Retirement Plan Type and Employee Mobility: The Role of Selection*

Gopi Shah Goda
Stanford University
and NBER

Damon Jones
University of Chicago
and NBER

Colleen Flaherty Manchester
University of Minnesota

December 2015

Abstract

Relative to defined benefit (DB) plans, defined contribution (DC) plans have been linked to employee mobility due to differences in plan characteristics that make DC plans more portable. Because employees with different underlying mobility tendencies can select across plans or across firms with different types of plans, however, any observed relationship between plan type and mobility may, at least in part, be due to selection. We identify the role of selection by exploiting a natural experiment at an employer in which an employee's probability of transitioning from a DB to a DC pension plan was affected by default rules such that the DC plan was the default for younger employees, while the DB plan was the default for older employees. Using the default assignment as a source of exogenous variation in plan enrollment, we find that employees with higher mobility tendencies self-select into the DC plan (i.e., positive selection). While our

*Email: gopi@stanford.edu; damonjones@uchicago.edu; cmanch@umn.edu.

We would like to thank Dan Black, John Beshears, Jeff Brown, Kristine Brown, John Budd, Ezra Golberstein, Jeff Grogger, Caroline Hoxby, David Molitor, Luigi Pistaferri, Aaron Sojourner, participants at the University of Minnesota CHRLS and Population Center, Massachusetts Institute of Technology IWER, University of Wisconsin-La Crosse, and NBER Summer Institute research seminars, and the editor and three anonymous referees 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.

results apply most aptly to employers undergoing a transition in plan offerings, they suggest that selection likely contributes to the observed positive relationship between the broader transition from DB to DC plans and employee mobility in the labor market.

JEL Classification: J3, J6, M5

1 Introduction

In this paper, we identify the role of selection in the context of retirement plans and employee mobility. While our setting is an employer undergoing a plan transition, it is informative about the relationship between two broader labor market trends: first, the change in the pension plan landscape and second, increased employee mobility. 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). Concurrently, employee job tenure and retention rates have decreased (Munnell, Haverstick and Sanzenbacher, 2006; Farber, 2007; Friedberg and Owyang, 2005). It is commonly thought that the increase in DC plans has played a role in this increase in mobility because DB and DC plans typically differ in how employee tenure relates to pension wealth. In particular, pension wealth accrual is typically more backloaded in DB plans relative to DC plans, which acts to reduce the relative portability of DB plans (Mitchell, 1982; Lazear, 1990). While 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, difference in the portability of the plans has naturally been the focus when relating plan type to mobility trends.

Reseachers have identified a relationship between retirement plan type and job tenure such that employees in DB plans tend to have longer tenure relative to those in DC plans (Munnell, Haverstick and Sanzenbacher, 2006; Friedberg and Owyang, 2005). However, the selection of employees 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 employees 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. Separating the direct effects of program participation from the effects generated by selection into participation has been a topic of interest in many different contexts, yet is typically challenging to achieve due to a lack of random assignment. We exploit exogenous variation in the probability of switching to the DC plan caused by a 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. Default rules 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). Furthermore, Goda and Manchester (2013) find that the effect of default provisions on plan enrollment is similarly powerful.[1]

We use the default retirement plan rules as an instrument for DC enrollment in order to overcome selection bias and identify the effect of plan type on employee mobility. We conduct a differences-in-differences (DD) estimation strategy that uses data before and after the plan transition and leverages the difference in default plan for employees under and over age 45 to isolate the role of selection. While conceptually the features of the default rule governing the plan transition point to the use of regression discontinuity estimation methods, empirically we have limited power to implement this technique due to the combination of low

[1] Goda and Manchester (2013) document default effects in this same context using administrative data on plan enrollment from the year of the plan transition. This previous paper focuses on the effects of the default assignment on plan enrollment and the policy question of how to select the optimal age-based default rule given assumptions about employee risk preferences, financial market returns, and employee mobility. The present study evaluates the effect of plan type on subsequent mobility decisions using distinct data on employee turnover collected from before and after the plan transition and relies on the variation in plan enrollment resulting from the default rule in the estimation strategy.

baseline mobility rates and a limited number of employees in the narrow window around age 45.[2] We sign the selection effect by comparing selection-corrected estimates to the simple probit estimates, which are confounded by employee selection. We reject the null hypothesis that our simple probit estimates are less than the selection-corrected estimates ($p < 0.01$) for one-, two- and three-year mobility outcomes. These findings provide evidence that employees with higher mobility tendencies select into the DC plan over the DB plan.

This paper contributes to the literature on pension plan type and mobility in three ways. First, the paper provides 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. Our approach uses exogenous variation induced by the default rule governing the plan transition in a Difference-in-Differences (DD) framework. This technique relies on the assumption that before and after the policy change, trends in unobservable determinants of mobility for affected employees relative to unaffected (but otherwise similar) employees did not differ on either side of the age governing the default plan. Our identifying assumption passes falsification tests which generally show no evidence of differential mobility on either side of alternative age thresholds or in years prior to the policy change. We also confirm that no one particular year of data from the pre-period is driving the results.

Second, 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

[2] Goda, Jones and Manchester (2013) uses a technique that applies a regression discontinuity approach with data on mobility prior to the transition year and finds results qualitatively similar to but less precise than those presented here.

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 observable mobility patterns across plans. 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, some 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). While Aaronson and Coronado (2005) theorizes that changes in employee benefit demand may have driven, at least in part, the transition from DB to DC plans, they do not present a conceptual model or empirical strategy for isolating a selection effect. 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 induces a positive relationship between mobility and endogenous DC plan enrollment. Interestingly, the DC plan is not associated with higher turnover once we account for this selection effect — i.e., exogenous assignment to the DC plan reduces mobility relative to the DB plan. This finding highlights the possibility that employees find the bundle of DC plan features, including increased control, transparent wealth accrual, and loan and withdrawal provisions, desirable relative to those of the DB plan (as measured by higher retention), which is in line with previous work that finds a low perceived benefit of additional DB benefits (Fitzpatrick, 2015; Brown et al., 2011).

Third, 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 pattern of positive selection effects and negative incentive effects are consistent across the one-, two-, and three-year time horizons for measuring mobility.

Our results are specific to our context and the specific features of the DB and DC plans under consideration. However, there are reasons to suggest that our results are more generally relevant for a number reasons. First, one key difference between the DB plan in our context and the standard DB plan is that the benefit formula in our setting is less backloaded, which weakens the relationship between tenure and pension wealth accrual. However, this feature biases us against finding evidence of positive selection into DC plans based on mobility tendencies relative to a setting with a more standard DB plan. Second, the theoretical framework can be applied to traditional DB plans and to employer-provided benefits beyond retirement plans, and our framework provides intuition as to how results may vary with changes in the setting. Third, the employer we study — a large private university — is diverse and features a range of employees across occupations and job categories, including service workers, technical employees, and skilled and unskilled positions.

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 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 the paper.

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 describes individual decisions regarding enrollment in a newly-offered benefit and subsequent turnover. Second, we evaluate the observable implications of this framework in two distinct choice scenarios for benefit enrollment: one where benefit enrollment is endogenously chosen and one where it is exogenously determined. Finally, we show how comparing the relationship between new benefit enrollment and turnover in these two scenarios provides insight into the selection effect (i.e., the relationship between underlying mobility tendencies and preferences for the new benefit).[3]

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 will map these unobservable parameters into empirical outcomes. We define B_i to be a binary variable indicating enrollment in the new benefit at one's current employer and L_i, for "Leaving," 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

[3]The model is similar in spirit to a Roy (1951) model where individuals self-select into a given state — e.g. industry — based on the potentially heterogeneous returns to that state. In that case, the econometric recovery of the effect of said state on outcomes — e.g. earnings — is obfuscated by the correlation between determinants of the outcome and selection into a given state. Such models have been applied in many contexts — e.g. the study of selection into higher education (Willis and Rosen, 1979) or selection into immigration (Borjas, 1987). In our case, individuals may select into a type of retirement plan based on the value of that plan, and this potentially heterogeneous value subsequently affects the likelihood of staying with the firm.

DC retirement plan rather than the DB plan, while $L_i = 1$ indicates that an individual has subsequently left the firm within one, two, or three years of being initially observed.[4]

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.[5] 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.

When enrollment is determined solely by the employee, she must pay a cost of switching to the new benefit, $c_i > 0$, in order to realize this utility change. This may include such costs as time, informational requirements or administrative hurdles 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 $V_i^o(w_i^o, B_i^o)$ 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

[4] In Appendix A.1, we recast our model using a potential outcomes framework, similar to Imbens and Angrist (1994). Leaving is a function of benefit enrollment — $L_i = L_i(B_i)$ — and benefit enrollment is in turn a function of the benefit regime — $B_i = B_i(Z_i)$, for $Z_i \in \{0, 1, 2\}$. When the DB plan is the default, but employees may switch to a DC plan, $Z_i = 0$; when all employees must enroll in the DB plan, $Z_i = 1$; and when all employees must enroll in the DC plan, $Z_i = 2$.

[5] Note that the i subscript allows for flexible differences in expected utility due to a host of unobservable traits beyond one's wage.

having the old benefit:

$$m_i \equiv \mathbb{E}\left[V_i^o\left(w_i^o, B_i^o\right)\right] - \mathbb{E}\left[V_i(w_i, 0)\right] - \eta_i, \tag{3}$$

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:

$$L_i = \begin{cases} 1 & \text{if } \phi_i \cdot B_i < m_i \\ 0 & \text{if } \phi_i \cdot B_i \geq m_i. \end{cases} \tag{4}$$

It may seem that we have suppressed the dynamic nature of the these two decisions made at different points in time. However, we have placed little restriction on the joint distribution of (ϕ_i, m_i, c_i). Thus, the correlation between these reduced-form parameters can capture forward-looking behavior. For example, agents with a high likelihood of leaving the firm — i.e. a high m_i — may generally have a low value of enrollment — i.e. ϕ_i low relative to c_i — because they will not be at the firm for long.

We have also assumed that the benefit does not directly affect utility at outside firms. This may be violated when a new benefit is more portable than the old, as is typically the case with DC plans. We discuss in more detail in Appendix A.3 the case where this phenomenon is captured by having the benefit directly affect the cost of switching — i.e. $\eta_i = \eta_i\left(B_i\right)$. In our particular setting, the DB plan is less backloaded than typical DB plans, and thus our simplification may be less of a concern. Additionally, we replicate our results in Appendix C.2 among a sample of employees who would be vested under either the DB or DC plan, which further reduces any discrepancies in portability.

We now consider two choice scenarios. In the first case, B_i is endogenously determined by the employees and the default policy is a DB plan. In this case, enrollment is determined according to Equation 2. In the second case, benefit enrollment is exogenously determined by the employer. 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 4. Consider a comparison of the subsequent leave probabilities among those enrolled and those not enrolled. We define this difference as:

$$\beta_{Endo} \equiv \beta_1 + \beta_{Selection}, \tag{5}$$

where the "Endo" subscript denotes endogenously determined benefit enrollment.[6] We have decomposed this observed difference into a treatment on the treated (β_1) and a selection effect ($\beta_{Selection}$). The treatment on the treated can be interpreted as the treatment effect among those who enroll, i.e. those for whom $B_i = 1$ in Equation 2. The selection effect is the baseline difference in L_i between those who enroll when given the choice (i.e. $B_i = 1$) and those who do not (i.e. $B_i = 0$).

To build intuition regarding this decomposition, note that those who have chosen to enroll (i.e. $B_i = 1$) 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 4, 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 refer to this direct effect of the new benefit on

[6] Continuing with the potential outcomes framework mentioned in footnote 4 and described in detail in Appendix A.1:
$\beta_{Endo} \equiv \mathbb{E}\left[L_i(1)|\, B_i(0)=1\right] - \mathbb{E}\left[L_i(0)|\, B_i(0)=0\right]$
$= \underbrace{\mathbb{E}\left[L_i(1)-L_i(0)|\, B_i(0)=1\right]}_{\beta_1} + \underbrace{\{\mathbb{E}\left[L_i(0)|\, B_i(0)=1\right] - \mathbb{E}\left[L_i(0)|\, B_i(0)=0\right]\}}_{\beta_{Selection}}.$

the likelihood of leaving, β_1, 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.

We now turn to the second component of the decomposition in Equation 5. Focusing on the right-hand sides of the inequalities in Equation 4, 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 refer to the difference in leave probabilities due to differences in the distribution of m_i between enrollees and non-enrollees as the "selection effect" or $\beta_{Selection}$. The sign of the selection effect depends on the baseline difference in leave probabilities absent the new benefit.

Now consider the second choice scenario, where benefit enrollment is exogenously determined. 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 4. However, now that employees are not self-selecting 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. 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 $0 \geq m_i > \phi_i$.

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:[7]

$$\beta_{Exog} \equiv \pi_0\beta_0 + \pi_1\beta_1. \tag{6}$$

Note, we have decomposed the treatment effect into an effect among employees who would enroll endogenously — $B_i = 1$ — and those who would not — $B_i = 0$. The incentive effect β_1 is the treatment on the treated, defined previously in Equation 5, while the incentive effect β_0 is an analogous treatment on the untreated. The weights π_0 and π_1 are the population shares of the two respective types.

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 are related to 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 weakly higher values for the benefit, and therefore experience larger reductions in the probability of leaving due to the incentive effect, all things equal.[8] 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

[7] Within the potential outcomes framework from footnote 4 and Appendix A.1, we have:
$$\beta_{Exog} \equiv \mathbb{E}\left[L_i(1) - L_i(0)\right] = \underbrace{\Pr\left(B_i(0) = 0\right)}_{\pi_0} \cdot \underbrace{\mathbb{E}\left[L_i(1) - L_i(0) \middle| B_i(0) = 0\right]}_{\beta_0}$$
$$+ \underbrace{\Pr\left(B_i(0) = 1\right)}_{\pi_1} \cdot \underbrace{\mathbb{E}\left[L_i(1) - L_i(0) \middle| B_i(0) = 1\right]}_{\beta_1}.$$

[8] 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.

endogenous and exogenous estimates, potentially even reversing the relative magnitude of β_{Endo} and β_{Exog}.

In Appendix A we formally show in Proposition 1 that if we are able to observe the relationship between plan enrollment and leaving in these two settings, we can learn something about the role of selection. In particular, the selection effect is bounded from below by the difference between the relationship estimated in endogenous and exogenous settings:

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

To see why this is the case, substitute for the β_{Endo} and β_{Exog} using Equations (5) and (6) as follows:

$$\begin{aligned} \beta_{Endo} - \beta_{Exog} &= \beta_1 + \beta_{Selection} - \pi_0 \cdot \beta_0 - \pi_1 \cdot \beta_1 \\ &= \beta_{Selection} + \pi_0 \left[\beta_1 - \beta_0\right]. \end{aligned} \tag{8}$$

We argue in the appendix that the second term in (8) is negative, thus establishing the lower bound on $\beta_{Selection}$. We cannot directly verify the assumption that $\beta_1 - \beta_0 < 0$. For benefits that primarily affect payoffs during employment with the current firm, such as health insurance, it makes sense to assume that the new benefit is less likely to make those who would actively choose the benefit to leave the firm than those who would not choose the benefit. As discussed above, one potentially relevant violation of our assumption is the case where the new benefit directly affects the cost of leaving the firm through, for example, greater portability. In our context, however, it happens to be the case that the DB plan is less backloaded than typical DB plans, and therefore, this may be less of a concern.

Importantly, Equation 7 shows that the key test for positive selection is asymmetric in that a negative or zero difference (i.e. $\beta_{Endo} \leq \beta_{Exog}$) is not informative about the sign of the selection effect.[9] This is because the selection effect could reinforce or counteract the

[9]This may seem counterintuitive given the standard approach of signing omitted variable bias. However, the standard omitted variable bias intuition does not hold in the presence of heterogeneous treatment effects

incentive effect. Only in the case where the exogenous estimates show a larger reduction in leave probabilities than the endogenous estimates (i.e. $\beta_{Endo} > \beta_{Exog}$), can we rule out a zero or negative selection effect in favor of a positive selection effect. A regression of mobility (i.e. L_i) on new benefit enrollment among employees who can choose their benefit approximates the endogenous case. As shown above in Equation 5, the correlation between L_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 (i.e., Equation 6).[10] The effect of B_i on L_i in that case identifies the average incentive effect. The resulting estimates can then be used to evaluate the key inequality in (7).

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 employees underwent a plan transition on September 1, 2002. Existing employees in this group 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.[11] Our analysis is restricted to these existing employees. If no election was made, the employee was enrolled in

and selection on treatment.

[10] In Section 4 we outline our econometric methodology, Two-Stage Residual Inclusion, which allows us to account for endogenous benefit enrollment while accommodating a nonlinear specification. While the method technically implies that we recover an average treatment effect, one may be inclined to interpret our results as a local average treatment effect. In Appendix A.4 we discuss this alternative interpretation in more detail and show how it would alter our results.

[11] This choice applied to union employees hired before January 1, 2001, and governed future benefit accruals only. All non-union employees hired after this date were enrolled in the DC plan.

the default plan. The default plan 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.[12]

The DB plan at the employer offered benefits equal to 2% of an employees average salary, multiplied by the total years of service. 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 less backloaded than is typically 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.[13] Employer contributions to the DC plan were considered vested immediately for employees in our sample.

How does our setting compare to other employers? One key difference between the DB plan in our context and the standard DB plan is that the benefit formula in our setting is less backloaded, which weakens the relationship between tenure and pension wealth accrual. In addition, our university setting is not typical in that the benefits and working conditions are likely superior to other private sector employers. Given this, the employees in our sample are likely to have lower underlying mobility tendencies than most private sector employees. Moreover, other benefits (such as health care, education benefits, etc.) may mitigate employees responses to any change in one particular benefit. We expect that both of these factors would bias us against finding evidence of positive selection into DC plans based on mobility tendencies.

An important dimension to consider when evaluating the effects of the plan transition on employee mobility is the relative generosity of the two plans. Because of differences in how pension wealth accrues, the relative value of the two plans depends on employee characteristics, such as risk preference, financial literacy, and mobility tendencies as well as

[12]Non-union employees were subject to an earlier plan transition on January 1, 1997. However, our data do not span this earlier policy change. 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.

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

the performance of financial markets. Goda and Manchester (2013) carefully evaluate the value of the two plans by comparing the certainty equivalents under a base set of assumptions and show that, among low levels of risk aversion, the plans are of comparable value around age 45 and the DC plan is more valuable to younger employees and the DB plan is more valuable to older employees. For higher levels of risk aversion, the DB plan has a higher certainty equivalent than the DC plan across all ages.

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 plan 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.[14] Pension plan records include information on annual plan enrollment as well as which plan was the default plan for employees who were eligible for the plan transition. Our outcome measures are binary variables that indicate whether an employee we observe in year t is present in the dataset in a future year for one-, two-, and three-year time horizons. As such, it measures the probability of leaving the employer, either voluntarily or involuntarily, within one, two, or three years.

While conceptually the variation in default plan by age aligns well with a regression discontinuity approach, our power is limited due to low baseline leave propensities and a relatively small sample size.[15] Instead, we employ a DD method using the transition year (2002) and pre-transition data (1999-2001). Table 1 reports summary statistics for the sample broken out by the relevant differences used in the analysis. Column (1) reports summary statistics for employees in all years and of all ages. Columns (2) and (3) report descriptive statistics for employees under 45 and employees 45 and older, respectively, for the

[14]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).

[15]See Goda, Jones and Manchester (2013) for results using a fuzzy regression discontinuity.

pre-transition years, while columns (4) and (5) show this same comparison for the transition year. Comparing the leave propensities across the columns, the data show a sizable drop in leave propensities among the under 45 employees (i.e. column (2) vs. (4)), while mobility rates for employees age 45 and over are relatively stable over this time period (i.e. column (3) vs. (5)). At the same time, the second row of Table 1 shows how DC enrollment went up dramatically for employees under age 45. Figure 1 plots one-year mobility and DC enrollment by year for these same two employee groups. Both employees above and below age 45 experienced a drop in mobility in 2001, prior to the plan transition. However, while the mobility of employees under age 45 (who became enrolled in the DC plan to a greater degree) continued to decline, the mobility of those over 45 increased slightly in 2002 following the plan transition. Overall, these descriptive results suggest that employees who ended up in the DC plan exhibited a greater *decrease* in leave propensities relative to employees who remained in the DB plan.

4 Empirical Strategy

We quantify the role of selection as outlined in Section 2 by estimating 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 Section 2, we exploit the variation in DC enrollment produced by the different default plan for employees on either side of age 45 in 2002. In what follows we describe our empirical strategy and how the resulting estimates map to Equation 7, which is the key inequality from our model.

Because we have a binary outcome with a mean relatively close to zero, we rely on a probit specification.[16]

$$L_i = \mathbf{1}\left\{\lambda_{Exog} DC_i + \lambda_1 Post2002_i + \lambda_2 Under45_i + \Gamma_1 X_i + u_i > 0\right\} \tag{9}$$

where L_i is a binary variable that equals one if the employee is not with the employer one year later. We also consider specifications that measure leaving two and three years later. The variable DC_i is a dummy equal to one if employee i is in a DC plan. The variable $Post2002_i$ is an indicator for being observed in the year 2002, and 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.[17] The vector X_i consists of demographic control variables for gender, race, hours, base salary, tenure at the employer and dummies for department. We also include specifications where X_i includes a series of age and year dummy variables, omitting $Post2002_i$ and $Under45_i$, to control more flexibly for age and year. Finally, $u_i \sim \mathcal{N}(0,1)$ is an unobserved factor related to mobility. We first consider the following probit specification:

$$\mathbb{E}\left[L_i | X_i\right] = \Phi\left(\lambda^*_{Endo} DC_i + \lambda^*_1 Post2002_i + \lambda^*_2 Under45_i + \Gamma^*_2 X_i\right). \tag{10}$$

In our case DC_i may be an endogenous regressor, i.e. DC_i and u_i may be correlated. The naïve probit regression of L_i on DC_i, $Post2002_i$, $Under45_i$ and X_i will be inconsistent, and in particular, the coefficient on DC_i will be biased. We define β_{Endo} as the average partial effect of DC_i on L_i, estimated from this endogenous regression:

$$\beta_{Endo} = \mathbb{E}\Big[\Phi\left(\lambda^*_{Endo} + \lambda^*_1 Post2002_i + \lambda^*_2 Under45_i + \Gamma^*_2 X_i\right)$$

[16]We obtain qualitatively similar results using a linear probability model and 2SLS — we consistently reject our null hypothesis in favor of positive selection. However, we tend to estimate incentive effects much larger in magnitude, most likely owing to the fact that our outcome variable is binary and has a relatively low baseline mean. Linear probability model results are available from the authors upon request.

[17]There are two possible ways to define "over" and "under" groups based on the cutoff of age 45 on September 1, 2002: cohort (e.g., age 44 on September 1, 2001), and age (e.g., age 45 on September 1, 2001)). Our base set of results are estimated using the cohort definition; however, our results are robust to defining $Under45_i$ based on age rather than cohort.

$$-\Phi\left(\lambda_1^* Post2002_i + \lambda_2^* Under45_i + \Gamma_2^* X_i\right)\Bigg]. \qquad (11)$$

In order to address the endogeneity issue, we apply a control function approach, sometimes referred to as Two-Stage Residual Inclusion (or 2SRI, Terza, Basu and Rathouz, 2008), using default retirement plan rules as an instrument for DC enrollment. Specifically, our instrument for DC enrollment is the interaction term $Post2002_i \times Under45_i$. Recall that this group has the DC plan as the default retirement plan. We briefly summarize the procedure here and show in more detail in Appendix B how this method allows us to overcome the endogeneity. In the first stage, we estimate the effect of the default provision on DC participation for those under 45 in 2002 relative to those over 45 in 2002 using a linear model. In the second stage, we estimate the effect of DC participation on the one-year turnover probability while including the residual from the first stage. Our first stage regression is as follows:

$$DC_i = \gamma_{DC} Post2002_i \times Under45_i + \gamma_1 Post2002_i + \gamma_2 Under45_i + \Gamma_3 X_i + v_i \qquad (12)$$

The residual from the first stage regression in (12) is then included as a control function in the following probit specification:

$$\mathbb{E}\left[L_i | X_i\right] = \Phi\left(\lambda_{Exog} DC_i + \lambda_1 Post2002_i + \lambda_2 Under45_i + \Gamma_1 X_i + \alpha \hat{v}_i\right) \qquad (13)$$

We define β_{Exog} as the average partial effect estimated using the parameters in (13):

$$\begin{aligned}\beta_{Exog} = \mathbb{E}\Big[&\Phi\left(\lambda_{Exog} + \gamma_1 Post2002_i + \gamma_2 Under45_i + \Gamma_1 X_i + \alpha \hat{v}_i\right) \\ &-\Phi\left(\gamma_1 Post2002_i + \gamma_2 Under45_i + \Gamma_1 X_i + \alpha \hat{v}_i\right)\Big]. \qquad (14)\end{aligned}$$

To gain further intuition into our approach, note that if we were to specify a linear probability model instead of a probit model, this control function method would be equivalent to 2SLS estimation. Thus, we are relying on the standard IV assumptions for our instrument. Namely, we assume that conditional on predetermined observables, the instrument $Post2002_i \times Under45_i$ is independent of u_i and v_i. We justify this assumption on grounds similar to those of a difference-in-differences analysis. That is, we assume that in the absence of our policy, the difference in leave patterns between those older and younger than the cohort turning 45 in 2002 would have remained constant. We assess the validity of this assumption using placebo regressions described below.

In order to test the key inequality in (7) we first estimate the endogenous probit regression (10) via maximum likelihood estimation, with L_i as the outcome and DC_i, $Post2002_i$, $Under45_i$ and X_i as regressors. These resulting parameters are then used to calculate β_{Endo} with the sample analog of (11). Next, we implement the 2SRI estimator. In the first stage, we first estimate the linear equation in (12) and retain the residuals $\hat{v}_i$. In the second stage, we estimate (13) by fitting a probit regression via maximum likelihood, with L_i as the outcome and DC_i, $Post2002_i$, $Under45_i$, X_i and $\hat{v}_i$ as regressors. We then calculate β_{Exog} with the sample analog of (14). Standard errors are adjusted for the fact that the regressor $\hat{v}_i$ is estimated in the first stage regression.[18]

Our estimates provide proxies for the relationship between mobility and DC enrollment in the endogenous case (β_{Endo}) and the exogenous case (β_{Exog}) laid out in Section 2. The endogenous probit 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 of plans because the selection effect could reinforce or counteract the incentive effect. By Equation 7, we can rule out both a negative selection effect and no selection effect if we can reject the null hypothesis that the endogenous probit estimate is less than or equal to the 2SRI estimate (i.e., $H_0 : \beta_{Endo} \leq \beta_{Exog}$). Therefore, the key statistic for testing for positive selection is the p-value for this null hypothesis; we report this for each specification

[18]See for example Newey and McFadden (1994) for results on the asymptotic properties of two-step estimators.

in our results tables.[19]

We test the robustness of our results and the plausibility of our identification assumption in several ways. First, we eliminate various years prior to the plan transition to demonstrate that our results are not driven by a particular pre-transition year. Second, we perform a variety of falsification exercises where we either assume the plan transition occurred in a year prior to 2002 at the same age-45 threshold, or that the age threshold for the default assignment in 2002 was either lower or higher than age 45. Because there does not exist an analog to Equation (12) in these placebo exercises, we instead report the results of reduced-form regressions, which replace DC_i in Equation (10) with $Post2002_i \times Under45_i$. We also provide additional results using an alternative definition of our control group in the pre-transition years in Appendix C.1. Specifically, we show that our results are not driven by differences in leave probabilities among very young and very old workers by limiting the sample to ages close to 45 in Appendix C.3. Finally, we consider an alternative approach to accounting for an endogenous regressor in the context of a nonlinear model, namely a Local Average Response Function (or LARF, Abadie, 2003) in Appendix D. We are reassured by the fact that our results are virtually identical when using this independent method of addressing endogeneity.

5 Results

In this section, we report the results of our test for positive selection based on Equation 7. We evaluate these main results for one-, two-, and three-year leave outcomes. We also report our estimates of the incentive effect, i.e. the effect of DC enrollment on turnover after adjusting for selection. We then examine the robustness to dropping various pre-plan transition years from the analysis and provide an extensive analysis of placebo default assignments in different years or at different ages younger or older than age 45. Finally, we briefly discuss robustness to our definition of age, vesting status, other sample restrictions,

[19]Inference in this case is adjusted to take into account sample correlation between β_{Endo} and β_{Exog}.

and our method of addressing endogeneity.

5.1 The Selection Effect

We report our findings for the three measures of leave propensities. For each outcome, we start by reporting the inputs to the selection test, namely the average partial effect using the endogenous probit regression (Equation 11) in the first row, followed by the average partial effect using the exogenous 2SRI probit (Equation 14) in the second row. The third row provides the p-value of the test of the null hypothesis that $\beta_{Endo} \leq \beta_{Exog}$, which is the key inequality from Equation 7 for detecting positive selection. The results tables also include the mean leave probability prior to the default retirement plan change for each estimation sample and the F-statistic from the first stage regression. The three columns represent different combination of controls. The first column includes only controls for $Under45_i$ and $Post2002_i$. The second column adds controls for gender, race, hours, base salary, tenure at the employer and dummies for department. The third column mirrors the second column, but replaces $Under45_i$ and $Post2002_i$ with age and year fixed effects.

We begin by estimating the effect of DC plan enrollment relative to DB enrollment on the probability of leaving the employer within the next calendar year and report the results in Table 2. The endogenous estimate of the correlation between DC plan enrollment and leaving the employer is negative but not significantly different from zero in all three specifications. By contrast, β_{Exog} is negative and statistically significant. Together, these two estimates imply a p-value for our hypothesis test for selection that consistently allows us to reject the null hypothesis that $\beta_{Endo} \leq \beta_{Exog}$ at the 1-percent level. Thus, based on Equation 7, $\beta_{Selection} > 0$ — because $\beta_{Endo} - \beta_{Exog}$ is strictly positive. We therefore conclude that mobility tendencies are positively related to preferences for the DC plan relative to the DB plan (i.e., the selection effect is positive). Our results show a strong and robust first-stage relationship, as evidenced by the first stage F-statistics. Results from the first stage — not reported here — indicate that employees below the age-45 threshold are about 52 percentage points more likely to enroll in the DC plan than employees age 45 or older.

For two-year and three-year leave outcomes, we find a similar pattern of results. Tables 3 and 4 report estimates and the test for selection using the same format as Table 2, but use a dependent variable that measures whether the employee leaves the employer within two years or three years, respectively. For these analyses, the sample is substantially smaller than that used for the one-year time horizon in order to eliminate employees in the pre-transition period whose two- or three-year horizons cross the 2002 introduction date of the DC plan. Results from the two-year leave outcome consistently produce a negative and significant β_{Exog} and resoundingly reject the null hypothesis from Equation 7 in favor of positive selection at the 5 percent significance level. The results for the three-year leave outcome shown in Table 4 are consistent with the two-year outcome.

5.2 The Incentive Effect

While our focus above has been on identifying the role of selection, the results for β_{Exog} provide estimates of the incentive effect, i.e. the direct effect of the DC plan on mobility relative to the DB plan. Column (3) in Table 2 indicates that the incentive effect is negative and sizable in magnitude: the direct effect of the DC plan on mobility relative to the DB plan is a 5.6 percentage point reduction in one-year turnover, a 72 percent reduction relative to the mean.

Our negative incentive effect is counter to conventional wisdom that, relative to DB plans, DC plans ought to increase mobility due to greater portability. Our results suggest an alternative perspective, namely that other attributes of the benefit, such as individual control, liquidity, and transparency, may generally make this DC plan more attractive than the DB plan and increase the likelihood that one remains with the employer in a way that dominates portability. Furthermore, as mentioned above, the importance of portability is reduced in our context given that the DB plan is less backloaded than typical DB plans.[20]

[20]Note that our findings cannot be explained by inertia among employees affected by the default because

Finding a negative incentive effect can be further corroborated with research that shows employees place a low value on additional DB benefits: Fitzpatrick (2015) finds that public teachers in Illinois would trade just 20 cents in current compensation for an additional dollar of DB benefit (measured in present discounted value terms). We perform a back-of-the-envelope calculation to determine the valuation of DC benefits relative to DB benefits needed to square the magnitude of our incentive effect with existing estimates. Assuming such a low valuation of DB benefits, employees in our sample would have to only value a dollar of DC benefits at $0.55 in order to generate the patterns we observe in our sample.[21]

5.3 Robustness Checks

In this section, we first evaluate the robustness of our results by examining the sensitivity to removing various pre-transition years of data that serve as our control. Second, we perform falsification exercises in which we vary the year of the plan transition or the age threshold of the default assignment rule.

5.3.1 Robustness to removing control years

To assess whether a particular year of data from the control period is driving our results, we execute our estimation strategy using different combinations of pre-transition years. For our one-year leave outcome, our baseline analysis uses pre-transition data from 1999, 2000, and 2001, which implies that we can test the sensitivity of the estimates using 5 additional combinations of these data by eliminating one or two years of data at a time. We can repeat this for our two-year leave outcome, but can only consider two possible subsamples because our baseline analysis only uses 1999 and 2000 from the pre-transition period. We cannot

employees on either side of the age 45 cutoff faced a default plan.

[21] We use the methodology from Goda and Manchester (2013) to determine the average net present value of DB and DC benefits for employees in our data and a turnover elasticity estimate from Manchester (2012) inferred from the retention response to a different form of employee compensation at this same institution, tuition reimbursement benefits. We then scale the value of a dollar of DC benefits relative to a dollar of DB benefits until we match our incentive effect on turnover.

conduct this sensitively analysis for the three-year leave outcome because we only use one year of pre-transition data in the baseline analysis.[22]

We report β_{Endo}, β_{Exog} and the p-value for our test of positive selection for the baseline analysis and these alternative subsamples in Table 5 for the one-year leave outcome and in Table 6 for the two-year leave outcome. For each of these tables, we report results from estimation using the full set of controls.

For the one-year leave outcome (Table 5), we reject the null hypothesis that $\beta_{Endo} \leq \beta_{Exog}$ in favor of positive selection — all reported p-values are below the 5 percent significance level despite the reduced power. In addition, the β_{Exog} estimates in the various subsamples are similar to the baseline results. Results for the two-year outcome are shown in Table 6. We find consistent support for positive selection and a negative incentive effect for the two-year outcome, and the magnitudes of the negative incentive effects are in line with our main results.

Overall, these results indicate that our baseline empirical finding, namely that employees who choose DC plans over DB plans have higher underlying mobility tendencies, is not sensitive to the composition of the control group. In addition, the estimates show that eliminating one pre-transition year from the analysis results in qualitatively and quantitatively similar estimates for β_{Exog}.

5.3.2 Placebo plan transitions and default assignment rules

We do two sets of falsification exercises in order to check the plausibility of our identifying assumption, namely that trends in unobservable determinants of mobility rates before and after the transition did not differ on either side of the age-45 threshold. First, we eliminate the plan transition year (2002) from our analysis and impose a placebo plan transition in 2000 to see if we find any evidence that employees on either side of the age-45 threshold had differential mobility in 2000 relative to 1999. We also repeat this same exercise, but

[22]Note that all of the alternative subsamples use the year 2002 because all post-transition outcomes are measured relative to that year in the baseline analysis.

use 2001 as the placebo policy change year. Second, we assign a placebo age threshold that determines the default plan assignment and use data exclusively on one side of the original age-45 threshold to determine whether we find evidence of differential mobility rates on either side of the placebo age thresholds. Our identifying assumption implies that we should find no evidence of changes in our outcome variables in either the placebo plan transition years or at the placebo age thresholds.

Table 7 shows the results of our falsification exercises for all three leave outcomes. In each row we report the reduced form results for one-year leave probabilities, where the coefficient represents the effect of being on the lower side of the assumed age threshold in the assumed transition year on one-year mobility.[23] The top row reports the baseline reduced form results for comparison (i.e. using the threshold of age 45 in 2002) and shows highly significant estimates. The next two rows of results assume that the plan transition occurs in 2001 and 2000, respectively, with an age-45 threshold for the default plan assignment. Alternatively, rows 4 and 5 use the original policy year, but change the age threshold. The "Age 32.5 Placebo" limits the sample to those younger than age 45, while the "Age 57.5 Placebo" limits the sample to employees age 45 and older.

Of these 9 placebo tests, only one estimate — the three-year mobility rate for the lower age threshold — yields a coefficient that is significant at the 5 percent level. The remaining placebo coefficients in the table are statistically insignificant. Overall, the results from the falsification exercises provide support for the conclusion that our baseline findings on selection and incentive effects are likely to be a result of the retirement plan transition in 2002 that led to differences in retirement plan enrollment on either side of the age-45 threshold.

[23] For each of these falsification exercises, we report the reduced form results because a first stage is not possible for the falsification specifications.

5.3.3 Sensitivity to Alternative Age Definitions, Vesting Status, Sample Restrictions and Endogeneity Correction

In our study thus far, our estimation procedure uses employees' age as measured on September 1, 2002, the date of the plan transition. By assigning employees to either side of this age-45 threshold, we are able to evaluate the effect of the plan transition while controlling for any underlying differences between younger and older employees in this cohort. Alternatively, we could measure employee age in each calendar year and compare employees on either side of age 45 for that year. We report the results of plan enrollment on one-, two-, and three-year mobility rates using this alternative definition in Tables C.2-C.4 in Appendix C.1. The results are consistent with our cohort analysis: the p-value of our test for selection is significant at conventional levels for all of the specifications. As for the incentive effects, the β_{Exog} estimates remain negative and statistically significant, although they tend to be slightly lower.

One may be concerned that the positive selection 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, albeit weaker due to the reduced sample size (see Tables C.5-C.7 in Appendix C.2). 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.

We also explore the sensitivity of our results to alternative sample restrictions that exclude very young and very old workers for whom our main identification assumption, namely that there are parallel time trends in the mobility of both age groups in the absence of the transition to the DC plan, may be more tenuous. In Appendix C.3, we show both DC enrollment and 1-year mobility rates by year for a 5- and 10-year bandwidth on either side of age 45 in Figure C.1. We also show our main estimation results for both of these restricted samples in Table C.8. As shown in the figure, pre-transition mobility rates among those over and under age 45 are much more similar when limiting the sample to those between the ages of 35 and 55 (10-year bandwidth) and even more so when the sample is limited to those between 40 and 50 (5-year bandwidth). However, in both samples, mobility rates decline for the Over 45 group relative to the Under 45 group.

These results are confirmed in the estimates presented in Table C.8. The 5-year bandwidth samples produce estimates of β_{Exog} that are similar in magnitude to those presented in Table 2 when the whole sample is used; the estimates using the 10-year bandwidth are slightly closer to zero. The β_{Endo} estimates are also closer to zero for the restricted sample, and thus the difference between β_{Exog} and β_{Endo} for each bandwidth is comparable to that estimated using the full sample. Due to the reduced power from the smaller sample size, our tests for selection fail to reject the null hypothesis at the 5 percent level when age and year fixed effects are included in the regressions. The null hypothesis may also differ due to attenuation of the β_{Exog} estimates. However, the results generally provide evidence that differences in mobility rates for very young and very old workers are not driving our main results.

Finally, we describe in Appendix D an alternative approach to addressing endogeneity in our nonlinear model — the Local Average Response Function (LARF) approach (Abadie, 2003). We provide an overview of the method in Appendix D.1 and report results in Appendix D.2. The results in Tables D.9-D.11 are nearly identical to those of Tables 2-4. While the first row in the respective tables are by construction identical, the second rows rely on distinct approaches to accounting for endogeneity — in the case of 2SRI we rely on a control function approach, while in the case of LARF we use a reweighting procedure.[24] Technically, the similarity between the results may suggest that the effect among the "complier" population is similar to the average treatment effect. This may not be too surprising, given the fact that our first stage regression implies that more than half of the population are among the "complier" group.

[24] In the case of a linear probability model, both approaches are equivalent to 2SLS. However, in the context of a nonlinear model, the difference between these approaches is nontrivial.

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 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. 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 identification strategy relies on the assumption that employees exogenously induced to remain in the DB plan (i.e., employees age 45 or older) did not have underlying mobility tendencies that were different from employees who were exogenously induced to switch to the DC plan (i.e., emploeyes younger than age 45) before and after the employer's plan transition in 2002. Our empirical results combined with insights from our model indicate that preferences for DC plans are positively related to unobservable mobility tendencies.

While extrapolating from our single employer context to other settings may warrant caution, there are reasons to believe that our findings have some external validity. First, our theoretical framework allows us to intuitively consider how our results would vary in a setting with a more traditional, backloaded DB. Second, the employer in our study, a large university, features a diverse set of occupations covering a wide range of skill sets and responsibilities, making the results potentially applicable to a larger set of employers. At the same time, this employer offers more generous benefits relative to most private sector employers, which limits generalizability.

Our findings have a number of implications for mobility and the transition from DB to DC plans. First, our results provide evidence of positive selection into DC plans over DB 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. Taken directly, our finding implies that the selection effect is at least 3.6 percentage points — approximately half the one-year turnover rate in our setting. Comparing this to past findings that DC plans are associated with lower job tenure, on average, relative to DB plans (Munnell, Haverstick and Sanzenbacher, 2006), our results imply that the selection effect would fully explain this difference, although such a direct comparison warrants great caution.[25]

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 compensating 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. This suggests that one should not simply characterize the difference in plan features between DB and DC plans in terms of portability and accrual; rather, it is important to recognize that 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 finding combined with the multi-dimensional difference between the plans highlight the need for

[25] Munnell, Haverstick and Sanzenbacher (2006) find that employees in a DB-only (DC-only) plan have 4.0 (2.7) years more tenure, on average, relative to a baseline average job tenure of 8.4 years. If a constant hazard rate is assumed, the implied difference in hazard rates between DC and DB plans would be approximately 1.0 percentage points.

additional research to identify the role of rational and behavioral factors in the relationship between pension plan type and employee mobility.

References

Aaronson, Stephanie, and Julia Coronado. 2005. "Are Firms or Workers Behind the Shift Away from DB Pension Plans?" *Finance and Economics Discussion Series, Federal Reserve Board of Washington D.C., 2005-17.*

Abadie, Alberto. 2003. "Semiparametric instrumental variable estimation of treatment response models." *Journal of Econometrics*, 113(2): 231–263.

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.*

Borjas, George J. 1987. "Self-Selection and the Earnings of Immigrants." *The American Economic Review*, 77(4): 531–553.

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. 2015. "How Much Are Public School Teachers Willing to Pay for Their Retirement Benefits?" *American Economic Journal: Economic Policy,* forthcoming.

Friedberg, Leora, and Michael T. Owyang. 2005. "Explaining the Evolution of Pension Structure and Job Tenure." *NBER Working Paper 10714.*

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.

Goda, Gopi Shah, Damon Jones, and Colleen Flaherty Manchester. 2013. "Retirement Plan Type and Employee Mobility: Role of Selection and Incentive Effects." *NBER Working Paper 18902*.

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 Average 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." *Industrial Relations*, 29(2): 263–281.

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.

Manchester, Colleen Flaherty. 2012. "General Human Capital and Employee Mobility: How Tuition Reimbursement Increases Retention through Sorting and Participation." *Industrial and Labor Relations Review*, 65(4).

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.*

Newey, Whitney K, and Daniel McFadden. 1994. "Chapter 36 Large sample estimation and hypothesis testing." In *Handbook of Econometrics.* , ed. Robert F Engle McFadden and Daniel L, 2111–2245 T2 –. Elsevier.

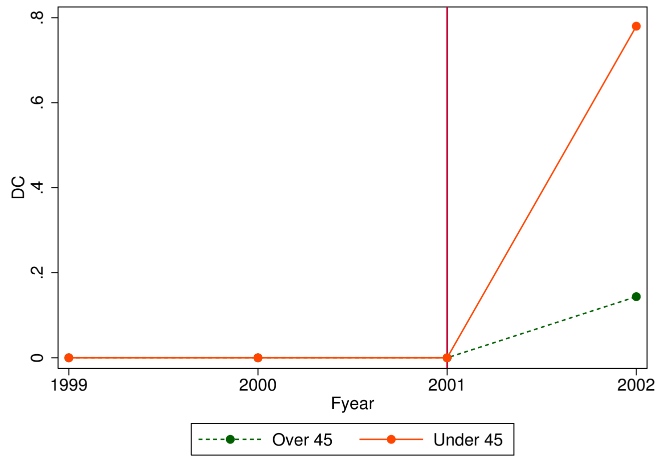
Rabe, Birgitta. 2007. "Occupational Pension, Wages, and Job Mobility in Germany." *Scottish Journal of Political Economy*, 54(4): 531–552.

Roy, A. D. 1951. "Some Thoughts on the Distribution of Earnings." *Oxford Economic Papers*, 3(2): 135–146.

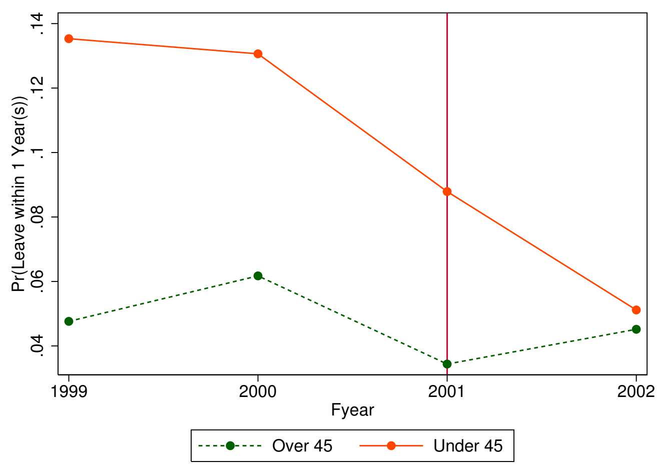
Terza, Joseph V, Anirban Basu, and Paul J Rathouz. 2008. "Two-stage residual inclusion estimation: Addressing endogeneity in health econometric modeling." *Journal of Health Economics*, 27(3): 531–543.

Willis, Robert J., and Sherwin Rosen. 1979. "Education and Self-Selection." *Journal of Political Economy*, 87(5): S7–S36.

Figure 1: DC Plan Enrollment and Probability of Leaving within One Year by Default Assignment: 1999-2002



(a) DC Plan Enrollment



(b) Probability of Leaving within One Year

Notes: Over 45 represents employees age 45 or older on September 1, 2002. Under 45 represents employees younger than age 45 on September 1, 2002. Employees over 45 were defaulted to remain in the DB plan for 2002 and later, while employees under 45 were defaulted to switch to the DC plan.

Table 1: Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)
	All Ages 1999 to 2002	Under 45 1999 to 2001	45 and Over 1999 to 2001	Under 45 2002	45 and Over 2002
Leave within 1 year	0.0701	0.119	0.0482	0.0512	0.0452
	(0.255)	(0.323)	(0.214)	(0.221)	(0.208)
DC Enrollment	0.0926	0	0	0.780	0.144
	(0.290)	(0)	(0)	(0.415)	(0.351)
Age on Sept. 1, 2002	47.33	36.84	54.50	37.23	54.24
	(10.88)	(6.217)	(6.979)	(6.135)	(6.919)
Female = 1	0.161	0.179	0.153	0.156	0.149
	(0.368)	(0.383)	(0.360)	(0.363)	(0.356)
Black	0.107	0.0959	0.117	0.0921	0.112
	(0.309)	(0.295)	(0.321)	(0.289)	(0.316)
Hispanic	0.280	0.332	0.241	0.358	0.239
	(0.449)	(0.471)	(0.428)	(0.480)	(0.427)
Asian/Am. Indian/Other	0.157	0.150	0.162	0.143	0.162
	(0.364)	(0.358)	(0.369)	(0.351)	(0.369)
Weekly hours	39.55	39.35	39.67	39.53	39.62
	(2.650)	(3.304)	(2.233)	(2.582)	(2.240)
Salary (in $1,000s)	46.57	42.01	47.73	47.75	52.20
	(12.99)	(12.22)	(12.47)	(12.65)	(13.32)
N	4223	1324	1910	391	598

Notes: Sample mean listed above; standard deviation in parentheses

Table 2: Effect of DC Plan on One-Year Leave Probability and Test for Selection

	(1)	(2)	(3)
β_{Endo}	-0.022	-0.020	-0.020
	(0.015)	(0.015)	(0.015)
β_{Exog}	-0.063***	-0.060***	-0.056***
	(0.015)	(0.014)	(0.015)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.001**	**0.002**	**0.005**
$\mathbb{E}[L_i]$	0.077	0.077	0.077
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	4,153	4,134	4,134
First Stage F-stat	622	622	626

Note: Sample includes employees in the years 1999 - 2002. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of Equation (7). Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 3: Effect of DC Plan on Two-Year Leave Probability and Test for Selection

	(1)	(2)	(3)
β_{Endo}	-0.024	-0.020	-0.021
	(0.023)	(0.023)	(0.023)
β_{Exog}	-0.128***	-0.126***	-0.121***
	(0.019)	(0.019)	(0.020)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**
$\mathbb{E}[L_i]$	0.146	0.146	0.146
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	3,137	3,123	3,123
First Stage F-stat	622	623	623

Note: Sample includes employees in the years 1999 - 2001 and 2002. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of Equation (7). Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 4: Effect of DC Plan on Three-Year Leave Probability and Test for Selection

	(1)	(2)	(3)
β_{Endo}	-0.033	-0.029	-0.029
	(0.025)	(0.025)	(0.025)
β_{Exog}	-0.177***	-0.177***	-0.171***
	(0.027)	(0.026)	(0.027)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**
$\mathbb{E}\left[L_i\right]$	0.187	0.187	0.187
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	2,040	2,039	2,039
First Stage F-stat	621	624	616

Note: Sample includes employees in the years 1999 and 2002. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of Equation (7). Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 5: Robustness Regressions: One-Year Leave Probability

	(1)	(2)	(3)	(4)	(5)
β_{Endo}	-0.024	-0.015	-0.025	-0.017	-0.010
	(0.017)	(0.016)	(0.016)	(0.018)	(0.014)
β_{Exog}	-0.068***	-0.051***	-0.082***	-0.064***	-0.050**
	(0.018)	(0.018)	(0.023)	(0.024)	(0.020)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.005**	**0.009**	**0.003**	**0.012**	**0.012**
$\mathbb{E}[L_i]$	0.091	0.075	0.089	0.096	0.058
Controls	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Years	1999,2000,2002	2000-2002	1999,2002	2000,2002	2001-2002
N	2,960	3,071	1,864	1,933	1,967
First Stage F-stat	599	628	578	586	621

Note: Samples vary as shown in the table. Column (1) uses the baseline sample. For further details see notes in Table 2. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 6: Robustness Regressions: Two-Year Leave Probability

	(1)	(2)	(3)	(4)
β_{Endo}	-0.021	-0.005	-0.019	-0.005
	(0.023)	(0.023)	(0.024)	(0.023)
β_{Exog}	-0.121***	-0.110***	-0.147***	-0.110***
	(0.020)	(0.026)	(0.026)	(0.026)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**	**0.000**
$\mathbb{E}\left[L_i\right]$	0.146	0.137	0.163	0.137
Controls	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Years	1999,2000,2002	2000-2002	1999,2002	2000,2002
N	3,123	2,060	1,925	2,060
First Stage F-stat	623	614	589	614

Note: Samples vary as shown in the table. Column (1) uses the baseline sample. For further details see notes in Table 3. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table 7: Reduced Form and Placebo Regressions

	(1) One-Year	(2) Two-Year	(3) Three-Year
Baseline (Reduced Form)	-0.040***	-0.086***	-0.119***
	(0.012)	(0.016)	(0.021)
2001 Placebo	-0.004		
	(0.019)		
2000 Placebo	-0.028	-0.023	
	(0.019)	(0.026)	
Age 32.5 Placebo	-0.011	-0.032	-0.125**
	(0.038)	(0.050)	(0.049)
Age 47.5 Placebo	0.002	-0.005	0.002
	(0.021)	(0.029)	(0.040)
Age FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: Samples vary according to specification. "Baseline (Reduced Form)" report reduced form results for the baseline sample. "2001 Placebo" and "2000 Placebo" drop 2002 and 2001-2002 from the baseline sample, respectively. "Age 32.5 Placebo" restricts the sample to ages 20-45. "Age 57.5 Placebo" restricts the baseline sample to ages 45-70. Demographic controls include gender, race, tenure dummies, department, hours worked per year and base pay rate. Standard errors are robust. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Appendix A: Model Discussion

A.1 Potential Outcomes Framework

In this section, we derive the equations in Section 2 using a potential outcomes framework, similar to Imbens and Angrist (1994). A distinct feature of our setting is that we will consider a multivalued instrument. We have a discrete instrument, $Z_i \in \{0, 1, 2\}$, which represents an exogenous factor that may influence benefit enrollment. When $Z_i = 0$, the default benefit is the DB plan, but employees may switch into a DC plan. When $Z_i = 1$ all employees must enroll in the DB plan, and when $Z_i = 2$ all employees must enroll in the DC plan. We make the following assumptions regarding the exogeneity of the instrument Z_i:

Assumption A.1 (Instrument Exogeneity)**.**

1.1. ***Independence:*** $\{L_i(B_i(0), 0), L_i(B_i(1), 1), L_i(B_i(2), 2), B_i(0), B_i(1), B_i(2)\} \perp\!\!\!\perp Z_i$*;*

1.2. ***Exclusion:*** $L_i(0, 0) = L_i(0, 1) \equiv L_i(0)$ *and* $L_i(1, 0) = L_i(1, 2) \equiv L_i(1)$*.*

Thus, the outcome of leaving is a function of benefit enrollment — $L_i(B_i) = B_i \cdot L_i(1) + (1 - B_i) \cdot L_i(0)$ — and benefit enrollment is a function of the instrument — $B_i(Z_i) = \mathbf{1}\{Z_i = 0\} \cdot B_i(0) + \mathbf{1}\{Z_i = 2\}$.

When enrollment is determined by the employee — i.e. $Z_i = 0$ — the decision rule to enroll is determined by Equation 2:

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

We now derive Equation 5:

$$\begin{aligned} \beta_{Endo} &\equiv \mathbb{E}[L_i | B_i = 1, Z_i = 0] - \mathbb{E}[L_i | B_i = 0, Z_i = 0] \\ &= \mathbb{E}[L_i(1) | B_i(0) = 1] - \mathbb{E}[L_i(0) | B_i(0) = 0] \end{aligned}$$

$$
\begin{aligned}
&= \mathbb{E}\left[L_i(1)|\, B_i(0)=1\right] - \mathbb{E}\left[L_i(0)|\, B_i(0)=1\right] \\
&\quad + \left\{\mathbb{E}\left[L_i(0)|\, B_i(0)=1\right] - \mathbb{E}\left[L_i(0)|\, B_i(0)=0\right]\right\} \\
&= \underbrace{\mathbb{E}\left[L_i(1)-L_i(0)|\, B_i(0)=1\right]}_{\beta_1} \\
&\quad + \underbrace{\left\{\mathbb{E}\left[L_i(0)|\, B_i(0)=1\right] - \mathbb{E}\left[L_i(0)|\, B_i(0)=0\right]\right\}}_{\beta_{Selection}}, \qquad \text{(A.2)}
\end{aligned}
$$

In the second line, we have relied on Assumptions 1.1 and 1.2.

In the exogenous case the employer's nudge completely determines enrollment in the new benefit — $B_i(1)=0$ and $B_i(2)=1$. We can thus show, as in Equation 6, that:

$$
\begin{aligned}
\beta_{Exog} &\equiv \mathbb{E}\left[L_i|\, B_i=1, Z_i=2\right] - \mathbb{E}\left[L_i|\, B_i=0, Z_i=1\right] \\
&= \mathbb{E}\left[L_i(1)-L_i(0)\right] \\
&= \Pr\left(B_i(0)=0\right)\cdot \underbrace{\mathbb{E}\left[L_i(1)-L_i(0)|\, B_i(0)=0\right]}_{\beta_0} \\
&\quad + \Pr\left(B_i(0)=1\right)\cdot \underbrace{\mathbb{E}\left[L_i(1)-L_i(0)|\, B_i(0)=1\right]}_{\beta_1} \\
&= \pi_0\beta_0 + \pi_1\beta_1, \qquad \text{(A.3)}
\end{aligned}
$$

where in the second line, we have again relied on Assumptions 1.1 and 1.2.

A.2 Derivation of a Lower Bound on the Selection Effect

As mentioned in Section 2 in the text, we can use the observed relationship between benefit enrollment and leaving under two distinct choice scenarios to establish a lower bound on the selection effect. We formally state this in the following proposition:

Proposition 1. *If the quasi-experimental estimate defined in Equation 6 is positive (i.e. $\beta_{Exog} \geq 0$) and the treatment on the treated is negative (i.e. $\beta_1 < 0$), 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 5) and exogenous (Equation 6) estimates is bounded from above by the selection effect defined in Equation 5. That is:*

$$\beta_{Endo} - \beta_{Exog} \leq \beta_{Selection}. \tag{A.4}$$

Before proving Proposition 1, we establish a useful lemma:

Lemma 1 (Selection and Observational Correlations)**.** *If the treatment on the treated is negative (i.e. $\beta_1 < 0$), then the observed difference in leave probabilities by benefit type (B_i) defined in (5) is bounded from above by the selection effect defined in (5). That is:*

$$\beta_{Endo} \leq \beta_{Selection} \tag{A.5}$$

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.

$$\begin{aligned} \beta_{Endo} &= \beta_1 + \beta_{Selection} \\ &\leq \beta_{Selection} \end{aligned}$$

where the first line was shown in Equation 5 and, in the second third line, we have used the assumption $\beta_1 < 0$. □

The assumption that $\beta_1 < 0$ is guaranteed in this version of the model, due to the nonnegative enrollment cost. To see that, note:

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

where the second line follows from Equation 4, and in the third line, we used the fact that $\phi_i \geq c_i \rightarrow \phi_i \geq 0$, since c_i is nonnegative.[26] Thus, the endogenous 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. Recall from Equation 6 that:

$$\beta_{Exog} = \pi_1\beta_1 + \pi_0\beta_0$$

Also, recall from above that $\beta_{Endo} = \beta_1 + \beta_{Selection}$. Next, the difference between β_{Endo} and β_{Exog} gives:

$$\begin{aligned}
\beta_{Endo} - \beta_{Exog} &= \beta_1 + \beta_{Selection} - \pi_1\beta_1 - \pi_0\beta_0 \\
&= \beta_{Selection} + (1-\pi_1)\,\beta_1 - \pi_0\beta_0 \\
&= \beta_{Selection} + \pi_0\left[\beta_1 - \beta_0\right]
\end{aligned}$$

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 $\beta_{Exog} \geq 0$, then

[26] Below in Section A.3 we relax the assumptions that ensure $\beta_1 < 0$ and show that our main result still holds.

we have:

$$
\begin{aligned}
0 &\leq \beta_{Exog} \\
&= \pi_1\beta_1 + \pi_0\beta_0 \\
&= \beta_1 - \pi_0\left[\beta_1 - \beta_0\right] \\
&\leq -\pi_0\left[\beta_1 - \beta_0\right] \\
\implies \left[\beta_1 - \beta_0\right] &\leq 0
\end{aligned}
$$

where in the fourth line we have used the assumption that $\beta_1 < 0$. Alternatively, we can just assume that $[\beta_1 - \beta_0]$ is negative. In either case, the result follows. $\square$

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 *who 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 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.[27]

[27] 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.

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. However, we show next in Appendix A.3 that Proposition 1 still holds in this case, so long as we still assume that $\beta_0 \geq \beta_1$.

A.3 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 $\mathbb{E}\left[V_i\left(w_i, B_i\right)\right]$. 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

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

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 $\tilde{\eta}\left(B_i\right)$ as follows:

$$\tilde{\eta}_i\left(B_i\right) \equiv B_i \cdot \eta_i$$

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:

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

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. That is, we may have $\beta_1 \geq 0$. This is the case, for example, when the new benefit does not have as demanding a vesting requirement. Nonetheless, 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. $\beta_0 \geq \beta_1$), then the difference between the endogenous (Equation 5) 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 above to show:

$$\beta_{Endo} - \beta_{Exog} = \beta_{Selection} + \pi_0 \left[\beta_1 - \beta_0\right],$$

and the result follows. □

A.4 Derivation of Lower Bound when a LATE is Estimated

As mentioned in Section 2, our results require an estimate of the average treatment effect, β_{Exog}, and our method of 2SRI technically recovers an average treatment effect. However, one may alternatively interpret our estimates as a local average treatment effect (LATE), which is a common interpretation of IV estimates (see e.g. Imbens and Angrist, 1994). In that case, we must use additional assumptions to establish a lower bound on the selection effect. To see this, redefine the instrument Z_i as a binary variable that takes a value of zero when the default is the DB plan and one when the default is the DC plan. We define the subpopulation of compliers as those who would enroll in the DB in the absence of this default, but who enroll in the DC plan in the presence of it — i.e. those for whom $B_i(1) > B_i(0)$. The LATE, then, is defined as follows:

$$\beta_{LATE} \equiv \mathbb{E}\left[L_i(1) - L_i(0) \middle| B_i(1) > B_i(0)\right]. \tag{A.6}$$

Note that our previously define treatment on the untreated, β_0 is related to the LATE as follows:

$$\begin{aligned}
\beta_0 &\equiv \mathbb{E}\left[L_i(1) - L_i(0) \middle| B_i(0) = 0\right] \\
&= \Pr\left(B_i(1) > B_i(0) \middle| B_i(0) = 0\right) \cdot \mathbb{E}\left[L_i(1) - L_i(0) \middle| B_i(1) > B_i(0)\right] \\
&\quad + \Pr\left(B_i(1) = B_i(0) = 0 \middle| B_i(0) = 0\right) \cdot \mathbb{E}\left[L_i(1) - L_i(0) \middle| B_i(1) = B_i(0) = 0\right] \\
&= \frac{\pi_C}{\pi_C + \pi_{NT}}\beta_C + \frac{\pi_{NT}}{\pi_C + \pi_{NT}}\beta_{NT} \\
&= \frac{\pi_C}{\pi_C + \pi_{NT}}\beta_{LATE} + \frac{\pi_{NT}}{\pi_C + \pi_{NT}}\beta_{NT},
\end{aligned} \tag{A.7}$$

where the "C" subscript denotes the complier subpopulation and the "NT" subscript refers to the "never-taker" subpopulation — those for whom $B_i(0) = B_i(1) = 0$. Rearranging terms from Equation (A.7), we have:

$$\begin{aligned}
\beta_{LATE} &= \frac{\pi_C + \pi_{NT}}{\pi_C}\beta_0 - \frac{\pi_{NT}}{\pi_C}\beta_{NT} \\
&= \beta_0 - \frac{\pi_{NT}}{\pi_C}\left(\beta_{NT} - \beta_0\right).
\end{aligned} \tag{A.8}$$

Suppose that $\beta_{Exog} = \beta_{LATE}$, then our key derivation is altered:

$$\begin{aligned}\beta_{Endo} - \beta_{Exog} &= \beta_1 + \beta_{Selection} - \beta_0 + \frac{\pi_{NT}}{\pi_C}(\beta_{NT} - \beta_0) \\ &= \beta_{Selection} + (\beta_1 - \beta_0) + \frac{\pi_{NT}}{\pi_C}(\beta_{NT} - \beta_0) \qquad \text{(A.9)}\end{aligned}$$

The assumption that $\beta_0 > \beta_1$ is now no longer sufficient to establish a lower bound, but rather we require that the sum of the second and third terms in (A.9) be negative.

In the main text, we maintain the assumption that our method recovers an average treatment effect. In a literal sense, the assumptions required to implement our 2SRI method imply that our estimates recover an average treatment. In addition, the standard results that equate IV estimates to a local average treatment effect, e.g. Imbens and Angrist (1994), do not technically apply in the case of a nonlinear specification, such as ours.

However, as we show below in Appendix D, there is an analogous method, the local average response function (LARF) method, that recovers an average treatment effect among the compliers, even in the case of a nonlinear specification. In that case, our empirical estimates are nearly identical to those using the 2SRI method. This either suggests that the effect among the compliers is comparable to the average treatment effect, or that the variation used to identify the 2SRI essentially recovers a local effect. In the former case, we are justified in interpreting our effect as an average treatment effect, while in the latter case, we are not.

Even if our method only identifies a local treatment effect, we have two additional arguments as to why our lower bound is likely to still hold. First, should the third additional term in Equation A.9 be positive, it is attenuated by a factor or π_{NT}/π_C which is roughly 1/3 in our sample, given our first stage results (available upon request). Second, we estimate the average characteristics of the complier subpopulation in Appendix A.5 below. In Table A.1 we compare the complier population to the general sample. We find evidence that compliers are lower tenured, and more likely to be Hispanic than the general sample; however, there is no evidence that they differ in their weekly hours, annual salary, or gender.

A.5 Complier Analysis

We provide some characteristics of the complier population by using the method described in Autor and Houseman (2005) to estimate the characteristics of the marginal DC enrollee, and report the results in Table A.1. Column (1) reports the means of various observable characteristics in our sample. Column (2) reports the estimated average characteristic of the "compliers," or 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. Column (3) reports the difference along with standard errors. In all cases, the estimates are regression-adjusted for age.

Table A.1: Estimated Complier Characteristics

	(1) Sample Average	(2) Complier Mean	(3) Difference
Female = 1	0.162***	0.172***	-0.010
	(0.006)	(0.032)	(0.031)
Black	0.107***	0.116***	-0.009
	(0.005)	(0.025)	(0.024)
Hispanic	0.278***	0.431***	-0.154***
	(0.007)	(0.040)	(0.039)
Asian/Am. Indian/Other	0.158***	0.117***	0.041
	(0.006)	(0.030)	(0.030)
Tenure	11.079***	6.763***	4.316***
	(0.143)	(0.469)	(0.476)
Weekly Hours	39.573***	39.458***	0.115
	(0.040)	(0.211)	(0.206)
Salary (in $1,000s)	46.584***	45.776***	0.808
	(0.201)	(1.064)	(1.047)
N	4,153	4,153	4,153

Appendix B: Two-Stage Residual Inclusion (2SRI)

Here we demonstrate the control function approach, Two-Stage Residual Inclusion (2SRI) (Terza, Basu and Rathouz, 2008, e.g. see). Suppose we have a binary outcome, Y_i, a key regressor of interest D_i, a set of predetermined covariates X_i and an instrument Z_i. The binary outcome is modeled using a standard probit model:

$$Y_i = \mathbf{1}\left\{\tilde{\lambda} D_i + \tilde{\Gamma}_1 X_i + u_i > 0\right\} \tag{B.10}$$

Under the assumption that $u_i \sim \mathcal{N}(0,1)$ is independent of (D_i, X_i), we have the following:

$$\mathbb{E}\left[Y_i \middle| D_i, X_i\right] = \Phi\left(\tilde{\lambda} D_i + \tilde{\Gamma}_1 X_i\right) \tag{B.11}$$

However, we are interested in the case where D_i may be and endogenous regressor, i.e. D_t and u_i may be correlated. In this case, the naïve probit regression of Y_i on D_i and X_i will be inconsistent, and in particular, the coefficient on D_i will be biased. Let (λ^*, Γ_1^*) be the parameters estimated from the naïve probit regression. Define the average partial effect of D_i on Y_i using the parameters from this naïve regression as:

$$\beta_{Endo} = \mathbb{E}\left[\Phi\left(\lambda^* + \Gamma_1^* X_i\right)\right] - \mathbb{E}\left[\Phi\left(\Gamma_1^* X_i\right)\right]. \tag{B.12}$$

The average partial effect will, by extension, also be biased. Consider the following ancillary regressions:

$$D_i = \gamma Z_i + \Gamma_2 X_i + v_i \tag{B.13}$$

$$u_i = \tilde{\alpha} v_i + e_i, \tag{B.14}$$

where $\tilde{\alpha} \neq 0$ captures the endogeneity of D_i. We make the following identifying assumptions:

Assumption B.1 (2SRI)**.**

B.1.1. ***First Stage:*** $\gamma \neq 0$.

B.1.2. ***Independence 1:*** *Conditional on X_i, Z_i is independent of (u_i, v_i, e_i).*

B.1.3. ***Independence 2:*** *v_i is independent of e_i.*

We now demonstrate identification of the average partial effect of D_i on Y_i. First, substitute for u_i in (B.10) using (B.14) and we have:

$$Y_i = \mathbf{1}\left\{\tilde{\lambda} D_i + \tilde{\Gamma}_1 X_i + \tilde{\alpha} v_i + e_i > 0\right\} \tag{B.15}$$

Note that the error term e_i is independent of the regressors. By normality of u_i, we have $e_i \sim \mathcal{N}(0, \sigma_e)$. Applying standard probit regression results, we have:

$$\begin{aligned} \mathbb{E}\left[Y_i \middle| D_i, X_i, v_i\right] &= \Pr\left(\tilde{\lambda} D_i + \tilde{\Gamma}_1 X_i + \tilde{\alpha} v_i + e_i > 0\right) \\ &= \Phi\left(\frac{\tilde{\lambda}}{\sigma_e} D_i + \frac{1}{\sigma_e}\tilde{\Gamma}_1 X_i + \frac{\tilde{\alpha}}{\sigma_e} v_i\right) \\ &= \Phi\left(\lambda D_i + \Gamma_1 X_i + \alpha v_i\right) \end{aligned} \tag{B.16}$$

We do not directly observe v_i, but we can obtain a consistent estimate using the residuals from a linear regression of D_i on Z_i and X_i, as per (B.13). We then estimate a probit regression of Y_i on D_i, X_i and the estimated v_i. The parameters from the probit estimation of (B.16) are then used to calculate the average partial effect of D_i on Y_i:

$$\beta_{Exog} = \mathbb{E}\Big[\Phi\left(\lambda + \Gamma_1 X_i + \alpha v_i\right) - \Phi\left(\Gamma_1 X_i + \alpha v_i\right)\Big]. \tag{B.17}$$

The variance covariance matrix for the estimate parameters are adjusted for the two-step procedure, using standard results (Newey and McFadden, 1994). Standard errors for the average partial effect are obtained via the delta method.

Appendix C: Supplemental Results

C.1 Robustness to Age Definition

Table C.2: Effect of DC Plan on One-Year Leave Probability and Test for Selection, Alternative Age Comparison

	(1)	(2)	(3)
β_{Endo}	-0.016	-0.013	-0.015
	(0.016)	(0.016)	(0.016)
β_{Exog}	-0.054***	-0.049***	-0.045**
	(0.017)	(0.017)	(0.018)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.005**	**0.012**	**0.031**
$\mathbb{E}[L_i]$	0.077	0.077	0.077
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	4,164	4,145	4,145
First Stage F-stat	622	621	628

Note: Sample includes employees in the years 1999 - 2002. DC is instrumented for using the difference in default pension plan type base on age of in the current year — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table C.3: Effect of DC Plan on Two-Year Leave Probability and Test for Selection, Alternative Age Comparison

	(1)	(2)	(3)
β_{Endo}	-0.012	0.001	-0.004
	(0.024)	(0.025)	(0.024)
β_{Exog}	-0.110***	-0.097***	-0.093***
	(0.022)	(0.024)	(0.024)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**
$\mathbb{E}[L_i]$	0.146	0.146	0.146
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	3,146	3,146	3,146
First Stage F-stat	622	618	624

Note: Sample includes employees in the years 1999 - 2001 and 2002. DC is instrumented for using the difference in default pension plan type base on age of in the current year — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table C.4: Effect of DC Plan on Three-Year Leave Probability and Test for Selection, Alternative Age Comparison

	(1)	(2)	(3)
β_{Endo}	-0.014	0.004	0.000
	(0.026)	(0.027)	(0.027)
β_{Exog}	-0.140***	-0.114***	-0.110***
	(0.032)	(0.035)	(0.036)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**
$\mathbb{E}[L_i]$	0.188	0.188	0.188
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	2,049	2,038	2,038
First Stage F-stat	621	590	592

Note: Sample includes employees in the years 1999 and 2002. DC is instrumented for using the difference in default pension plan type base on age of in the current year — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

C.2 Robustness to Vesting Status

Table C.5: Effect of DC Plan on One-Year Leave Probability and Test for Selection, Vested Sample

	(1)	(2)	(3)
β_{Endo}	-0.026*	-0.024	-0.024
	(0.015)	(0.016)	(0.016)
β_{Exog}	-0.049***	-0.049***	-0.046***
	(0.015)	(0.015)	(0.016)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.078**	**0.084**	**0.096**
$\mathbb{E}[L_i]$	0.060	0.060	0.060
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	2,688	2,630	2,630
First Stage F-stat	380	372	373

Note: Sample includes employees in the years 1999 - 2002 who have at least 5 years of service. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table C.6: Effect of DC Plan on Two-Year Leave Probability and Test for Selection, Vested Sample

	(1)	(2)	(3)
β_{Endo}	-0.034	-0.033	-0.034
	(0.025)	(0.025)	(0.024)
β_{Exog}	-0.102***	-0.106***	-0.103***
	(0.024)	(0.022)	(0.023)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.003**	**0.003**	**0.004**
$\mathbb{E}[L_i]$	0.126	0.126	0.126
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	2,023	2,004	2,004
First Stage F-stat	380	376	372

Note: Sample includes employees in the years 1999 - 2001 and 2002 who have at least 5 years of service. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

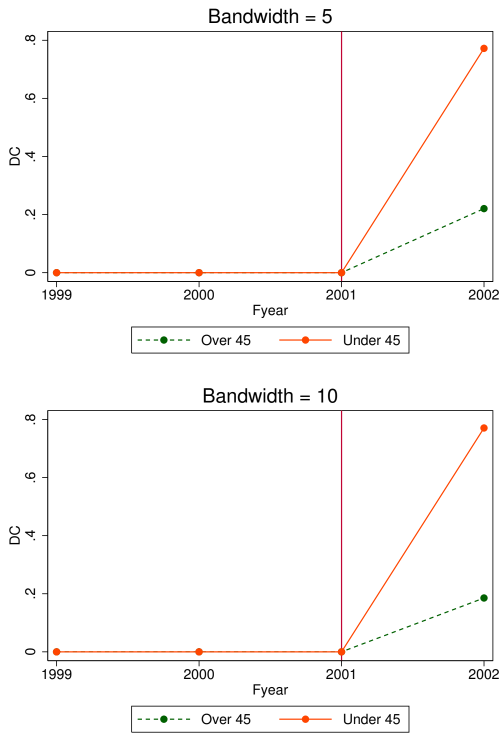
Table C.7: Effect of DC Plan on Three-Year Leave Probability and Test for Selection, Vested Sample

	(1)	(2)	(3)
β_{Endo}	-0.056**	-0.051*	-0.052*
	(0.028)	(0.028)	(0.028)
β_{Exog}	-0.149***	-0.152***	-0.148***
	(0.034)	(0.031)	(0.033)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.001**	**0.000**	**0.001**
$\mathbb{E}[L_i]$	0.171	0.169	0.169
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	1,361	1,354	1,354
First Stage F-stat	380	372	361

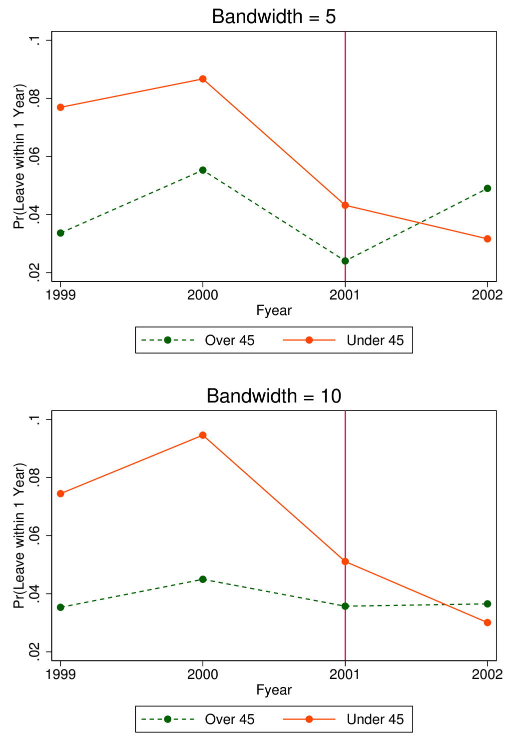
Note: Sample includes employees in the years 1999 and 2002 who have at least 5 years of service. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

C.3 Robustness to Bandwidth

Figure C.1: DC Plan Enrollment and Probability of Leaving within One Year by Default Assignment: 1999-2002, Alternative Bandwidths



(a) DC Plan Enrollment



(b) Probability of Leaving in One Year

Notes: Over 45 represents employees age 45 or older on September 1, 2002. Under 45 represents employees younger than age 45 on September 1, 2002. Employees over 45 were defaulted to remain in the DB plan for 2002 and later, while employees under 45 were defaulted to switch to the DC plan.

Table C.8: Effect of DC Plan on One-Year Leave Probability and Test for Selection, Alternative Bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
β_{Endo}	-0.009	-0.009	-0.008	-0.014	-0.014	-0.014
	(0.023)	(0.022)	(0.023)	(0.017)	(0.017)	(0.016)
β_{Exog}	-0.057**	-0.058***	-0.052**	-0.046***	-0.046***	-0.042**
	(0.022)	(0.022)	(0.022)	(0.017)	(0.016)	(0.017)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.048**	**0.043**	**0.059**	**0.056**	**0.049**	**0.072**
$\mathbb{E}\left[L_i\right]$	0.052	0.052	0.052	0.054	0.054	0.054
Controls	No	Yes	Yes	No	Yes	Yes
Age FEs	No	No	Yes	No	No	Yes
Year FEs	No	No	Yes	No	No	Yes
Bandwidth	5	5	5	10	10	10
N	1,499	1,499	1,499	2,584	2,584	2,584
First Stage F-stat	155	155	156	320	321	323

Note: Sample includes employees in the years 1999 - 2002. DC is instrumented for using the difference in default pension plan type base on age of in the current year — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Two-Stage Residual Inclusion (2SRI) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Appendix D: Local Average Response Function (LARF) Results

D.1 Overview of Method

We consider an alternative approach to addressing endogenous regressors in the context of a nonlinear, probit specification. In particular, we apply the Local Average Response Function (LARF) approach to our context (Abadie, 2003). We recast our econometric model within a potential outcomes framework. Let $Y(D(Z), Z)$ be a binary outcome of interest, which is a function of a binary treatment variable, $D(Z)$ and a binary instrument, Z. Define Y_1 and Y_0 as the potential outcomes, as a function of the treatment variable D. For a given individual, the observed outcome is $Y = DY_1 + (1 - D) Y_0$. Likewise, define the potential treatments as D_1 and D_0, which are functions of the instrument Z. For a given individual, the observed treatment status is $D = ZD_1 + (1 - Z) D_0$. Let X be a vector predetermined covariates. Using the shorthand $Y_{dz} = Y(d, z)$ for the potential outcomes, we make the following assumptions regarding the instrument, Z:

Assumption D.1 (LARF)**.**

D.1.1. ***Independence:*** *Conditional on X, the random vector $(Y_{00}, Y_{01}, Y_{10}, Y_{11}, D_0, D_1)$ is independent of Z*

D.1.2. ***Exclusion:*** $\Pr(Y_{d1} = Y_{d0} | X) = 1$ *for* $d \in \{0, 1\}$

D.1.3. ***First Stage:*** $0 < \Pr(Z = 1| X) < 1$ *and* $\Pr(D_1 = 1| X) > \Pr(D_0 = 1| X)$

D.1.4. ***Montonicity:*** $\Pr(D_1 \geq D_0 | X) = 1$

Abadie (2003) shows that the instrument Z can be used to estimate a Local Average Response Function (LARF). We briefly sketch the results from Abadie (2003) and apply them to our specific context. Let the average response function be $\mathbb{E}[Y(D)| X]$ — i.e. the average relationship between the expected outcome and treatment variable. We define the LARF as $\mathbb{E}\left[Y(D)|X, D_1 > D_0\right]$ — i.e. the average response among the complier subpopulation, or the group for whom $D_1 > D_0$. Consider the following weight κ:

$$\kappa = 1 - \frac{D(1-Z)}{\Pr(Z=0|X)} - \frac{(1-D)Z}{\Pr(Z=1|X)} \tag{D.18}$$

Let $g(Y, D, X)$ be a general function with bounded expectation. Abadie (2003) proves that under the assumptions above, we have the following:

$$\mathbb{E}\left[g(Y, D, X)|D_1 > D_0\right] = \mathbb{E}\left[\frac{\kappa}{\mathbb{E}[\kappa]} \cdot g(Y, D, X)\right] \tag{D.19}$$

In words, we can estimate any statistical moment among the subpopulation of compliers by using a weighted expectation over the entire population. Intuitively, the LARF generalizes the classic Local Average Treatment Effect (LATE) (Imbens and Angrist, 1994) to a broad class of nonlinear models. Indeed, if we were to model our outcome using a linear probability model the LARF and 2SLS LATE estimates are identical.

In our context, we assume the local average response function takes on a probit form:

$$\mathbb{E}\left[Y(D)|X, D_1 > D_0\right] = \Phi\left(\lambda_{LARF} D + \Gamma_{LARF} X\right) \tag{D.20}$$

The parameters of interest maximize a probit likelihood function among compliers:

$$\begin{aligned}(\lambda_{LARF}, \Gamma_{LARF}) \quad = \quad & \underset{\lambda,\Gamma}{\operatorname{argmax}}\, \mathbb{E}\Big[Y \ln\Phi(\lambda D + \Gamma X) \\ & \qquad - (1-Y)\ln(1-\Phi(\lambda D + \Gamma X))\Big| D_1 > D_0\Big]\end{aligned} \tag{D.21}$$

We cannot estimate the sample analog of (D.21) because we do not simultaneously observe D_1 and D_0. However, using the result above in (D.19), we can yet recover the parameters as follows:

$$\begin{aligned}\left(\lambda_{LARF},\Gamma_{LARF}\right) &= \underset{\lambda,\Gamma}{\operatorname{argmax}}\,\mathbb{E}\left[\frac{\kappa}{\mathbb{E}\left[\kappa\right]}\Bigg(Y\ln\Phi\left(\lambda D+\Gamma X\right)\right.\\ &\qquad\left.+\left(1-Y\right)\ln\left(1-\Phi\left(\lambda D+\Gamma X\right)\right)\Bigg)\right] \qquad \text{(D.22)}\end{aligned}$$

The resulting parameters can thus be used to calculate an average partial effect among the compliers:

$$\begin{aligned}\beta_{LARF} &= \mathbb{E}\big[\Phi\left(\lambda_{LARF}+\Gamma_{LARF}X\right)-\Phi\left(\Gamma_{LARF}X\right)\big|D_1>D_0\big]\\ &= \mathbb{E}\left[\frac{\kappa}{\mathbb{E}\left[\kappa\right]}\Big(\Phi\left(\lambda_{LARF}+\Gamma_{LARF}X\right)-\Phi\left(\Gamma_{LARF}X\right)\Big)\right] \qquad \text{(D.23)}\end{aligned}$$

In practice the weight κ, and in particular $Pr\left(Z=1|\,X\right)$ must be estimated in a first stage. We specify a linear model as follows:

$$Z=\pi X+\mu. \qquad \text{(D.24)}$$

We then estimate the LARF parameters using the sample analog of (D.22). Inference is performed accounting for the fact that the weight κ is estimated in a first stage (Newey and McFadden, 1994). The average partial effect in (D.23) now holds a causal interpretation — the LARF function returns the *ceteris paribus* effect of variation in D on Y among a consistent group, the compliers, and therefore does not suffer from the selection bias that confounds the naïve, endogenous regression in (10) in the text.

In our context, the outcome of interest is L_i, the endogenous regressor is DC_i, the set of controls are $(Post2002_i, Under45_i, X_i)$ and the instrument is $Post2002_i \times Under45_i$. We approximate the endogenous average partial effect as before using the naïve probit in (10) and the exogenous average partial effect with β_{LARF}. We then test the key inequality in equation 7. Inference is adjusted to account for the sample correlation between these two parameters.

D.2 LARF Results

Table D.9: Effect of DC Plan on One-Year Leave Probability and Test for Selection, LARF Estimates

	(1)	(2)	(3)
β_{Endo}	-0.022	-0.020	-0.020
	(0.015)	(0.015)	(0.015)
β_{Exog}	-0.063***	-0.060***	-0.056***
	(0.013)	(0.014)	(0.015)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.001**	**0.002**	**0.005**
$\mathbb{E}[L_i]$	0.077	0.077	0.077
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	4,153	4,134	4,134
First Stage F-stat	622	622	626

Note: Sample includes employees in the years 1999 - 2002. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Local Average Response Function (LARF) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table D.10: Effect of DC Plan on Two-Year Leave Probability and Test for Selection, LARF Estimates

	(1)	(2)	(3)
β_{Endo}	-0.024	-0.020	-0.021
	(0.023)	(0.023)	(0.023)
β_{Exog}	-0.121***	-0.120***	-0.116***
	(0.018)	(0.018)	(0.019)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**
$\mathbb{E}[L_i]$	0.146	0.146	0.146
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	3,137	3,123	3,123
First Stage F-stat	622	623	623

Note: Sample includes employees in the years 1999 - 2001 and 2002. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Local Average Response Function (LARF) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.

Table D.11: Effect of DC Plan on Three-Year Leave Probability and Test for Selection, LARF Estimates

	(1)	(2)	(3)
β_{Endo}	-0.033	-0.029	-0.029
	(0.025)	(0.025)	(0.025)
β_{Exog}	-0.168***	-0.170***	-0.164***
	(0.027)	(0.026)	(0.027)
$\boldsymbol{H_0}$: $\boldsymbol{\beta_{Endo} \leq \beta_{Exog}}$	**0.000**	**0.000**	**0.000**
$\mathbb{E}[L_i]$	0.187	0.187	0.187
Controls	No	Yes	Yes
Age FEs	No	No	Yes
Year FEs	No	No	Yes
N	2,040	2,039	2,039
First Stage F-stat	621	624	616

Note: Sample includes employees in the years 1999 and 2002. DC is instrumented for using the difference in default pension plan type base on age of in 2001 — i.e. instrument is DC plan default. "Endogenous" estimates are from a simple probit regression. "Exogenous" estimates are calculated using Local Average Response Function (LARF) with a linear first-stage and probit second stage. Average partial effects are reported. P-value for H_0 reported for evaluating implication of equation 7. Demographic controls include gender, race, a cubic in tenure dummies, hours worked per year and base pay rate. Robust standard errors are adjusted for first-stage estimation. * Significantly different at the 10% level; ** at the 5% level; *** at the 1% level.