and how our results inform the relationship between employee heterogeneity and employee benefit enrollment. Section 3 provides details regarding the natural experiment we exploit in our empirical application. We outline our DRD empirical strategy 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 Empirical Model of Benefit Choice and Mobility

We construct a conceptual framework for understanding the implications of observational and quasi-experimental estimates of the relationship between mobility patterns and employee benefit enrollment given unobservable heterogeneity across employees. We model the decision between a new employer-provided benefit and a mutually exclusive 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; $c_i \geq 0$, 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 $F(\cdot)$.

We denote B_i a binary variable indicating adoption of the new benefit at one's current employer and $Leave_i$ 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 thus 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 order to realize this utility change, the employee must pay a cost of switching to the new benefit, $c_i \geq 0$. This may include such costs as a time commitment, informational requirement or administrative hurdle associated with switching benefits. 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 $V_i^O\left(w_i^O, B_i^O\right)$ 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 original benefit at the current employer:

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

Thus, individuals with a higher m_i are more "mobile", in that their outside options tend to be better relative to the current employer. 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 thought experiments. 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 comovement of preferences for the new benefit, ϕ, and mobility, m.

In the endogenous case, an employer introduces a new benefit and allows employees to select into this benefit according to equation (2). Subsequently, employees make a decision on whether or not to leave the firm according to equation (3). Consider a comparison of the subsequent leave probabilities among those enrolled, $\mathbb{E}\left[Leave_i | B_i = 1\right]$, and those not enrolled, $\mathbb{E}\left[Leave_i | B_i = 0\right]$.

First, note that those who have chosen to enroll must have a positive value of ϕ_i, given equation (2) and the assumption that $c_i \geq 0$. Focusing just on the left-hand sides of the inequalities in (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."

Now we turn to the right-hand sides of the inequalities in (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:

$$\Pr\left(m_i > 0 \middle| \phi_i \geq c_i\right) - \Pr\left(m_i > 0 \middle| \phi_i < c_i\right) \lesseqgtr 0 \tag{4}$$

To explore the role of selection, fix $c_i = c$. Conditional on c, if ϕ_i and m_i are independent, then (4) is zero and we would expect there to be no selection effect on leave probabilities. In this case, the incentive effect ensures that leave probabilities are lower for those employees that do enroll in the benefit. Alternatively, assume that, conditional on c, (4) 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 (4) 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 sorting effect only mitigates, neutralizes, or dominates the incentive effect. We provide an illustrative example of these different scenarios in Appendix A.

Now consider the exogenous case, where an employer forces all employees to enroll in the benefit. Imagine comparing the probability of leaving the firm before and after this change takes place. The decision to leave the firm is still dictated by Equation (3). However, now that employees are not self-selecting into benefit participation, it is no longer the case that $\phi_i \cdot B_i > 0$ for all enrollees. Thus, 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. On the other hand, we no longer have a selection effect, since plan enrollment in this case is independent of m. In a data set containing pre and post data, the comparison of $\mathbb{E}\left[Leave_i \middle| B_i = 1\right]$ and $\mathbb{E}\left[Leave_i \middle| B_i = 0\right]$ identifies the incentive effect among all employees, where variation in B_i is driven by an exogenous change in benefit enrollment. 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 $0 \geq m_i > \phi_i$. Thus, this estimate signs the relative share of employees that have a positive value for the new benefit, among the set of "marginal" employees.

Let us again explore the implications of the distribution of (ϕ, m, c) when comparing the results from the endogenous and exogenous cases. Fixing $c_i = c$ again, assume that there is no selection effect, i.e. (4) has a sign of zero. This means that the distribution of m does not differ among those who choose to enroll in the new benefit under endogenous enrollment, and all employees under exogenous enrollment. In that case, we would expect to find a larger reduction in leave probabilities under the endogenous case than under the case where benefit choice is exogenous. The reason is that 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. Those that select into the new benefit have relatively higher values for the benefit, and therefore larger reductions in the probability of leaving, all things equal. Now, assume that the selection effect is negative. This only increases the discrepancy between the endogenous and exogenous cases, as the baseline likelihood of leaving is lower among enrollees in the endogenous case than it is for the average employee in the exogenous case. Finally, a positive selection effect offsets the difference between the endogenous and exogenous estimates, potentially even reversing the sign.

Mapping the model to empirical estimates, a regression of leave on new benefit enrollment among employees who can choose their benefit approximates the endogenous case. The correlation between $Leave_i$ and B_i is driven by both the incentive effect and the selection effect. In general any result may arise, but in the presence of a positive correlation between enrollment in the new benefit and mobility, we can rule out a zero or negative 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. The difference between the two estimates is ambiguous, but again, in the case where the estimates from random assignment show a larger reduction in leave probabilities than the endogenous estimates, we can rule out a zero or negative selection effect.

In the next section, we describe the natural experiment that allows us to obtain both the endogenous and exogenous estimates in order to sign the selection and incentive effects in the context of employer-provided retirement plans.

3 Institutional Setting and Data

3.1 Setting

This paper primarily uses data on unionized, non-faculty employees from a large research university. These unionized employees, including both skilled and unskilled blue-collar 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.[1] If no election was made, the employee was enrolled in the default plan. The default plan was heterogeneous and depended on the employee's age as of September 1: employees under age 45 were assigned the DC plan as the default, while employees age 45 or older were assigned the DB plan as the default.[2] 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.[3] Employer contributions were considered vested after 1 year of service.[4]

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, employment group, salary, and weekly hours worked as well as demographic characteristics, including exact date of birth, gender, race, and hire date. 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). In addition, police officers were excluded from the sample because they were covered under a different collective bargaining agreement which specified a different pension accrual process (30 observations).

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

[2] 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, and 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.

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

[4] In a study which examines the 2002 transition for union employees, Goda and Manchester (2010) 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.

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 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 sample is the subset of union employees 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. We supplement this sample with union employees present in 1999-2001. These employees were all enrolled in the DB plan, and therefore do not have a discontinuity in policy at age 45. These data provides information about the counterfactual leave probabilities in this sample, and helps us to test for any other, coincident changes in leaving at age 45.

Table (1) shows summary statistics for the 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. Column 3 applies the age restriction of 40 to 50 years of age. Finally, the last two columns represent the data used in the primary analysis. Column 4 includes union employees in 2002, while Column 5 includes union employees in 1999-2001. 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.

4 Empirical Strategy

Based on the model outlined in Section 2, we estimate the endogenous and exogenous relationship between enrollment in the DC plan and mobility albeit with one difference. Rather than true random assignment as described in 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 differences-in-regression-discontinuities (DRD) design. That is, we compare outcomes in 2002 for people over and under 45 to the same difference in the years 1999-2001 where there was no discontinuity. This discontinuity in the default plan 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. Formally, these are employees for whom $|\phi_i| \leq c_i$. These employees may not represent the average employee. Nonetheless, we can use the same conditions to sign the selection effect in that finding a larger reduction in leave probabilities from our exogenous estimates (DRD) than under our exogenous estimates (OLS) indicates a positive selection effect. The predictions for OLS and DRD estimates under various assumptions are summarized in Table (2). [5]

We first compute OLS estimates of the effect of DC plans on employee mobility by running the following regression:

$$leave1_i = \beta^{OLS} \cdot DC_i + f(A_i - 45) + f(A_i - 45) \cdot Under45_i + \Gamma X_i + \varepsilon_i \tag{5}$$

where $leave1_i$ is a binary variable which 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 which 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.

As described in Section 2, OLS estimates that compare mobility rates among DC participants and DB participants are driven 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

[5] Note that results in the opposite direction, i.e. a larger reduction in leave probabilities under OLS than under our DRD are not informative about selection in our case. This may seem counterintuitive given the standard approach of signing omitted variable bias. However, we show in Appendix Appendix A.4 why this is the case. In short, the standard omitted variable bias intuition does not hold in the presence of heterogeneous treatment effects.

of plans. In particular, if unobservable mobility tendencies are systematically positively related to preferences for the DC plan, the selection effect works in the opposite direction of the incentive effect and can lead to a correlation between mobility rates and DC enrollment that is positive, negative, or zero.

To isolate the incentive effect, we combine elements of difference-in-difference with a fuzzy RD design, i.e. a differences-in-regression-discontinuities (DRD), which exploits the random variation in plan enrollment induced by the structure of the default. 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 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:

$$DC_i = \delta \cdot DCDefault_i + f\left(A_i - 45\right) + f\left(A_i - 45\right) \cdot Under45_i + \Gamma X_i + \varepsilon_i \tag{6}$$

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 a union 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:

$$leave1_i = \beta^{DRD} \cdot DC_i + f\left(A_i - 45\right) + f\left(A_i - 45\right) \cdot Under45_i + \Gamma X_i + \varepsilon_i \tag{7}$$

where DC is instrumented for with the age-based policy as shown above. The estimate β^{DRD} 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.

In our results, we also report the results of reduced-form regressions, which replace DC_i in Equation 7 with $DCDefault_i$, and the results of a Hausman test, which allows us to determine whether β^{DRD} is significantly different from β^{OLS}. If $\beta^{OLS} - \beta^{DRD}$ is positive, we can rule out a negative selection effect.

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. Restricting the window of observations around age 45 is desirable to improve comparability of individuals on either side of the age cutoff. However, doing so can considerably reduce power. Therefore, we make use of additional sets of employees who did not undergo changes in their retirement plans. We include data from union employees in 1999-2001 prior to the transition. In is in this sense that our estimation incorporates features of a differences-in-differences estimator. Note that the assumptions underlying a fuzzy RD estimator allow us to estimate everything using just on cross section. However, the inclusion of pre-policy cross sections has at least two advantages over one cross section. First, the additional data allows us increased precision and power to control for flexible functions of age. In particular, these additional control groups allow us to add a fourth, nonparametric functional form for our control function in age:

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

where k is the size of the bandwidth around age 45 to which the analysis is restricted. In these specifications, X_i also includes employment group and time fixed effects. Secondly, 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. One cross section of data would not allow us to directly test this.

5 Results

5.1 Graphical Analysis

Figure 1 shows the DC plan enrollment rate by 6-month age bins for union employees in 2002, union employees in 1999-2001, and non-union employees across all four years. While union employees in 2002 have DC enrollment rates that vary across age due to the default provision, the other comparison groups do not. Figures 2 and 3 confirm that the age distribution is smooth across the age 45 cutoff and that observable factors do not vary in conjunction with the default.

Figure 4 depicts the probability of leaving within one year for 6-month age bins between 40 and

50 for union employees employed at the firm in 2002. The figure does not show a clear discontinuity between mobility rates on either side of the age 45 cutoff despite the difference in underlying plan enrollment rates shown in Figure 1, though the estimates are noisy.

In Figure 5, we show the difference in employee mobility between union employees in 2002 and union employees in 1999-2001, who did not undergo a transition in their pension plan, by 6-month age bins. The figure shows evidence of a discontinuity in one-year leave probabilities across the age 45 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.

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

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. Table (3) reports the results for the sample of union employees affected by the policy change in 2002. Coefficients from the OLS regression in equation (5) are reported in the first row. The first three columns pertain to a 5-year window around the age of 45, while the second three columns refer to the sample within a 10-year window. The correlation between DC plan enrollment and leaving the firm is not significantly different from zero and furthermore does not have a stable sign across different bandwidths.

The next three rows contain the results from the first stage, reduced form, and fuzzy RD regressions described in equations (6) and (7). Coefficients (7) - (12) all demonstrate a significant first-stage relationship between the default policy and DC enrollment, though the point estimate varies between 0.455 and 0.768. Coefficients (13) - (18) report the corresponding reduced form estimates of the default policy on the likelihood of leaving the firm within the next calendar year. The results are negative, though generally statistically insignificant.

The fuzzy RD results follow in the bottom row of coefficients. 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. It should also be noted that the fuzzy RD estimates are almost always more negative than the OLS estimates, suggesting a positive selection of high-mobility employees into the DC plan. However, the p-values from the Hausman Test, reported on the bottom row of the table, do not allow us to reject the null hypothesis that the OLS and fuzzy RD estimates are the same.

In order to increase the power of our estimates, we combine a difference-in-difference approach with our fuzzy RD in a DRD framework by including union employees from the three years preceding the retirement plan change in our sample and examine the effect of the DC plan on one-year leave probabilities. These results are reported in Table (4), which mirrors Table (3) in format. In this case, we again see very little evidence of a correlation between employee mobility and DC plan enrollment in the first row of OLS regression results. Our second row now demonstrates a strong and robust first-stage relationship between the policy and DC plan enrollment. Those just below the age 45 threshold are about 50 percentage points more likely to enroll in the DC plan, which we interpret as the causal effect of a DC default assignment on DC enrollment.

The third row in Table (4) now features a significant and stable four to five percentage point reduction in employee leave probabilities as a result of the policy. This effect translates into a DRD estimate of an eight to ten percentage point reduction in the probability of leaving the firm within the next year as a result of being enrolled in the DC plan. Our estimates are now much more stable and robust to alternative methods of controlling for the running variable. When restricting analysis to a 5-year window surrounding the age of 45, the p-values from the Hausman test allow us to reject, at the 5 percent level, the null hypothesis that the OLS and DRD estimates are the same. Thus, we confirm our earlier observation that the selection effect is positive, i.e. mobility tendencies are positively related to preferences for the DC plan. While the results of the Hausman test are slightly weaker if we widen the window around the threshold age from five years to ten, the smallest window of estimation provides a more comparable control group for the RD analysis. Therefore, our preferred estimates come from columns (1) through (4) in Table (4).

These results are large relative to the average leave probability among the sample, which is between four and five percent. We discuss below 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. We also investigate the appropriateness of the control group in the DRD estimates in Section 5.5.

5.3 Two- and Three-Year Leave Probabilities

We extend the analysis from the previous section to longer-run outcomes, i.e. the likelihood of leaving the firm within two or three years. Table (5) reports fuzzy RD for the 2002 and DRD results using 1999-2000, for various samples and specifications. Our sample is slightly smaller than that used in our main set of estimates in order to eliminate employees 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, they remain identical to those contained in the two previous tables.

We focus on the lower panel, which mirrors our preferred results from Table (4). We now see 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 in the sample. This pattern suggests that there is an additional, though relatively smaller, reduction in employees leaving in the second year.[6]

Looking at results of pension plans 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, with a window of 5 years around age 45, the effect on three-year turnover 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.

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

5.4 Characterizing the Marginal DC Enrollee

In Table (6) we 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, for the years 1999 to 2002. The final characteristic, predicted leave, is an estimated leave probability. This measure is taken from a regression of the one year leave probability on the set of covariates, using only the 1999 - 2001 data, and is meant to capture the underlying level of employee mobility. 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, so that they are comparable to the complier estimates.

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. There seem to be slight differences in racial and gender composition, and a marginally lower underlying leave probability among compliers. It does not appear that we have enough power to detect any meaningful differences between the marginal enrolled and the average employee.

5.5 Robustness Checks

5.5.1 Placebo Discontinuities at Alternate Ages

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 the DRD design is that specification checks that help to test the validity of our exclusion restrictions are readily available. 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 (7) contains the results for one-year leave probabilities at these alternative thresholds.

We see that across all of our specifications, we do not detect significant effects. In fact, the DRD results are wildly unstable, due to the fact there is not an associated first stage. In particular, the first stage F-statistics are dramatically smaller, and the imprecisions associated with weak instruments are therefore borne out. Thus, we 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. We test for this phenomenon in alternative years in the next section.

5.5.2 Placebo Discontinuities in 1999-2001

We use union employees during the year 1999 - 2001 as our main control group. In theory, this group, which was not affected by our policy, should not exhibit any jumps in leave probabilities around the age of 45. In the first panel of Table (8) we assess the validity of this control group by estimating the reduced form discontinuity in one-year leave probabilities at age 45 for union employees for each of the years 1999-2001. In each case, we do not find a significant difference between those just under and above age 45 nor do we find any trend. In the final column, we estimate the average difference pooling the years 1999 to 2001, which represents the counterfactual we use when pooling the data among union employees in 1999 - 2002 in Table (4). Here, we find a very convincing estimate of zero difference in turnover probabilities at the threshold age, suggesting that the 1999-2001 union employees serve as an appropriate control group.

Our data also includes non-union employees during the years 1999-2002. In the second panel of Table (8) we assess this potential control group by repeating the analysis described above. In each year, there is not a significant change in leave probabilities at the threshold age. However, there does seem to be a trend of increasing mobility over the time period as the estimate in column (8) for the year 2002 is much larger than those shown in columns (5) and (6) for non-union employees in 1999 and 2000. Most importantly, column (9) shows that there is a marginally significant increase in leave probabilities in 2002 relative to the year 1999 to 2001 for this sample of non-union employees.

This estimate represents the counterfactual that would be used when pooling the union and non-union employees from 1999-2002 to estimate a triple difference. Thus, when pooling these data, we would expect to overstate the difference between those under and over age 45. This is confirmed in our Appendix Tables (B.1) and (B.2). These results suggest that there are nontrivial unobservable differences between the union and non-union employees, particularly when comparing turnover in the year 2002 to earlier time periods. For these reasons, we only rely on the pre-policy union employees as a control sample.

6 Conclusion

Past research on the incentive and selection effects in explaining pensions and mobility have generally compared employees covered by DB pension coverage to uncovered workers, finding that covered workers have lower mobility than those who are not covered. This comparison in mobility rates among covered and uncovered workers combines an incentive effect driven by specific plan features (such as backloading and vesting in DB plans) and a selection effect due to differences in underlying mobility tendencies for workers who choose to work at a firm that provides a pension as opposed to a firm without a pension. Approaches and results are varied. Studies that have tried to estimate both effects have found evidence of incentive and selection effects using a selection model (Allen and Clark 1993) or a natural experiment (Disney and Emmerson 2004). Studies that have tried to isolate the incentive effect using changes in DB regulations have found no evidence (Andrietti and Hilderband 2004), while others have suggested that the negative relationship between pension coverage and mobility is driven by a compensation premium for jobs with coverage (Gustman and Steinmeier 1993, Allen and Clark 1993).

What insights can the results obtained from evaluating covered versus non-covered workers provide on the likely impact of the transition from DB to DC plan on mobility? If the differences in mobility between covered and uncovered workers were driven primarily by incentives of DB plans, and if the DC plans are void (or have fewer) of the features, then mobility is likely to increase as more workers are covered by the DC plan. If, however, the differences in mobility between covered and non-covered workers were driven by compensation premiums, then the transition from DB pensions to DC plans is not likely to have an effect on mobility if firms with DC plans have similar overall compensation for firms with a DB plan. Finally, if the difference in mobility between covered and non-covered workers is due to selection, then the transition could drive a correlation between plan type and mobility without changing the underlying mobility propensities. It is also important to note that DC and DB plans differ in other ways that may drive mobility tendencies. In particular, they also differ in risk exposure, liquidity (due to loan and hardship withdrawals which are generally possible for DC plans), and transparency (DB plans have a formulaic benefit structure, while DC plans provide individuals with an individual account), which have unknown relationships with mobility.

In this paper, we exploit a natural experiment that created random variation in retirement plan enrollment to study the effects of retirement plans 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 previous literature. We develop an empirical model that helps us to interpret these results. This framework provides predictions regarding the different effects of endogenous and exogenous retirement plan enrollment as they relate to the role of selection in plan type. In particular, our empirical results combined with insights from our model suggest 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 more generally 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 plan features. 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 shown to explain a potentially large part of the mobility differences between covered and uncovered workers (Gustman and Steinmeier 1993).

Third, we find evidence that, perhaps counter to casual inference, DC plans may reduce mobility relative to DB plans. This result shows that 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 multidimensional and include differences in risk exposure, liquidity, and transparency. Fourth, we find that the incentive and selection effects work in opposite directions. 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. That those with relatively high values of the DC plan choose it will tend to offset the feature of the DC plan, such as a shorter vesting period, that tend to general higher turnover. From an employer perspective, the tradeoff of higher DC enrollment and lower turnover may be mitigated by transaction costs, implying a nonzero level of optimal benefit switching costs.

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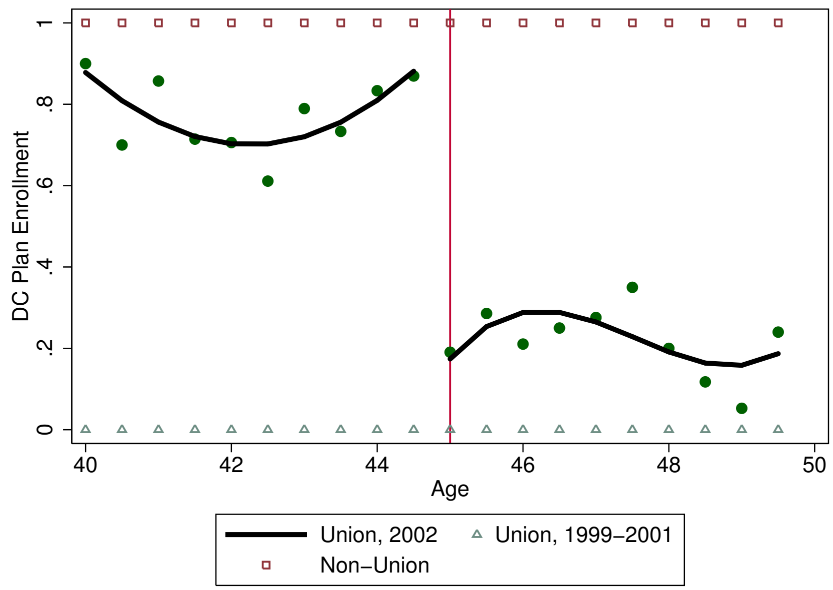
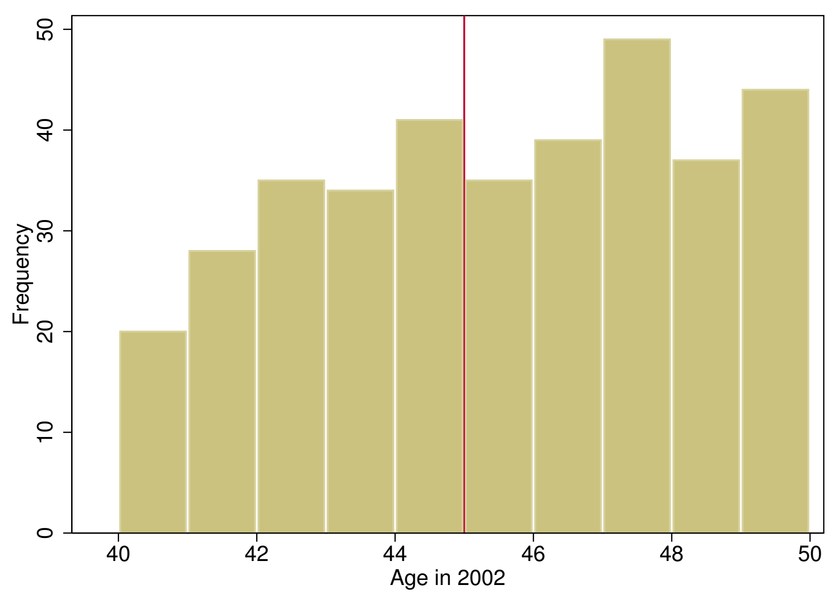
Figure 1: DC Plan Enrollment by Year and Employment Group



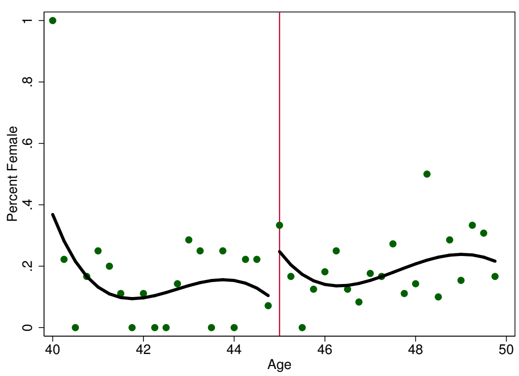
Notes: Dots represent DC enrollment rate for quarter year of age bins.

Figure 2: Distribution of Employee Age for Union Employees in 2002

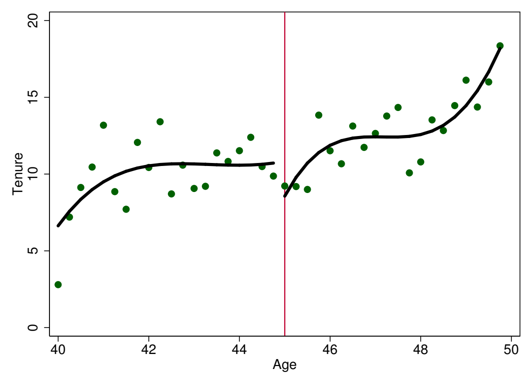


Notes: Histogram of employee age as of September 1, 2002 for union employees, using one-year bins.

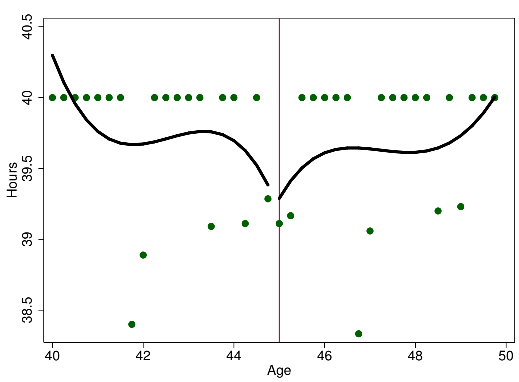
Figure 3: Average Value of Covariates by Quarter Year of Age



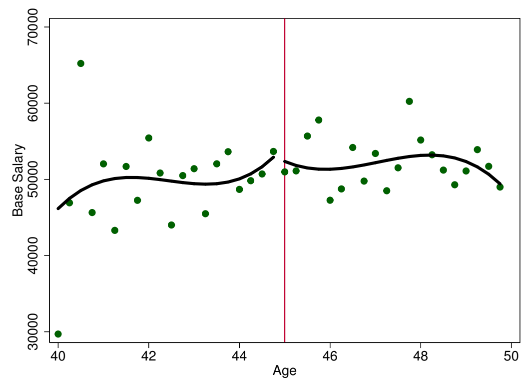
(a) Proportion Female



(b) Tenure at Firm



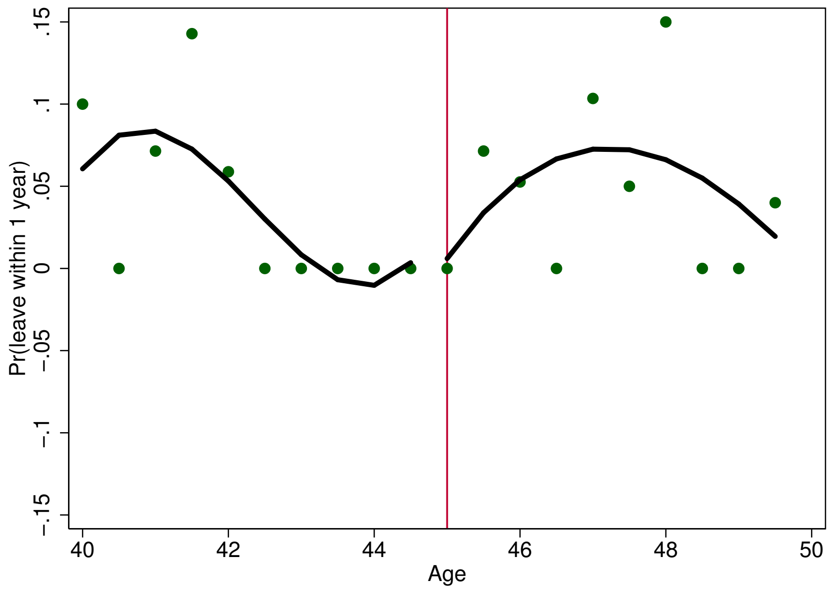
(c) Average Hours per Week



(d) Average Annual Salary

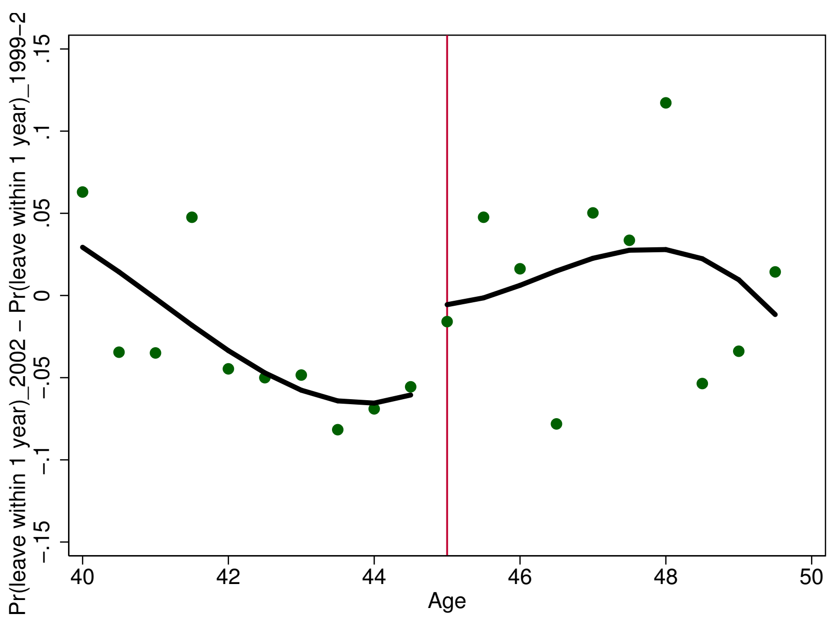
Notes: Panels used to verify no discontinuity at age 45 for other observable characteristics; vertical line marks age 45.

Figure 4: Probability of Leaving within One Year: Union Employees in 2002



Notes: Dots represent average leave probability for 6-month age bins in the year 2002.

Figure 5: Probability of Leaving within One Year: Union Employees in 1999-2002



Notes: Dots represent average leave probability for 6-month age bins in the year 2002 relative to years 1999-2001.

Appendix A Model Discussion

Appendix A.1 Simple Illustration – Endogenous Case

Recall our two thought experiments in Section 2. In the Case 1, B_i is endogenously determined by the employees. In Case 2, 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 comovement of preferences for the new benefit, ϕ, and mobility, m. In Case 1, an employer introduces a new benefit and allows employees to select into this benefit according to equation (2). Subsequently, employees make a decision on whether or not to leave the firm according to equation (3). Consider a comparison of the subsequent leave probabilities among those enrolled, $\mathbb{E}\left[Leave_i \middle| B_i = 1\right]$, and those not enrolled, $\mathbb{E}\left[Leave_i \middle| B_i = 0\right]$.

We demonstrate Case 1 graphically in Figure A.1. For illustrative purposes, we make the following simplifying assumptions about the support and conditional distributions of (ϕ, m, c). Conditional on c, let ϕ be distributed uniformly on the interval $\left[\underline{\phi}, \overline{\phi}\right]$. Furthermore, conditional on (ϕ, c), let m be distributed uniformly on the interval $\left[\underline{m}(\phi), \overline{m}(\phi)\right]$. We will examine three conditions. First, if ϕ and m are independent, conditional on c, then $\left[\underline{m}(\phi), \overline{m}(\phi)\right]$ are constant functions of ϕ. Second, to model a negative statistical relationship between ϕ and m, we will make $\left[\underline{m}(\phi), \overline{m}(\phi)\right]$ weakly decreasing functions of ϕ. Finally, a positive relationship between ϕ and m arises when $\left[\underline{m}(\phi_i), \overline{m}(\phi_i)\right]$ are weakly increasing functions of ϕ. All figures are drawn holding c fixed.

Looking at Figure A.1, Panel 1, we first examine the case where m and ϕ are independent, conditional on c. That is, the upper and lower bounds on m are constant. The support of ϕ and m is partitioned by three key sets. The first is the x-axis, where $m = 0$. In the absence of the new benefit, those above this line leave the firm, and those below the line stay. The next is the 45 degree line where $\phi = m$. This line determines the leave/stay decision in a similar fashion for those under the new benefit. Finally, we have the vertical line where $\phi = c$. Workers to the left of the line remain in the default benefit, while those to the right of the line switch into the new benefit. Leavers are indicated by a darker shade of gray than stayers.

The incentive effect is represented by area D_1. These individuals would have left the firm were

Table 1: Descriptive Statistics

Employment Group Ages Years 	Union & Non-Union All Ages 1999 to 2002 1	Union All Ages 1999 to 2002 2	Union 40 to 50 1999 to 2002 3	Union 40 to 50 2002 4	Union 40 to 50 1999 to 2001 5
Leave within 1 year	0.138	0.0702	0.0494	0.0414	0.0519
	(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)
Asian/Am. Indian/Other	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)
Observations	8981	4217	1499	362	1137

Notes: Sample mean listed above; standard deviation in parentheses

Table 2: Implications of Empirical Observations for Employee Heterogeneity

Empirical Observation	No Sorting Effect	Negative Selection Effect	Positive Selection Effect
OLS $<$ 0	X	X	X
OLS $\geq$ 0			X
OLS $<$ DRD	X	X	X
OLS $\geq$ DRD			X

Note: Table summarizes results from Section 2.

Table 3: Union Employees, 2002: One-Year Leave Probability

OLS	(1)	(2)	(3)	(4)	(5)	(6)
DC	0.017	0.046	0.048	-0.016	-0.016	-0.007
	(0.022)	(0.030)	(0.039)	(0.028)	(0.034)	(0.037)
First Stage	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	0.455***	0.612***	0.623***	0.496***	0.492***	0.768***
	(0.076)	(0.142)	(0.236)	(0.050)	(0.091)	(0.158)
Reduced Form	(13)	(14)	(15)	(16)	(17)	(18)
Treatment	-0.052**	-0.025	-0.093	-0.022	-0.069*	-0.021
	(0.026)	(0.055)	(0.074)	(0.022)	(0.038)	(0.049)
Fuzzy RD	(19)	(20)	(21)	(22)	(23)	(24)
DC	-0.114**	-0.040	-0.149	-0.045	-0.140*	-0.028
	(0.057)	(0.081)	(0.112)	(0.043)	(0.080)	(0.060)
Mean Leave Prob	0.026	0.026	0.026	0.041	0.041	0.041
Bandwidth	5	5	5	10	10	10
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
$f(Age)$	None	Linear	Cubic	None	Linear	Cubic
N	196	196	196	362	362	362
First Stage F-stat	35.5	18.5	6.98	99.5	29	23.5
Hausman Test	0.052	0.416	0.114	0.565	0.150	0.798

Note: Sample includes union employees in the year 2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is instrumented for using discontinuity in default retirement benefit at the age of 45. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 4: Union Employees, 1999 - 2002: One-Year Leave Probability

OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DC	0.015	0.014	0.013	0.015	-0.011	-0.011	-0.011	-0.010
	(0.027)	(0.027)	(0.027)	(0.027)	(0.025)	(0.025)	(0.025)	(0.025)
First Stage	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treatment	0.519***	0.518***	0.519***	0.518***	0.543***	0.543***	0.543***	0.543***
	(0.060)	(0.060)	(0.060)	(0.060)	(0.043)	(0.043)	(0.043)	(0.043)
Reduced Form	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Treatment	-0.056**	-0.056**	-0.056**	-0.056**	-0.043*	-0.043*	-0.043*	-0.043*
	(0.028)	(0.028)	(0.028)	(0.028)	(0.025)	(0.025)	(0.025)	(0.025)
Fuzzy RD	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
DC	-0.108**	-0.109**	-0.107**	-0.109**	-0.080*	-0.080*	-0.079*	-0.079*
	(0.054)	(0.054)	(0.054)	(0.054)	(0.045)	(0.045)	(0.045)	(0.045)
Mean Leave Prob	0.045	0.045	0.045	0.045	0.049	0.049	0.049	0.049
Bandwidth	5	5	5	5	10	10	10	10
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$f(Age)$	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par
N	815	815	815	815	1,499	1,499	1,499	1,499
First Stage F-stat	74.1	74.4	74.8	74.2	156	157	157	157
Hausman Test	0.039	0.041	0.045	0.040	0.153	0.145	0.155	0.151

Note: Sample includes union employees in the years 1999 - 2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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.

Table 5: Union Employees, 1999 - 2002: Two- and Three-Year Leave Probabilities

Union, 2002	(1)	(2)	(3)		(4)	(5)	(6)	
2 Year	-0.103	0.019	0.113		-0.067	-0.160	0.047	
	(0.069)	(0.106)	(0.222)		(0.050)	(0.101)	(0.113)	
Mean Leave Prob	0.046	0.046	0.046		0.058	0.058	0.058	
N	196	196	196		362	362	362	
3 Year	-0.080	-0.036	0.176		-0.068	-0.072	0.024	
	(0.089)	(0.136)	(0.290)		(0.061)	(0.124)	(0.160)	
Mean Leave Prob	0.087	0.087	0.087		0.094	0.094	0.094	
N	196	196	196		362	362	362	
Union, 1999-2002	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**
2 Year	-0.167**	-0.168**	-0.165**	-0.167**	-0.147**	-0.147**	-0.147**	-0.145**
	(0.080)	(0.080)	(0.079)	(0.080)	(0.061)	(0.061)	(0.061)	(0.061)
Mean Leave Prob	0.088	0.088	0.088	0.088	0.092	0.092	0.092	0.092
N	611	611	611	611	1,129	1,129	1,129	1,129
3 Year	-0.141	-0.141	-0.139	-0.140	-0.165*	-0.166*	-0.162*	-0.160*
	(0.115)	(0.115)	(0.113)	(0.114)	(0.087)	(0.086)	(0.087)	(0.086)
Mean Leave Prob	0.113	0.113	0.113	0.113	0.119	0.119	0.119	0.119
N	397	397	397	397	739	739	739	739
Bandwidth	5	5	5	5	10	10	10	10
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$f(Age)$	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par

Note: Sample includes union employees in the years 1999 -2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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.

Table 6: Union Employees, 1999 - 2002 Characteristics

	(1) Sample Mean	(2) DC Mean	(3) Complier Mean	(4) Sample Mean	(5) DC Mean	(6) Complier Mean
Female	0.128	0.192	0.095	0.128	0.198	0.102
	(0.027)	(0.041)	(0.076)	(0.026)	(0.031)	(0.054)
White	0.388	0.365	0.284	0.386	0.351	0.300
	(0.039)	(0.049)	(0.094)	(0.038)	(0.037)	(0.066)
Black	0.129	0.110	0.146	0.129	0.104	0.145
	(0.026)	(0.032)	(0.062)	(0.026)	(0.023)	(0.044)
Hispanic	0.357	0.330	0.475	0.356	0.375	0.468
	(0.038)	(0.048)	(0.094)	(0.037)	(0.038)	(0.069)
Other Race	0.126	0.195	0.095	0.128	0.170	0.087
	(0.027)	(0.041)	(0.076)	(0.026)	(0.030)	(0.051)
Tenure	9.022	10.140	10.582	9.026	10.427	10.051
	(0.532)	(0.637)	(1.193)	(0.524)	(0.478)	(0.862)
Hours/Wk	39.7	39.5	39.8	39.6	39.6	39.8
	(0.1)	(0.3)	(0.5)	(0.1)	(0.2)	(0.3)
Base Wage	45,533	49,659	49,116	45,549	50,322	48,311
	(872)	(1,351)	(2,480)	(859)	(1,041)	(1,818)
Leader	0.254	0.255	0.168	0.252	0.273	0.224
	(0.046)	(0.056)	(0.104)	(0.045)	(0.043)	(0.075)
Problem Solving	48.718	46.286	48.140	48.710	46.914	45.493*
	(1.046)	(1.248)	(2.402)	(1.024)	(0.928)	(1.655)
Critical Thinking	55.856	54.291	55.445	55.846	54.977	53.177
	(0.835)	(1.127)	(2.095)	(0.815)	(0.849)	(1.488)
Economics/Acct	21.007	22.085	19.120	21.016	22.104	20.105
	(0.890)	(1.239)	(2.303)	(0.870)	(0.916)	(1.610)
Predicted Leave	0.079	0.060	0.058*	0.079	0.054	0.056**
	(0.005)	(0.007)	(0.012)	(0.005)	(0.005)	(0.008)
Bandwidth	5	5	5	10	10	10
$f(Age)$	Non-Par	Non-Par	Non-Par	Non-Par	Non-Par	Non-Par
N	815	815	815	1,499	1,499	1,499

Note: Sample includes union employees in the years 1999 - 2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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

Table 7: Union Employees, 1999 - 2002: Placebo One-Year Leave Probability *(Alternative Ages)*

Union, 2002	(1)	(2)	(3)		(4)	(5)	(6)	
First Stage	-0.037	0.035	0.326		0.052	0.012	-0.308	
	(0.065)	(0.133)	(0.252)		(0.062)	(0.132)	(0.262)	
Reduced Form	0.085**	0.106	0.085		-0.041	-0.066	0.047	
	(0.039)	(0.070)	(0.132)		(0.038)	(0.069)	(0.089)	
Fuzzy RD	-2.287	3.053	0.260		-0.782	-5.779	-0.153	
	(3.450)	(10.212)	(0.383)		(1.086)	(57.504)	(0.298)	
First Stage F-stat	0.332	0.068	1.675		0.707	0.008	1.376	
N	158	158	158		204	204	204	
Union, 1999-2002	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**	**(14)**
First Stage	-0.013	-0.013	-0.015	-0.013	0.044	0.045	0.044	0.045
	(0.064)	(0.064)	(0.064)	(0.064)	(0.057)	(0.057)	(0.056)	(0.057)
Reduced Form	0.047	0.049	0.047	0.050	-0.014	-0.014	-0.014	-0.014
	(0.043)	(0.043)	(0.043)	(0.043)	(0.034)	(0.034)	(0.034)	(0.034)
Fuzzy RD	0.000	0.000	-3.044	-3.933	-0.326	-0.311	-0.310	-0.308
	(0.025)	(0.026)	(12.112)	(18.954)	(0.876)	(0.852)	(0.870)	(0.854)
First Stage F-stat	0.039	0.040	0.058	0.039	0.606	0.620	0.600	0.619
N	662	662	662	662	837	837	837	837
Bandwidth	5	5	5	5	5	5	5	5
Threshold	42.5	42.5	42.5	42.5	47.5	47.5	47.5	47.5
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$f(Age)$	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par

Note: Sample includes union employees in the years 1999 -2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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.

Table 8: Union and Non-union Employees, 1999 - 2002: Placebo One-Year Leave Probability *(Alternative Years and Employment Groups)*

Union 1999-2001	(1) 1999	(2) 2000	(3) 2001		(4) 1999-2001
Reduced Form	-0.017	0.016	-0.007		-0.002
	(0.035)	(0.044)	(0.039)		(0.031)
N	619	619	619		619
Non-Union 1999-2002	**(5) 1999**	**(6) 2000**	**(7) 2001**	**(8) 2002**	**(9) 2002 vs. 1999-2001**
Reduced Form	-0.041	-0.057	0.027	0.087	0.106*
	(0.086)	(0.093)	(0.075)	(0.075)	(0.057)
N	560	560	560	560	560
Bandwidth	5	5	5	5	5
Demographics	Yes	Yes	Yes	Yes	Yes
$f(Age)$	Non-Par	Non-Par	Non-Par	Non-Par	Non-Par

Note: Sample includes union and non-union employees in the years 1999 - 2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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.

it not for the new benefit. In this case, those to the left of the benefit take-up line, $\phi = c$, provide an accurate counterfactual for those to the right of the line. Thus, the difference in leave probabilities among those who do not claim the benefit and those who do claim the benefit recovers the incentive effect.

We will denote this effect as follows:

$$\beta^1_{Endo} = \mathbb{E}_1 \left[Leave_i | \, B_i = 1, \text{Endo} \right] - \mathbb{E}_1 \left[Leave_i | \, B_i = 0, \text{Endo} \right]$$

where the 1 superscript and subscript refers to Panel 1 in the figure, and "Endo" condition refers to the fact that plan enrollment is endogenously determined. Graphically, this parameter can be quantified as follows:

$$\begin{aligned}
\beta^1_{Endo} &= \frac{E_1}{C_1 + D_1 + E_1} - \frac{B_1}{A_1 + B_1} \\
&= -\frac{D_1}{C_1 + D_1 + E_1} + \frac{D_1 + E_1}{C_1 + D_1 + E_1} - \frac{B_1}{A_1 + B_1} \\
&= -\frac{D_1}{C_1 + D_1 + E_1} + \frac{\overline{m}}{\overline{m} - \underline{m}} - \frac{\overline{m}}{\overline{m} - \underline{m}} \\
&= -\frac{D_1}{C_1 + D_1 + E_1} \\
&= -\frac{1/2 \left(\overline{m} + c \right)}{\overline{m} - \underline{m}}
\end{aligned}$$

We have just used the fact that given the assumption of a uniform distribution, the probability of being in any of the shaded regions is simply the area. In interpreting the above calculations, first note that baseline leave probability in the absence of the new benefit is $\overline{m} / \left(\overline{m} - \underline{m} \right)$. However, the incentive effect reduces this leave probability by: $-1/2 \left(\overline{m} + c \right) / \left(\overline{m} - \underline{m} \right)$. Focusing on the numerator, $1/2 \left(\overline{m} + c \right) = \mathbb{E} \left[\phi | \, B_i = 1 \right]$. Thus, the incentive effect identified in this experiment is proportional to the average value of the new benefit among those who take-up the benefit, i.e. a treatment among the treated. We now show two cases where a statistical relationship between ϕ and m will bias our estimation of this treatment effect.

In Figure A.1, Panel 2, the distribution of m shifts down, toward a higher probability of staying, as ϕ increases. Again, shaded areas represent a uniform distribution of individuals. As before, the set of individuals who no longer leave the firm because of the new benefit is represented by the area D_2. However, the probability of leaving among those who do not claim the benefit no longer provides an accurate counterfactual, i.e. we have a selection effect. We can see this by noting that in the absence of the new benefit the selection effect is represented as follows:

$$\begin{aligned}\mathbb{E}_2\left[Leave_i \middle| \phi_i \geq c_i, B_i = 0\right] - \mathbb{E}_2\left[Leave_i \middle| \phi_i < c_i, B_i = 0\right] &= \frac{E_2 + D_2}{C_2 + D_2 + E_2} - \frac{B_2}{A_2 + B_2} \\ &< 0\end{aligned}$$

where we have use the fact that $Leave_i = \mathbb{1}\left\{m_i > 0\right\}$ when $B_i = 0$.

Now define a new parameter β^2_{Endo} as:

$$\beta^2_{Endo} = \mathbb{E}_2\left[Leave_i \middle| B_i = 1, \text{Endo}\right] - \mathbb{E}_2\left[Leave_i \middle| B_i = 0, \text{Endo}\right]$$

where, again, the 2 superscript and subscript refers to Panel 2. We can show that:

$$\begin{aligned}\beta^2_{Endo} &= \frac{E_2}{C_2 + D_2 + E_2} - \frac{B_2}{A_2 + B_2} \\ &< \frac{E_1}{C_1 + D_1 + E_1} - \frac{B_1}{A_1 + B_1} \\ &= -\frac{D_1}{C_1 + D_1 + E_1} \\ &= \beta^1_{Endo}\end{aligned}$$

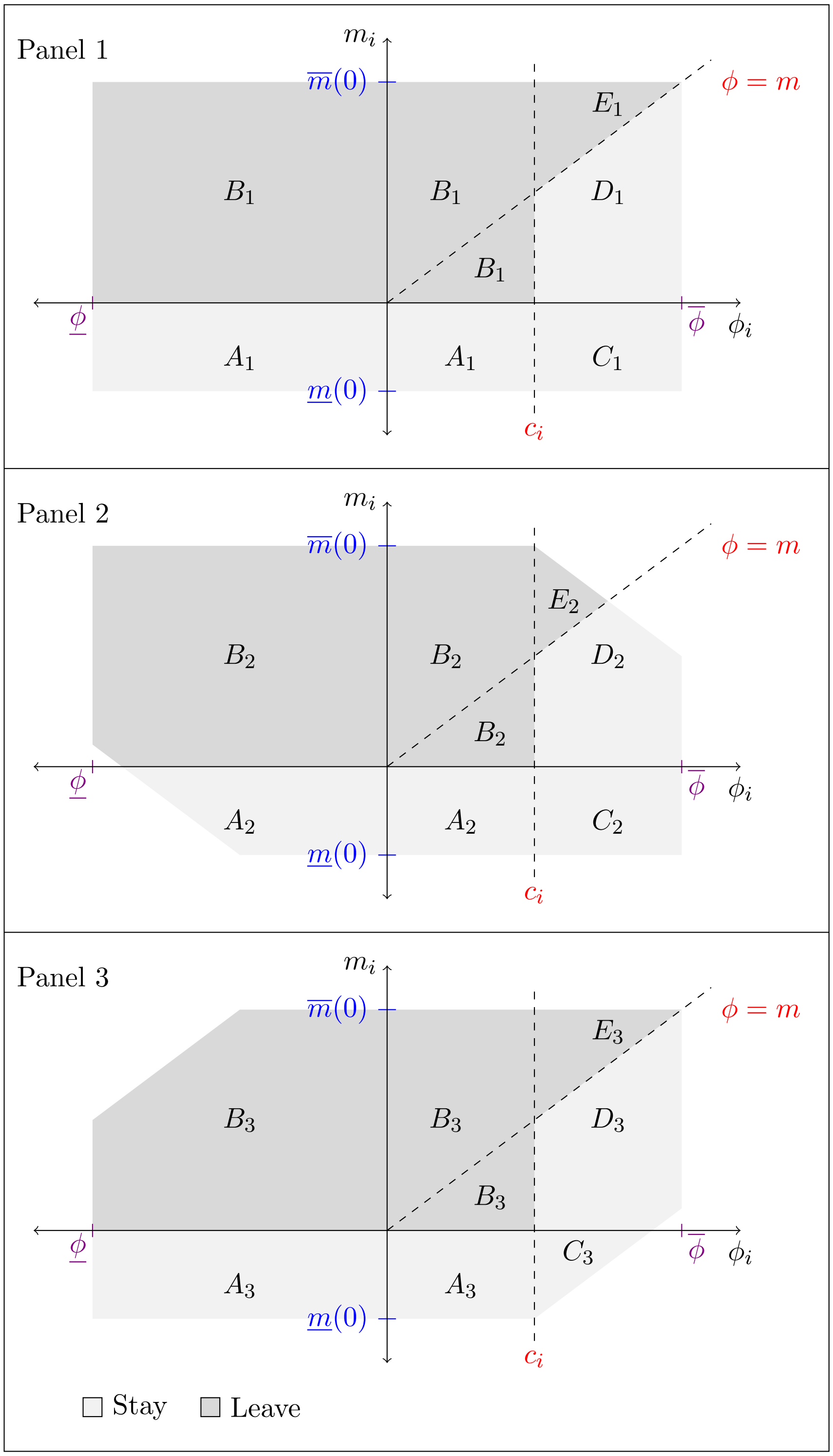
where we have used comparisons to Panel 1 to bound the difference in leave probabilities. In this case, the reduction in leave probabilities is exaggerated due to the underlying relationship between ϕ and m. Intuitively, when those who chose to be enroll in the new benefit are also less likley to leave the firm for other reasons, then both the direct effect and selection effect work in concert to create a negative correlation between plan enrollment and leaving. Note, in both Panels 1 and 2, we expect to observe a negative correlation between leave probabilities and new benefit participation. Finally, in Panel 3, we can likewise show that:

$$
\begin{aligned}
\beta^3_{Endo} &= \mathbb{E}_3\left[Leave_i \middle| B_i = 1, \text{Endo}\right] - \mathbb{E}_3\left[Leave_i \middle| B_i = 0, \text{Endo}\right] \\
&= \frac{E_3}{C_3 + D_2 + E_3} - \frac{B_3}{A_3 + B_3} \\
&> \frac{E_1}{C_1 + D_1 + E_1} - \frac{B_1}{A_1 + B_1} \\
&= -\frac{D_1}{C_1 + D_1 + E_1} \\
&= \beta^1_{Endo}
\end{aligned}
$$

It follows that the reduction in leave probabilities will be underestimated when the relationship between ϕ and m is positive. That is, the selection effect works in the opposite direction of the incentive effect.

Now the observed correlation between leave probabilities and new benefit participation is ambiguous. In particular, this is the only in case in which a positive correlation between leaving and new benefit enrollment may appear. It is this sense in which a positive an observed correlation relationship between leaving and new benefit enrollment helps to distinguish between the panels in Figure A.1. On the other hand, an observed negative correlation between leaving and new benefit enrollment is not informative.

Figure A.1: Case 1, Endogenous Benefit Enrollment



Appendix A.2 Simple Illustration – Exogenous Case

We now turn to our second thought experiment where benefit enrollment is exogenously determined. This is depicted in Figure A.2. The figure illustrates the ex post leave patterns of employees, once all employees have been enrolled into the new benefit. As before, the darker shade of gray indicates an employee who will leave the firm. Now, with everyone enrolled, the decision to leave for all employees depends on the plane $\phi = m$. We will compare these leave patterns to those that would prevail in the absence of the benefit. In that counterfactual case, all agents with $m > 0$ would have left the firm.

Relative to Figure A.1, the new benefit is taken up over a range of values for ϕ that includes lower and even negative values. As such, we predict the direct effect of benefit enrollment on leaving to be attenuated and potentially even reversed in sign. For example, consider Panel 1, where the distribution of m is independent of ϕ. For those with $\phi < 0$, the new benefit actually increases the likelihood of leaving relative to the counterfactual with no new benefit. This increase in leaving is represented by area D_1. For those with a positive value of ϕ, the probability of leaving has been decreased, as represented by area C_1. The net effect of exogenously enrolling all employees is now ambiguous. Formally, that effect is:

$$\begin{aligned}
\beta^1_{Exog} &= \mathbb{E}_1\left[Leave_i | B_i = 1, \text{Exog}\right] - \mathbb{E}_1\left[Leave_i | B_i = 0, \text{Exog}\right] \\
&= \frac{G_1 + J_1 + M_1 + N_1}{F_1 + \cdots + N_1} - \frac{H_1 + G_1 + J_1 + K_1 + M_1}{F_1 + \cdots + N_1} \\
&= \frac{N_1 - K_1 - H_1}{F_1 + \cdots + N_1}
\end{aligned}$$

where the "Exog" condition makes explicit that enrollment is now exogenously assigned. Comparing Panel 1 in Figure A.2 to its analgo in Figure A.1, we can show that when m is independent of ϕ, the exogenous effect of benefit enrollment on leaving is less negative than in the endogenous case:

$$\begin{aligned}
\beta^1_{Exog} &= \frac{N_1 - K_1 - H_1}{F_1 + \cdots + N_1} \\
&= \Pr{}_1(\phi \geq c)\frac{-H_1}{F_1 + G_1 + H_1} + \Pr{}_1(\phi < c)\frac{N_1 - K_1}{I_1 + \cdots + N_1} \\
&> \Pr{}_1(\phi \geq c)\frac{-H_1}{F_1 + G_1 + H_1} + \Pr{}_1(\phi < c)\frac{-H_1}{F_1 + G_1 + H_1} \\
&= \frac{-H_1}{F_1 + G_1 + H_1} \\
&= \frac{-D_1}{C_1 + D_1 + E_1} \\
&= \beta^1_{Endo}
\end{aligned}$$

where in the second line, we have decomposed the effect into the effect on those with $\phi \geq c$ and those with $\phi < c$. In the third line, we have used the fact that the effect on those with the highest value of ϕ will cause a greater reduction in leave probabilities than the mixture of an effect among those with lower but positive values of ϕ (K_1) and negative values of ϕ (N_1).

Now consider Panel 2, where a negative relationship between ϕ and m is introduced. We can show that the effect estimated in the endogenous case will still be more negative than the effect estimated in the exogenous case:

$$\begin{aligned}
\beta^2_{Exog} &= \mathbb{E}_2\left[Leave_i|\, B_i = 1, \text{Exog}\right] - \mathbb{E}_2\left[Leave_i|\, B_i = 0, \text{Exog}\right] \\
&= \frac{N_2 - K_2 - H_2}{F_2 + \cdots + N_2} \\
&= \Pr{}_2(\phi \geq c)\frac{-H_2}{F_2 + G_2 + H_2} + \Pr{}_2(\phi < c)\frac{N_2 - K_2}{I_2 + \cdots + N_2} \\
&> \Pr{}_2(\phi \geq c)\frac{-H_1}{F_1 + G_1 + H_1} + \Pr{}_2(\phi < c)\frac{N_2 - K_2}{I_2 + \cdots + N_2} \\
&> \Pr{}_2(\phi \geq c)\frac{-H_1}{F_1 + G_1 + H_1} + \Pr{}_2(\phi < c)\frac{-H_1}{F_1 + G_1 + H_1} \\
&= \frac{-H_1}{F_1 + G_1 + H_1} \\
&= \beta^1_{Endo} \\
&> \beta^2_{Endo}
\end{aligned}$$

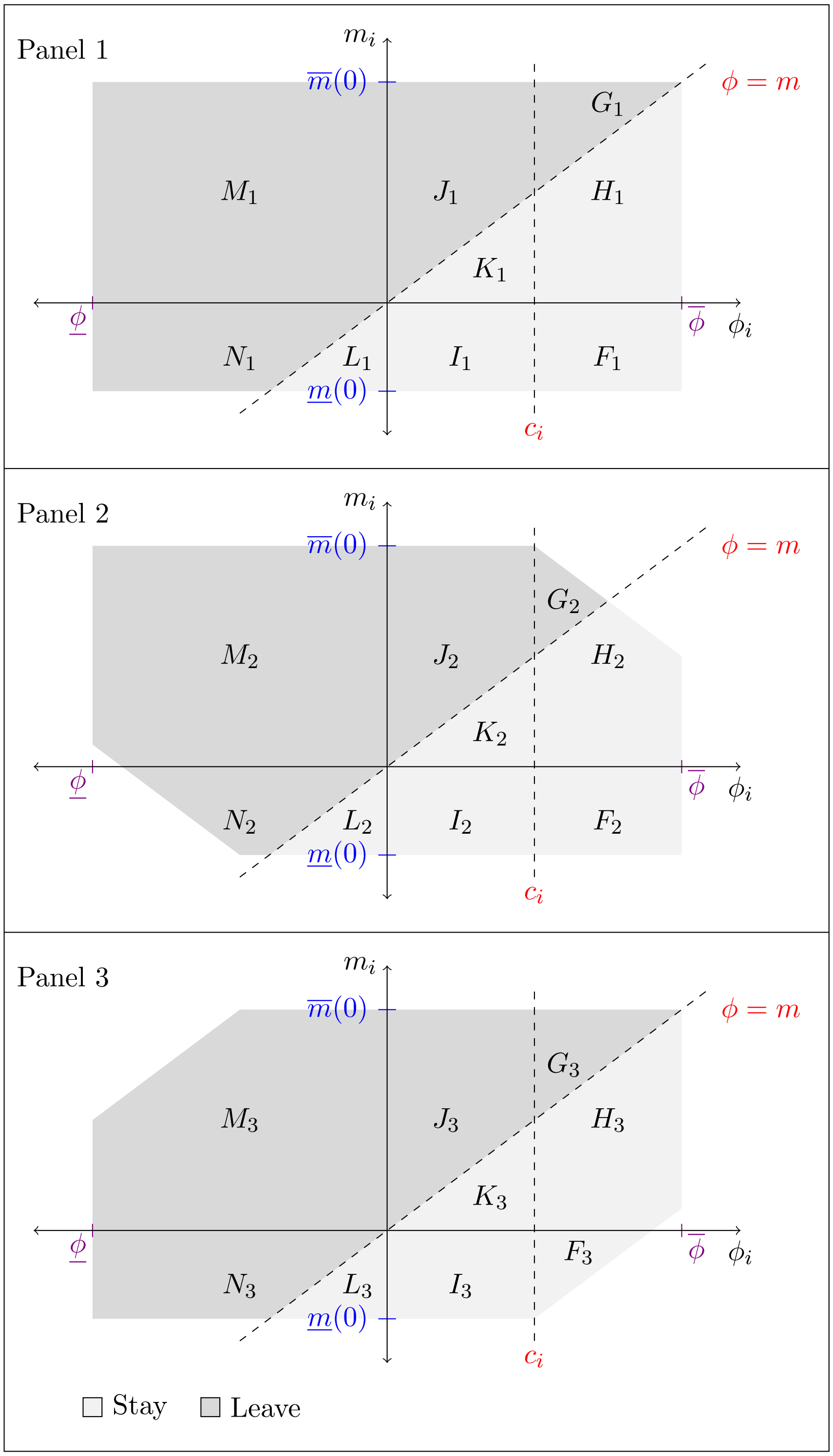
Here we have decomposed the effect again, and, in the fourth line, we have used the fact that the direct effect among those those with $\phi \geq c$ is attenuated in Panel 2 relative to Panel 1. Intuitively, when those who have a high value of ϕ also have tend to have a low value of m, the reduction in leave probabilities is smaller, because these individuals are likely to stay with the firm anyway. In the fifth line, we use the fact that the reduction in leave probabilities for those with $\phi \geq c$ in Panel 1 (H_1) is greater than that for those with $\phi < c$ in Panel 2, which is averaged over a group with lower values of ϕ (K_2) and a group with negative values of ϕ (N_2). Finally, we use the fact that $\beta^1_{Endo} > \beta^2_{Endo}$, which was shown above.

Finally, we consider Panel 3, where there is a positive relationship between ϕ and m. First, we show that the difference in leave probabilities in Panel 3 is more negative than effect in Panel 1. That is:

$$\begin{aligned}
\beta^3_{Exog} &= \mathbb{E}_3 \left[Leave_i | \, B_i = 1, \text{Exog} \right] - \mathbb{E}_3 \left[Leave_i | \, B_i = 0, \text{Exog} \right] \\
&= \frac{N_3 - K_3 - H_3}{F_3 + \cdots + N_3} \\
&< \frac{N_1 - K_1 - H_1}{F_1 + \cdots + N_1} \\
&= \beta^1_{Exog}
\end{aligned}$$

where have relied on the fact that the reduction in H_1 in the denominator is more than offset by the reduction of F_1 and M_1 in our case. Moreover, it is possible for the exogenous effect in Panel 3 to become more negative than analogous endogenous effect. As before, a positive relationship between ϕ and m may be detected when $\beta_{Exog} < \beta_{Endo}$.

Figure A.2: Case 2, Exogenous Benefit Enrollment



Appendix A.3 Simple Illustration – LATE Effect Among Compliers

Thus far, we have compared an observation correlation obtained via OLS to a treatment effect obtained from a an exogenous change in benefit enrollment among all employees. However, in our actual empirical application, we use a two-stage, fuzzy regression discontinuity, which can be likened to an instrumental variables estimate. In this case, we identify a local average treatment effect (LATE) among the set of compliers, i.e. those for whom the default alters outcomes.

Within our model, we can illustrate the group whom this LATE effect applies. These are employees for whom the being defaulted to the old beneft plan will not result in a switch to the new benefit, even when the new benefit is preferred, i.e. those employees who satisfy the following:

$$0 < \phi < c$$

This group also includes employees for whom being defaulted to the new benefit will result in staying with that benefit even when the old benefit is more beneficial. The following holds or these employees:

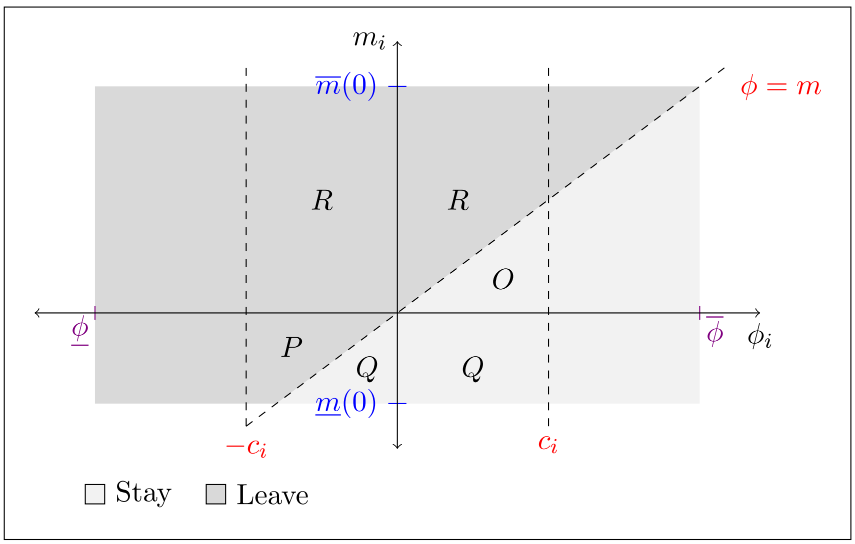
$$-c < \phi < 0$$

This group is graphically illustrated in Figure A.3. The treatment effect among this group is:

$$\begin{aligned}
\beta_{Comp} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Comp}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Comp}\right] \\
&= \frac{O + P}{O + P + Q + R}
\end{aligned}$$

Our results regarding a comparison between the endogenous case and the exogenous case hold when comparing the endogenous case to this effect among compliers.

Figure A.3: Case 3, Exogenous Benefit Enrollment Among Compliers



Appendix A.4 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:

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

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

$$\begin{aligned}\beta_{Exog} &= \mathbb{E}\left[Leave_i \middle| B_i = 1, \text{Exog}\right] - \mathbb{E}\left[Leave_i \middle| B_i = 0, \text{Exog}\right] \\ &= \pi_1\beta_1 + \pi_0\beta_0\end{aligned}$$

where π_1 is the share of employees that would choose the benefit voluntarily, and π_0 is the share of employees who would not enroll. Since we have assumed that $\beta_0 > \beta_1$, it follows that:

$$\beta_1 < \pi_1\beta_1 + \pi_0\beta_0$$

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.

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

The heterogeneity in treatment effects, then, prevents us from relying on the traiditional intution 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 Robustness Checks

Table B.1: Union & Non-Union Workers, 2002: One-Year Leave Probability

OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DC	0.020	0.029	0.030	0.026	-0.023	-0.017	-0.014	-0.016
	(0.035)	(0.035)	(0.035)	(0.035)	(0.033)	(0.033)	(0.033)	(0.033)
First Stage	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treatment	0.484***	0.480***	0.479***	0.479***	0.505***	0.500***	0.504***	0.507***
	(0.070)	(0.070)	(0.071)	(0.071)	(0.047)	(0.048)	(0.047)	(0.048)
Reduced Form	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Treatment	-0.113**	-0.105*	-0.104*	-0.107*	-0.081*	-0.072*	-0.072*	-0.075*
	(0.057)	(0.055)	(0.055)	(0.056)	(0.043)	(0.043)	(0.042)	(0.043)
Fuzzy RD	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
DC	-0.234**	-0.218*	-0.217*	-0.224**	-0.161*	-0.145*	-0.143*	-0.147*
	(0.115)	(0.112)	(0.112)	(0.114)	(0.082)	(0.083)	(0.082)	(0.082)
Mean Leave Prob	0.068	0.068	0.068	0.068	0.083	0.083	0.083	0.083
Bandwidth	5	5	5	5	10	10	10	10
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$f(Age)$	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par
N	369	369	369	369	702	702	702	702
First Stage F-stat	48	46.6	45.4	45.7	114	110	113	113
Hausman Test	0.025	0.028	0.029	0.029	0.092	0.129	0.120	0.112

Note: Sample includes union and non-union employees in 2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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.

Table B.2: Union & Non-Union Workers, 1999 - 2002: One-Year Leave Probability

OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DC	0.004	0.005	0.004	0.004	-0.020	-0.019	-0.018	-0.018
	(0.035)	(0.035)	(0.035)	(0.035)	(0.030)	(0.030)	(0.030)	(0.030)
First Stage	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treatment	0.516***	0.514***	0.513***	0.515***	0.543***	0.542***	0.541***	0.542***
	(0.061)	(0.061)	(0.061)	(0.061)	(0.044)	(0.044)	(0.044)	(0.044)
Reduced Form	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Treatment	-0.170***	-0.169***	-0.169***	-0.169***	-0.120**	-0.119**	-0.118**	-0.119**
	(0.064)	(0.064)	(0.063)	(0.064)	(0.049)	(0.049)	(0.049)	(0.049)
Fuzzy RD	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
DC	-0.329**	-0.329**	-0.330***	-0.329**	-0.221**	-0.220**	-0.218**	-0.219**
	(0.128)	(0.128)	(0.128)	(0.128)	(0.091)	(0.091)	(0.091)	(0.091)
Mean Leave Prob	0.076	0.076	0.076	0.076	0.078	0.078	0.078	0.078
Bandwidth	5	5	5	5	10	10	10	10
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$f(Age)$	None	Linear	Cubic	Non-Par	None	Linear	Cubic	Non-Par
N	1,375	1,375	1,375	1,375	2,575	2,575	2,575	2,575
First Stage F-stat	72.1	71.6	70.9	71.8	155	154	153	154
Hausman Test	0.005	0.005	0.005	0.005	0.021	0.021	0.022	0.021

Note: Sample includes union and non-union employees in the years 1999 -2002. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. DC is 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.

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