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## EVALUATING PENSION PORTABILITY REFORMS. THE TAX REFORM ACT OF 1986 AS A NATURAL EXPERIMENT\*

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### Abstract

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This paper uses the Tax Reform Act of 1986 (TRA86) as a natural experiment to evaluate the job mobility response of prime aged US employees participating into employer sponsored defined benefit (DB) pension plans to a reduction in the vesting period for pension rights accrual. The repeated panel data design of the Survey of Income and Program Participation (SIPP) allows us to implement a "difference-in-difference" identification strategy using data from pre and post-reform periods. The effect of the policy change is identified as the difference between the change in predicted voluntary job mobility of the treated group and the change in predicted voluntary job mobility of the control group, over the period under study. We find that the reform had no significant effects on voluntary job mobility of the treated group. Our findings are robust to the use of different control groups and different pre/post reform samples.

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**Keywords:** Labour mobility, Employer Provided Pension Plans, Vesting, Difference-in-Difference

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# 1 Introduction

Federal policy toward portability of pension rights has been seen as an important issue since the 1960s in the United States. Approximately 40 percent of pension plans had no vesting provisions before the Employees' Retirement Income Security Act (ERISA) of 1974 established minimum vesting standards. The Tax Reform Act of 1986 further reduced the maximum vesting period from 10 to 5 years of plan participation.

Following the policy debate, driven by efficiency and equity issues, much of the empirical investigation has focused on the likely effects of enhanced pension portability on retirement income and on employee productivity. Simulation studies<sup>1</sup> have shown that, despite shorter vesting periods, benefit losses when changing jobs can still be significant for workers participating in defined benefit (DB) plans. Alternatively, while a number of empirical studies have found that participation in employer sponsored pension plans is associated with less frequent quits and layoffs<sup>2</sup>, there is disagreement over whether reduced job mobility arising from nonportable pensions enhances or reduces the efficiency of labor markets<sup>3</sup>.

Although policy reforms reducing the length of the vesting period have been enacted in most industrialized countries with wide employer provided pension coverage<sup>4</sup> no

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<sup>1</sup>See Clark and McDermed (1988), Employee Benefits Research Institute (1987).

<sup>2</sup>Mitchell (1983), Gustman and Steinmeier (1993), Allen, Clark and McDermed (1988, 1993), Andrietti and Hildebrand (2001).

<sup>3</sup>See Dorsey (1995) for a review of the literature.

<sup>4</sup>See Andrietti (2002) for a review of the reforms implemented in the United States, Canada, Ireland, the United Kingdom and the Netherlands.

empirical study has been produced to evaluate the impact of these policies.

In this paper we examine the impact of a reduction in the vesting period, introduced by the Tax Reform Act of 1986 (TRA86), on voluntary job mobility of US private sector prime aged employees. Given the typical structure of US employer provided pension plans, the reform affected workers participating in a DB plan with 5 to 10 accrued years of service, but is predicted to have no effect on another group - workers with 5 to 10 years of tenure but not participating in a DB plan. The latter group includes workers participating in an employer provided defined contribution (DC) plan as well as workers not participating to any employer provided pension arrangement. In order to identify the effect of the reform we adopt a "difference-in-difference" strategy, comparing the pre-post reform change in voluntary job mobility for workers treated by the reform with that of workers with similar characteristics but not affected by the reform and therefore taken as the control group.

We find that the reform had a not statistically significant effect on voluntary job mobility of the treated group. Our findings are robust to the use of different control groups and pre-post reform samples.

The paper is organized as follows. Section 2 describes employer provided pensions in the US and the 1986 Tax Reform Act. Section 3 illustrates the identification strategy. Section 4 describes the data. Section 5 presents the results. We conclude in section 6.

## 2 Employer Provided Plans, Vesting Provisions and the 1986 Tax Reform Act

Employer provided pension plans typically fall within one of two broad categories: defined benefit (DB) and defined contribution (DC) plans. In a traditional DB plan, each employee's future benefit is determined by a specific formula, and the plan provides a nominal level of benefits upon retirement. The typical "*final pay*" formula relates pension benefits to the length of service and to the final salary received, with the pension promise being usually funded through employers' contributions. DC plans provide for periodic contributions into an individual pension account for each worker. The contributions may be made by the firm and/or the worker. The level of benefit at retirement is determined by the total amount of contributions made and the rate of return of each individual's retirement assets. Although different types of DC plans<sup>5</sup> are offered in the US, most of them have the so called *401(k) option* which allows participant employees to make pre-tax contributions<sup>6</sup>. In principle, employers could establish 401(k) plans that rely entirely on voluntary employee contributions. However, they usually offer matching contributions up to a limit.

Individuals enrolled in pension plans, either of the DB or DC type, are subject to a vesting period before being fully entitled to their pension rights. Once vested, a worker

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<sup>5</sup>Money purchase plans, saving and thrift plans, profit sharing plans, stock bonus plans and employee stock ownership plans.

<sup>6</sup>401(k) plans are also referred to as *salary reduction plans*, as participating workers' take-home pay is reduced to make contributions to the plan.

can quit his/her job and retain the legal right to the future pension benefit he/she has been contributing for.

Prior to the Employee Retirement Income Security Act (ERISA) of 1974, there were no required vesting standards. ERISA established three primary vesting rules, contingent on the minimum plan participation standards which initially allowed plans to exclude workers under age 25, those working fewer than 1,000 hours annually and those within 5 years of normal retirement age (not to exceed age 65)<sup>7</sup>. Under "cliff vesting" participants were granted full (100-percent) rights to all accumulated benefits only after completing 10 years of plan participation. Under "graded vesting" the employee had to be at least 25-percent vested in the plan's accrued benefits after 5 years of plan participation, with increases in this percentage phased in over the next 5 years of service and reaching 100-percent vesting after 15 years. Finally, under the third vesting standard, called as "the rule of 45", an employee with 5 or more years of plan participation had to be at least 50-percent vested when the sum of the employee's age and the employee's years of plan participation reached 45, with increases of the nonforfeitable pension rights' percentage under a fixed schedule. ERISA also stipulated shorter vesting schedules for the so called "class year plans", defined as profit sharing, stock bonus, or money purchase plans providing for an employee's rights to contributions for each plan year separately. In this case, the plan had to ensure a 5-years vesting

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<sup>7</sup>Employee Benefit Research Institute (1986).

schedule for such employer contributions, with 100-percent vesting not later than the end of the fifth plan year after the contributions were made. Of course, plans could allow participants to vest more quickly than the minimum standards set by law.

The Tax Reform Act of 1986 (TRA86) introduced shorter vesting schedules and reduced the vesting options available to employers. Private single employer plans were allowed to provide either cliff vesting after 5 years of service or graded vesting of 20 percent after 3 years of service and 20 percent for each subsequent year of service, with full vesting reached after 7 years of service. The "class" vesting schedule was eliminated. The changes became effective for plan years beginning January 1st, 1989.

Graham (1988) uses data from the 1986 Employee Benefit Survey, administered to medium and large private sector firms, to show the influence of ERISA on vesting schedules. According to the survey, the vast majority of individuals participating in DB plans were subject to a 10 years "cliff" vesting schedule, while only 13 percent were subject to graded vesting schedules and overall only 10 percent of participants were offered vesting schedules more liberal than those prescribed by ERISA.

Alternatively, more than one quarter of DC plan participants were given immediate full vesting, while a minority of DC plan participants were offered cliff vesting within 5 years. Finally, most graduated and class vesting schedules were providing full vesting after 5 years of service.

The 1986 Employee Benefits Survey indicates that the vesting schedules of nearly

all DB plans needed to be modified to comply with the standards introduced by the TRA86, while most DC plans were already providing much more liberal vesting schedules than those prescribed by ERISA and therefore were already complying with the new legislation. Currently, most DC plans allow for the immediate vesting of employee contributions, while virtually all DB plans impose five years vesting<sup>8</sup>.

### **3 Identification Strategy**

The evidence reported in the previous section suggests that the vesting reform introduced by the TRA86 worked as a "natural experiment", affecting almost exclusively workers enrolled in DB plans with 5 to 10 years of service. A natural experiment occurs when some exogenous event - like a change in government policy - changes the environment in which agents operate. A natural experiment always has a control group, which is not affected by the policy change, and a treatment group, which is thought to be affected by the policy change. Unlike "true experiments", where treatment and control groups are randomly and explicitly chosen, control and treatment groups in natural experiments arise from the particular policy change. The policy change provides a transparent exogenous source of variation in the explanatory variables determining assignment to treatment.

In order to control for systematic differences between the control and treatment groups at least two years of data are needed: one before and one after the policy

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<sup>8</sup>See Woods (1993).

change. The sample is then broken down into four groups: the control group before the change, the control group after the change, the treatment group before the change, and the treatment group after the change.

The reason why a control group is required is that there might be other changes in the economy affecting the treatment group's outcome other than the policy intervention. If a control group is available which is affected by these other changes in the economy in the same way as the treated group, then it is possible to identify the impact of the policy. This is done through a "difference-in-difference" approach, which estimates the excess outcome growth of the treated group relative to a well suited control group.

In our setting, the difference-in-difference estimator is implemented through a simple comparison of the difference in voluntary job mobility rates of workers enrolled in employer provided DB pension plans with 5 to 10 years of service, which were affected by the policy reform (the treatment group), with the same difference for workers with similar service but not participating in a DB plan (the control group). Our dependent variable - voluntary job mobility- is dichotomous. This requires a distributional assumption on the unobservables to restrict the probability of observed outcomes in the zero-one range. However, to illustrate the difference-in-difference approach we consider for simplicity a linear probability model.

We define a dummy variable  $DB_i$  which equals one if individual  $i$  is in the treatment group, and zero otherwise. We also define a dummy variable  $Post_i$  if individual  $i$



is observed after the policy reform, as well as an interaction term among these two dummies. The equation of interest is

$$M_i = \gamma_0 + \gamma_1 DB_i + \gamma_2 Post_i + \gamma_3 Post_i \cdot DB_i + other\_factors, \quad (1)$$

where  $M_i$  is the outcome variable of interest - a dummy equal to one if a worker experienced a voluntary job to job transition - and  $\gamma_3$  - the interaction term coefficient - measures the effect of the policy. Without other factors in the regression,  $\hat{\gamma}_3$  will be the difference-in-difference estimator<sup>9</sup>:

$$\hat{\gamma}_3 = (\bar{\gamma}_{DB,post} - \bar{\gamma}_{DB,pre}) - (\bar{\gamma}_{NoDB,post} - \bar{\gamma}_{NoDB,pre}), \quad (2)$$

where the bar denotes group average, the first subscript denotes the treatment/control group, and the second subscript denotes the pre/post reform period.

However, the observed differences in job mobility rates may reflect underlying differences between the treatment and control groups rather than a treatment effect. Controlling for individual and job specific characteristics in a difference-in-difference framework is important if the composition of the treatment or control group changes over time and some of the characteristics are correlated with the dependent variable. When explanatory variables are added to equation (1) the OLS estimate of  $\gamma_3$  no longer has the simple form of (2), but its interpretation is similar.

Given the dichotomous nature of our dependent variable  $M_i$ , we assume that the

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<sup>9</sup>We could obtain the difference-in-difference estimate regressing the outcome dummy on the reform dummy separately for the treatment/control groups and subtracting the control estimated coefficients from the treated one.

equation errors are normally distributed and estimate probit regressions of the following form:

$$P(M_i) = \Phi(\gamma_0 + \gamma_1 DB_i + \gamma_2 Post_i + \gamma_3 Post_i \cdot DB_i + \beta X_i), \quad (3)$$

where  $\Phi(\cdot)$  is the cumulative normal distribution. The variables included in the vector  $X_i$  control for observable differences in characteristics of the treatment and control groups that affect the voluntary job mobility rate. Common unobservable differences among groups are controlled for by the variable  $DB_i$ , while  $Post_i$  controls for common macro effects.

A test of the impact of TRA86 is a test that workers participating in an employer sponsored DB pension plan with job tenure between 5 and 10 years increased their job mobility after the passage of the reform relative to workers with a similar job tenure in the control group. It is a test that  $\gamma_3$  is greater than zero. Thus our hypothesis tests are one-tailed.

There are a number of potential problems to be addressed while using the difference-in-difference approach. Finding a suitable control group can be difficult. It is required that the control group be unaffected by the policy itself while being affected by other (macro) economic factors in the same way as the treatment group. Moreover, treatment and control group have to be comparable. This latter aspect is important for three main reasons<sup>10</sup>. First, the D-D estimator relies on the similarity of the control

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<sup>10</sup>See Meyer (1995).

and treatment groups in order to separate the effect of interest from other exogenous influences. The key assumption, which is likely to hold only if the groups are comparable, is that the effect of these exogenous influences is the same on the control and treatment groups. Second, the more different the control and experimental groups are, the higher is the bias from hidden interactions. For example, an estimate of the effect of the reform that includes in the control group only (or also) individuals not participating in any employer provided pension arrangement, may be biased if individuals in employer provided pension plans are less likely to leave their jobs because they hold good jobs. Third, since the true model equation is unknown, there is a risk of misspecifying the form of the estimating equation. For example, if the true equation is in levels, a transformation to a nonlinear (probit) form will result in misspecification bias. This bias may be exacerbated if the control group and the treatment group are dissimilar in the mean or distribution of the dependent variable.

To estimate the effect of the vesting reform it is then essential to find two groups that are comparable, one of which is affected by the reform. The difference-in-difference approach requires the assignment into the treatment and the control groups to be random, and therefore not related to the outcome under study. With nonrandom assignment, differences in labour market outcomes may reflect the noncomparability of the two groups rather than the effect of the vesting reform. To prevent this possibility, we estimate regressions in which the relevant demographic, job related and local labor market

characteristics are controlled for. With this adjustment we need a weaker assumption: conditional on observable characteristics, allocation into the treatment and the control group is random. In our case, comparing the voluntary job mobility rate of DB participants with that of no-DB participants may bias the results if a nonrandom group of individuals participate into employer provided DB pension arrangements. In particular, DB plan participants are likely to differ from non-participants both in observable and unobservable characteristics. There is widespread evidence that participants in DB plans have higher wages and higher education. Also, DB participants are likely to have lower discount rates, thus preferring tax deferred compensation to cash wages while at the same time being intrinsically less mobile. Alternatively, employers can use pension plans to screen out movers<sup>11</sup>. However, alternative control groups can be workers not participating to a DB plan as well as workers participating only to a DC plan. The latter group in particular should be pretty similar to the group of DB participants, in both observable and unobservable characteristics.

In order to assess the robustness of our estimates, we use multiple control groups in the empirical analysis. The advantage of having multiple control groups is that if we find similar results, we can be more confident that we are estimating the actual effect of the vesting reform and not just the effect of other contemporaneous changes or trend differences between the control and treatment groups. Ultimately, then, the credibility

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<sup>11</sup>See Ippolito (1997).

of our results lies in the consistency of our estimates across different treatment and control groups rather than on any one estimate.

## 4 Data

Our source of data is the Survey of Income and Program Participation (SIPP). The SIPP is a set of independent overlapping short panels<sup>12</sup>. The adults followed in each SIPP panel come from a nationally representative sample of individuals 15 years of age and older to be selected in households in the civilian non-institutionalized US population. Those individuals, along with others who subsequently live with them, are interviewed once every four months over the life of the panel. Each panel is randomly divided into four rotation groups. Each rotation group is interviewed in a separate month and the four rotation groups constitute a wave. In each year of the panel data are collected for a varying number of waves, covering up to 48 months.

Information collected in SIPP falls into two categories: *core* and *topical*. The core questionnaire is administered at every interview and covers demographic and economic characteristics of each household member. Most core questions provide information covering the 4 months since the previous interview, although a few core items are measured only as of the interview date, once every 4 months. Topical questions produce in-depth information on specific subjects and are not asked at every interview. Infor-

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<sup>12</sup>SIPP has been collected and released for the years 1984, 1985, 1986, 1987, 1988, 1989, 1990, 1991, 1992, 1996 and 2001.

mation on employer sponsored pension plans is typically asked in a topical module<sup>13</sup>, and allows us to assign each worker a (mutually exclusive) pension participation status: participating to a DB plan, participating to a DC plan, not participating into any employer provided pension arrangement. However, further detailed characteristics of the plans is not available in SIPP data.

Relying on data from the Employee Benefit Surveys<sup>14</sup>, we assume that the typical vesting schedule applied by DB plans before the TRA86 reform was a "cliff" 10 years vesting, while the typical vesting schedule applied in DC plans was to give full vesting in less than 5 years. Under this assumption, the reform affected only workers participating in a DB plan with 5 to 10 years of accrued years of service; these workers represent the "treatment group".

For estimation purposes we use data extracts from the 1984, 1986, 1990, 1992 and 1996 SIPP panel years. The choice of these particular years is driven by the fact that we need data on participation to employer provided pension arrangements (either DB or DC) and voluntary job mobility coming from the pre-reform and post-reform period. We analyze job mobility behavior during the year following the collection of pension information<sup>15</sup>. Our dependent variable equals one if a worker changes job

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<sup>13</sup>The pension topical module is asked in wave 4 during SIPP 1984 and SIPP 1986 and in wave 7 during SIPP 1996.

<sup>14</sup>See Graham (1988).

<sup>15</sup>Our observation periods thus span from wave four to wave seven for SIPP 1984, 1986, 1990 and 1992 and from wave seven to wave ten for SIPP 1996.

without an intervening spell of unemployment during the twelve months following the first interview. Although the information necessary to differentiate quits from layoffs is available in the SIPP data, it does not appear to be very reliable. As pointed out by Gustman and Steinmeier (1993), several variables, such as the randomly assigned job number or direct questions to employees, could be used to identify mobility in the SIPP data. However, the mobility information derived from these variables is often contradictory. Therefore, we first adopt a broad definition of mobility, that defines a transition to a new job to have occurred as long as one of those variables indicates a job change. We then define voluntary job mobility as a transition to a new employer occurring over the observed period without an intervening spell of unemployment.

We restrict the sample to individuals in full time employment working in private sector - non agricultural, non construction - firms in the last month of the reference period. We exclude agricultural and construction workers due to the idiosyncratic nature of job turnover in these sectors. In particular construction workers are unique in both the highly seasonal nature of their work and the tendency for their pension plans to be provided through unions in the form of multiemployer plans. The latter plans eliminate most of the portability issues arising in single employer plans by considering service with multiple employers under the same plan as if the individual had worked with one employer the entire time. Thus construction workers usually combine high turnover with little discontinuity of pension coverage. In the same fashion, public sector workers

are excluded both because they have different patterns of turnover and because public pension plans usually have more generous portability provisions. In order to focus our analysis on a group that has a high attachment to the labor force, avoiding the sample selection issues related to labor market young age entry and old age exit, we restrict our analysis to 25 to 50 years-old individuals. Moreover, the empirical analysis is performed separately for males and females because of their typically different turnover patterns.

We assess the robustness of our results estimating the model using different pooled SIPP samples as well as different control groups. First, we use SIPP 1984 and 1986 as pre-reform data and SIPP 1990, 1992 and 1996 as post-reform data (Sample 1). Second, in order to account for the progressive enactment of the reform, we consider only SIPP 1996 as post reform period (Sample 2). Finally, we use only SIPP 1984 as pre-reform period (Sample 3) to account for the fact that individuals observed in SIPP 1986, although not yet directly affected by the TRA86 reform, could have changed their behavior while expecting its enactment (1st January 1989). The control groups used in the estimation are represented by individuals with 5 to 10 years of service not participating to a DB plan (control group 1), individuals with 5 to 10 years of service not participating to any employer provided pension arrangement (control group 2), and finally by individuals participating to a DC employer sponsored pension plan (control group 3).

Tables 1 and 2 presents the mean values of some relevant characteristics of the



treatment and control groups respectively for males and females. There are relevant differences in job mobility rates, particularly between control group 2 and the treatment group. These two groups seem to be different along other dimensions as well. Individuals participating in a DB plan earn higher wages, are better educated, are more likely to have employer provided health insurance, to be union members and to be in large firms. The differences are somewhat reduced but still relevant while extending the control group to individuals participating to DC plans (control group 1). As a consequence, we can be concerned that estimates obtained including the first two control groups are inconsistent. Alternatively, when we restrict the control group to workers participating in DC plans (control group 3) we notice its similarity with the treatment group over the relevant observed characteristics.

Tables 3 and 4 present voluntary job mobility rates for the treatment group and control groups by panel year. The figures emerging from these tables are displayed in Figures 1 and 2. They provide a clear illustration of the similarity of the treatment group and control group 3.

Tables 5 to 10 present voluntary job mobility rates for the treatment group and control groups in the years before and after the reform, by gender and samples as well as "difference" and "difference-in-difference" estimates using a linear probability model. In each table, column 1 and 2 correspond respectively to the average job mobility rate pre and post TRA86; column 3 contains the difference estimates of the change in job

mobility by control group. The difference-in-difference estimates of the impact of the reform on job mobility is provided in the fourth column. These preliminary figures suggest that the voluntary job mobility rates of the treated increased after the TRA86. Alternatively, the job mobility rates of the control groups experienced small increments or decrements. The difference-in-difference estimates are usually found to be positive, although never statistically significant at standard levels.

## 5 Results

Because the treatment group and the control groups differ in demographic and job related characteristics, the observed differences in job mobility outcomes may reflect underlying differences between the treatment and control groups rather than a treatment effect. Controlling for demographic and job related characteristics in a difference-in-difference approach is important if the composition of the treatment and the control groups changes over time and some of these characteristics are correlated with the dependent variable. In addition, controlling for demographic and job related characteristics reduces the residual variance of the regression and produces more efficient estimates.

We estimate the model under two specifications. Under the first specification, the vector  $X_i$  in equation (3) contains personal and family related variables proxing for

mobility costs<sup>16</sup>, job related variables<sup>17</sup>, local labour market variables<sup>18</sup> controlling for cycle effects as well as year dummies. Industry dummies - with manufacturing industry as the reference group - and occupation dummies - with blue collar occupation as the reference group - are included to proxy for industry and occupation specific turnover rates faced by individuals. Moreover, we include an interaction term between the post-reform period and the age variable to account for the fact that individuals with different ages can be affected in different way by macroeconomic trends.

Under the second specification, among the costs of mobility we include a variable proxying for the opportunity cost of leaving a pension job (the pension loss), as well as an interaction term between the post-reform period and the pension loss applicable after the reform.

Table 11 to 16 present the results of estimating equation (3) separately for males and female, using different control groups and different samples<sup>19</sup>. The results reported in columns 1 and 2 have been obtained using the wider control group. Column 2 specification also controls for pension loss variables. Similarly, the results reported in

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<sup>16</sup>Included are dummy variables for age, geographical area (SMSA as well as regions), marital status, children, house ownership, race, employed spouse, and continuous variables for experience, experience squared, education and family size.

<sup>17</sup>Included are dummy variables for pension plan participation, firm size, employer provided training and health insurance, union status, industry and occupation, and continuous variables for log-hourly wage and job tenure.

<sup>18</sup>State unemployment rates.

<sup>19</sup>The estimated coefficients for family size as well as the dummy variables controlling for marital status, children under 18, housing tenure, industry, occupation and region of residence, are not reported in the tables. The estimated coefficients have been transformed into marginal effects. *t* - values are reported in parenthesis, while the coefficients' statistical significance is indicated with one (90%) or two (95%) asterisks.

columns 3 and 4 have been obtained using the restricted, but less comparable control group, while those reported in columns 5 and 6 use the more comparable control group 3. In general, these results are quite comparable to those reported in Tables 5 to 10, obtained through a linear probability model without other covariates. Also the results are similar when using different control groups and sample sizes.

The estimated coefficients on *Post* are small in magnitude and not statistically significant for females, suggesting that there is no overall trend in average job mobility for the two groups. This is not the case for males, where the effect is always positive and statistically significant, becoming stronger while using control group 3. The coefficient on *DB* has a negative sign, as expected, but is statistically significant only for females when using samples 1 and 2. Finally, the difference-in-difference estimate is generally close to zero and always statistically not significant. The results are also generally robust to the inclusion of pension loss variables pre and post reform. The coefficients on the other control variables included in the regressions all have the expected signs.

These results seem to suggest that the vesting reform did not succeed in its main objective of fostering the voluntary job mobility rate of workers "tied" to jobs by employer provided plans with long vesting periods. Moreover, the fact that the treatment effect,  $\gamma_3$ , changes very little when we include our vector of control variables suggests that any changes in the composition of the treatment and control groups that occurred over time are uncorrelated with the treatment.

## 6 Conclusions

In this paper we exploit the natural experiment produced by the TRA86 vesting reform to evaluate its effects on voluntary job mobility behavior of the individuals affected by the reform - workers participating in a DB plan with 5 to 10 years of service. We use a difference-in-difference identification strategy, comparing the pre-post reform change in voluntary job mobility for workers treated by the reform with that of various groups of workers with similar characteristics but not affected by the reform and therefore taken as control groups. Our main finding is that the reform had a not statistically significant effect on the voluntary job mobility rate of the treated groups, both for males and for females. Our findings are robust to the use of different control groups and pre/post reform samples. These results suggest that, while the reform reduced the pension loss of workers participating in DB plans, it was ineffective in its main purpose of fostering the voluntary job mobility rate of workers "tied" to jobs by employer provided plans with long vesting periods.

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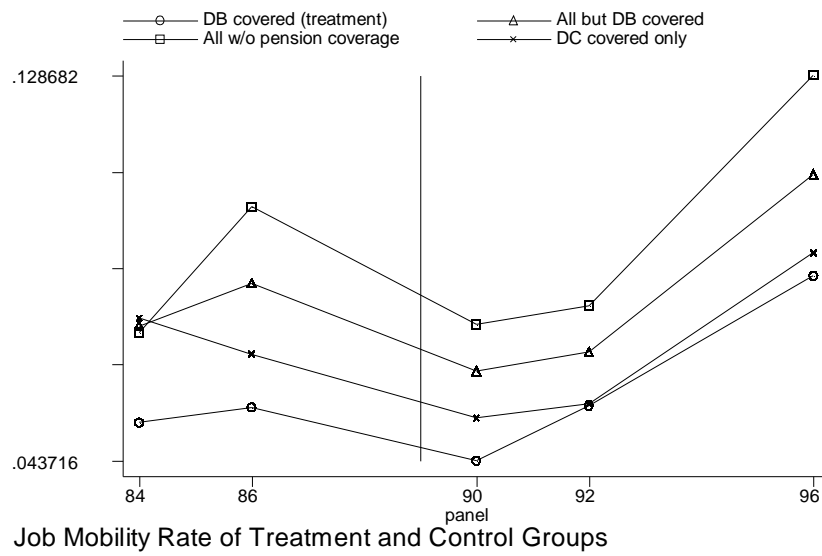


Figure 1:



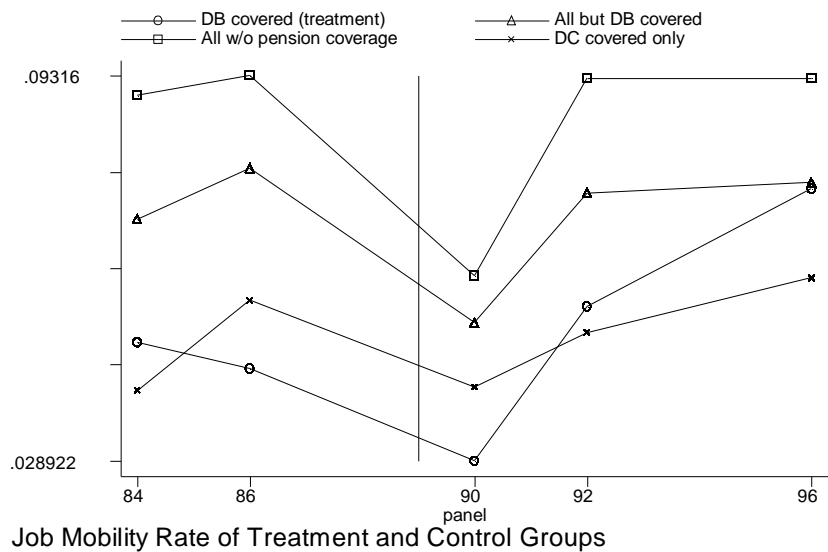


Figure 2:

Table 1: Summary Statistics (Means): Females

Variable	Treatment Group			Control Group 1			Control Group 2			Control Group 3		
	S.1	S2	S3	S1	S2	S3	S1	S2	S3	S1	S2	S3
Job Mobility (%)	5.62	6.5	7	7.63	7.7	9.06	8.85	9.96	10.2	6.42	7.4	8
Hourly Wage	17.5	18.62	18.5	15.08	16.15	15.89	13.16	14.10	13.7	17	18.05	17.94
Employed Spouse (%)	35	35	35	28.3	27	26.7	22.25	21.85	21.1	34	32	32
House Owner (%)	76.4	75	75	70	69	68.6	64.62	62.38	48.6	75	75	75
DC Plan (%)	43.8	45	51.5	50	52	51.8						
Pension Loss Pre Reform	112	111	110									
Pension Loss Post Reform	54	33	43									
Married (%)	77.2	77	77.8	71.8	77	73	68.6	69.57	69.4	75	76	76
Black (%)	10.2	10	10.2	12	10	14	13.68	14.38	16.5	10.2	11	11.7
Household Size	3.2	3.3	3.29	3.2	3.2	3.2	3.25	3.27	3.28	3.2	3.2	3.2
Children Under 18 (%)	60	55	61.7	57	57	57	54.18	54.9	54.6	59	59	59.3
Experience	15.4	15.6	15.9	15.7	16	16.6	16.14	16.39	17	15.4	15.7	16.2
Education	14.2	14	13.9	13.5	13.4	13.3	12.95	12.79	12.6	14.1	14	13.8
Employer Size: >100 (%)	5.7	73	69	42	52	49.4	29.37	34.44	32.7	54	67	65
Employer Training (%)	15.7	10	6.9	13.4	8.5	4.7	11.96	7.6	4.7	14.8	9	4.8
Emp. Health Insurance (%)	92	92	90.8	81	82	80	73.8	73.3	70.1	89	91	89
Union (%)	30	31	32.6	12	13	12.3	10	10.65	11.3	13.8	15	13.3
State Unempl. Rate (%)	6.27	5.7	5.67	6.2	5.6	5.5	6.35	5.78	5.7	6.05	5.5	5.3
Sample Size	1.815	998	771	2.985	1.508	1.159	1.447	723	559	1.448	785	600

Source: Our elaboration on SIPP data.

Table 2: Summary Statistics (Means): Females

Variable	Treatment Group			Control Group 1			Control Group 2			Control Group 3		
	S1	S2	S3	S1	S2	S3	S1	S2	S3	S1	S2	S3
Job Mobility (%)	4.9	5.74	6.44	6.9	7.72	7.68	8.36	9.62	9.36	5.1	5.5	5.85
Hourly Wage	13.45	14.28	14.38	11.41	12.37	12.45	10.24	11.14	11	12.85	13.79	14
Employed Spouse (%)	41.5	41.29	41.8	34.3	34.77	33.2	30.7	31.2	27.9	38.78	38.9	38.9
House Owner (%)	73.9	74.46	74.7	69.3	68	67.5	66.36	64.4	63.8	73.04	72.13	71.5
DC Plan (%)	38	42.28	49.6	44.65	46.25	47.9						
Pension Loss Pre Reform	110.7	111.8	110.7									
Pension Loss Post Reform	54.3	35.5	46.2									
Married (%)	66	66.44	67	62.9	62.68	61.9	62.6	61.66	59.3	63.24	63.86	64.7
Black (%)	15.2	15.44	17	16.09	16.7	18.7	17.37	18.58	22	14.5	14.55	15.1
Household Size	3.07	3.12	3.13	3.11	3.15	3.12	3.2	3.26	3.22	2.99	3.02	3.01
Children Under 18 (%)	54.1	54.45	53.9	56.1	57.15	56.1	57.9	59.55	56.8	54	54.36	55.3
Experience	16.67	16.88	17.26	17.25	17.6	17.9	17.74	18.15	18.6	16.64	17	17.24
Education	14.2	14.13	14.1	13.30	13.24	13.24	12.94	12.84	12.8	13.75	13.69	13.75
Employer Size: >100 (%)	55.17	69.6	67.4	41.83	51.9	51.4	32.3	39.92	39.3	53.68	65.85	64.53
Employer Training (%)	14.48	9.1	6.83	11.39	6.94	4.43	9.54	6.46	3.47	13.68	7.5	5.47
Emp. Health Insurance (%)	78.17	79.1	76	66.45	66.64	65.4	57.75	57.58	56	77.25	77.18	75.7
Union (%)	25.72	26.34	26.68	9.54	10.2	10.3	8.43	9.09	9.53	10.93	11.49	11.1
State Unempl. Rate (%)	6.18	5.63	5.5	6.16	5.54	5.4	6.25	5.64	5.52	6.05	5.43	5.25
Sample Size	1.878	1.010	776	2.766	1.412	1.107	1.531	759	776	1.235	653	530

Source: Our elaboration on SIPP data.

Table 3: Job Mobility Rate. Males

SIPP Panel:	1984	1986	1990	1992	1996
Treatment Group	5.93	4.85	3.67	5.18	7.83
Control Group 1	8.09	7.16	6.02	7.11	9.59
Control Group 2	8.04	9.15	6.83	8.66	11.64
Control Group 3	8.15	5.41	5.12	5.44	7.93
Sample Size	745	576	1.052	1.152	1.185

Source: Our elaboration on SIPP data.

Table 4: Job Mobility Rate. Females

SIPP Panel:	1984	1986	1990	1992	1996
Treatment Group	5.41	3.42	2.79	4.85	7.83
Control Group 1	7.10	7.87	4.59	7.61	7.95
Control Group 2	8.25	10.44	4.88	9.67	9.97
Control Group 3	5.48	4.07	4.18	5.08	7.93
Sample Size	648	539	1.091	1.131	1.235

Source: Our elaboration on SIPP data.

Table 5: Difference in Difference Estimate of Voluntary Job Mobility. Males. Sample 1

	<i>Pre TRA86</i>	<i>Post TRA86</i>	<i>D</i>	<i>D-D</i>
<i>Treatment Group (%)</i>	5.496 [564]	5.675 [1.251]	0.179 (0.15)	
<i>Control Group 1 (%)</i>	7.662 [757]	7.624 [2.138]	-0.038 (0.03)	0.217 (0.13)
<i>Control Group 2 (%)</i>	8.51 [388]	8.97 [1.059]	0.46 (0.28)	-0.281 (0.14)
<i>Control Group 3 (%)</i>	6.775 [369]	6.3 [1.079]	-0.473 (0.32)	0.652 (0.35)

Source: Our elaboration on SIPP data.

Table 6: Difference in Difference Estimate of Voluntary Job Mobility. Males. Sample 2

	<i>Pre TRA86</i>	<i>Post TRA86</i>	<i>D</i>	<i>D-D</i>
<i>Treatment Group (%)</i>	5.496 [564]	7.834 [434]	2.338 (1.48)	
<i>Control Group 1 (%)</i>	7.662 [757]	9.587 [751]	1.925 (1.33)	0.413 (0.19)
<i>Control Group 2 (%)</i>	8.51 [388]	11.64 [335]	3.13 (1.40)	-0.808 (0.30)
<i>Control Group 3 (%)</i>	6.775 [369]	7.933 [416]	1.158 (0.62)	1.18 (0.49)

Source: Our elaboration on SIPP data.

Table 7: Difference in Difference Estimate of Voluntary Job Mobility. Males. Sample 3

	<i>Pre TRA86</i>	<i>Post TRA86</i>	<i>D</i>	<i>D-D</i>
<i>Treatment Group (%)</i>	5.93 [337]	7.83 [434]	1.9 (1.03)	
<i>Control Group 1 (%)</i>	8.09 [408]	9.59 [751]	1.5 (0.85)	0.4 (0.15)
<i>Control Group 2 (%)</i>	8.04 [224]	11.64 [335]	3.6 (1.38)	-1.7 (0.55)
<i>Control Group 3 (%)</i>	8.15 [184]	7.93 [416]	-0.22 (0.09)	2.12 (0.70)

Source: Our elaboration on SIPP data.

Table 8: Difference in Difference Estimate of Voluntary Job Mobility. Females. Sample 1

	<i>Pre TRA86</i>	<i>Post TRA86</i>	<i>D</i>	<i>D-D</i>
<i>Treatment Group (%)</i>	4.528 [530]	5.044 [1.348]	0.516 (0.47)	
<i>Control Group 1 (%)</i>	7.458 [657]	6.733 [2.109]	-.725 (0.64)	1.241 (0.76)
<i>Control Group 2 (%)</i>	9.28 [388]	8.05 [1.143]	-.725 (0.64)	1.243 (0.76)
<i>Control Group 3 (%)</i>	4.832 [269]	5.176 [966]	.344 (0.23)	.172 (0.09)

Source: Our elaboration on SIPP data.

Table 9: Difference in Difference Estimate of Voluntary Job Mobility. Females. Sample 2

	<i>Pre TRA86</i>	<i>Post TRA86</i>	<i>D</i>	<i>D-D</i>
<i>Treatment Group (%)</i>	4.528 [530]	7.083 [480]	2.555 (1.74)*	
<i>Control Group 1 (%)</i>	7.458 [657]	7.947 [755]	0.489 (0.34)	2.066 (0.99)
<i>Control Group 2 (%)</i>	9.28 [388]	9.97 [371]	0.69 (0.32)	1.865 (0.74)
<i>Control Group 3 (%)</i>	4.832 [269]	5.989 [384]	1.157 (0.64)	1.398 (0.60)

Source: Our elaboration on SIPP data.

Table 10: Difference in Difference Estimate of Voluntary Job Mobility. Females. Sample 3

	<i>Pre TRA86</i>	<i>Post TRA86</i>	<i>D</i>	<i>D-D</i>
<i>Treatment Group (%)</i>	5.41 [296]	7.08 [480]	1.67 (0.92)	
<i>Control Group 1 (%)</i>	7.10 [206]	7.95 [755]	0.85 (0.49)	0.82 (0.33)
<i>Control Group 2 (%)</i>	8.25 [352]	9.97 [371]	1.72 (0.68)	-0.05 (0.01)
<i>Control Group 3 (%)</i>	5.48 [146]	5.99 [384]	0.51 (0.22)	1.16 (0.40)

Source: Our elaboration on SIPP data.

Table 11: Probit Job Mobility Equation. Sample 1. Males

	Control Group 1		Control Group 2		Control Group 3	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Post Reform*DB Plan	0.003 (0.19)	0.030 (0.59)	0.001 (0.03)	0.029 (0.55)	0.005 (0.30)	-0.001 (0.02)
DB Plan	-0.016 (1.15)	-0.056 (1.33)	-0.014 (0.86)	-0.098 (1.80)	-0.006 (0.42)	0.000 (0.01)
Post Reform	0.032 (1.75)*	0.031 (1.68)*	0.040 (1.78)*	0.038 (1.70)*	0.046 (2.17)**	0.046 (2.17)**
Age 25-40	0.029 (1.50)	0.028 (1.45)	0.019 (0.81)	0.019 (0.79)	0.056 (2.74)**	0.056 (2.73)**
Post Reform*Age 25-40	-0.020 (1.07)	-0.018 (0.95)	-0.029 (1.27)	-0.026 (1.11)	-0.041 (1.82)	-0.042 (1.80)
Black	-0.009 (0.82)	-0.009 (0.81)	-0.015 (1.20)	-0.015 (1.21)	0.002 (0.12)	0.001 (0.12)
Education	0.008 (3.93)**	0.007 (3.84)**	0.007 (3.09)**	0.007 (2.95)**	0.008 (3.44)**	0.008 (3.44)**
Experience	-0.001 (0.61)	-0.002 (0.67)	-0.003 (1.17)	-0.003 (1.31)	-0.002 (0.72)	-0.002 (0.72)
Experience Squared	0.000 (0.79)	0.000 (0.79)	0.000 (0.86)	0.000 (0.91)	0.000 (1.51)	0.000 (1.51)
Log Hourly Wage	-0.036 (4.59)**	-0.036 (4.59)**	-0.037 (3.95)**	-0.038 (3.96)**	-0.035 (3.87)**	-0.035 (3.88)**
Job Tenure	-0.009 (3.62)**	-0.011 (3.39)**	-0.011 (3.47)**	-0.015 (3.45)**	-0.006 (2.00)**	-0.005 (1.34)
Employer Size: >100	0.008 (0.93)	0.007 (0.91)	0.015 (1.54)	0.015 (1.49)	0.004 (0.44)	0.004 (0.44)
Employer Training	0.006 (0.55)	0.006 (0.57)	0.004 (0.32)	0.005 (0.37)	-0.001 (0.13)	-0.001 (0.13)
Employer Health Insurance	-0.026 (2.42)**	-0.026 (2.40)**	-0.032 (2.60)**	-0.032 (2.56)**	-0.030 (2.08)**	-0.030 (2.08)**
DC Plan	-0.013 (1.45)	-0.012 (1.39)				
Union Member	-0.020 (2.02)**	-0.020 (2.06)**	-0.024 (2.14)**	-0.025 (2.22)**	-0.023 (2.25)**	-0.023 (2.24)**
State Unemployment Rate	-0.003 (1.20)	-0.003 (1.21)	-0.002 (0.64)	-0.002 (0.67)	-0.001 (0.24)	-0.001 (0.24)
SMSA	0.016 (2.25)**	0.016 (2.25)**	0.009 (1.09)	0.009 (1.08)	0.017 (2.11)**	0.017 (2.11)**
Year 84	0.015 (1.02)	0.015 (1.05)	0.007 (0.42)	0.008 (0.46)	0.020 (1.16)	0.020 (1.15)
Year 90	-0.031 (2.90)**	-0.031 (2.90)**	-0.035 (2.80)**	-0.035 (2.79)**	-0.030 (2.55)**	-0.030 (2.55)**
Year 92	-0.019 (1.66)	-0.019 (1.67)	-0.017 (1.23)	-0.017 (1.23)	-0.023 (1.76)	-0.023 (1.76)
Pension Loss		0.041 (1.02)		0.071 (1.62)		-0.006 (0.14)
Post Reform*Post Reform Pension Loss		-0.033 (0.56)		-0.036 (0.59)		0.008 (0.14)
Log Likelihood	1.117	1.116.7	779	777.6	699.9	699.9
Observed P	6.86	6.86	7.05	7.05	5.98	5.98
Predicted P( $\bar{X}$ )	5.93	5.93	5.86	5.85	5.1	5.1
Sample Size	4.710 <sup>9</sup>	4.710	3.262	3.262	3.263	3.263

Table 12: Probit Job Mobility Equation. Sample 1. Females

	Control Group 1		Control Group 2		Control Group 3	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Post Reform*DB Pension	0.010 (0.64)	0.018 (0.39)	0.012 (0.68)	0.023 (0.47)	0.005 (0.30)	-0.007 (0.16)
DB Plan	-0.032 (2.41)**	-0.086 (2.16)**	-0.038 (2.37)**	-0.099 (1.84)*	-0.002 (0.16)	-0.064 (1.27)
Post Reform	-0.008 (0.46)	-0.008 (0.46)	-0.006 (0.29)	-0.006 (0.30)	-0.002 (0.08)	-0.001 (0.04)
Age 25-40	-0.039 (1.99)**	-0.037 (1.91)*	-0.050 (2.18)**	-0.049 (2.12)**	-0.039 (1.71)*	-0.034 (1.53)
Post Reform*Age25-40	0.033 (2.02)**	0.033 (1.99)**	0.034 (1.77)**	0.034 (1.76)*	0.040 (2.17)**	0.037 (2.00)**
Black	-0.013 (1.44)	-0.013 (1.44)	-0.011 (1.05)	-0.011 (1.06)	-0.010 (1.02)	-0.010 (1.00)
Education	0.003 (1.47)	0.003 (1.43)	0.002 (0.84)	0.002 (0.80)	0.003 (1.13)	0.003 (1.12)
Experience	0.003 (1.42)	0.003 (1.39)	0.006 (2.10)**	0.006 (2.06)**	0.002 (0.70)	0.002 (0.70)
Experience Squared	0.000 (2.09)*	0.000 (2.09)*	0.000 (2.84)**	0.000 (2.84)**	0.000 (0.97)	0.000 (0.95)
Log Hourly Wage	-0.011 (1.43)	-0.012 (1.51)	-0.019 (2.03)**	-0.020 (2.10)**	-0.008 (0.85)	-0.008 (0.89)
Job Tenure	-0.004 (1.58)	-0.007 (2.29)**	-0.002 (0.75)	-0.005 (1.39)	-0.002 (0.87)	-0.008 (1.95)*
Employer Size: >100	-0.005 (0.60)	-0.005 (0.60)	-0.006 (0.60)	-0.006 (0.61)	-0.009 (0.97)	-0.008 (0.95)
Employer Training	0.006 (0.58)	0.006 (0.56)	0.007 (0.53)	0.006 (0.51)	0.009 (0.82)	0.009 (0.79)
Employer Health Insurance	-0.012 (1.41)	-0.012 (1.43)	-0.008 (0.80)	-0.008 (0.81)	-0.029 (2.61)**	-0.029 (2.63)**
DC Plan	-0.024 (3.02)**	-0.024 (2.93)**				
Union Member	-0.026 (2.68)**	-0.026 (2.73)**	-0.025 (2.22)**	-0.025 (2.26)**	-0.025 (2.55)**	-0.025 (2.58)**
State Unemployment Rate	-0.001 (0.39)	-0.001 (0.42)	-0.001 (0.31)	-0.001 (0.34)	0.000 (0.05)	0.000 (0.08)
SMSA	0.016 (2.34)**	0.016 (2.33)**	0.010 (1.20)	0.010 (1.20)	0.017 (2.15)**	0.016 (2.12)**
Year 84	0.009 (0.67)	0.010 (0.69)	0.010 (0.64)	0.011 (0.66)	0.022 (1.25)	0.021 (1.22)
Year 90	-0.037 (3.68)**	-0.036 (3.64)**	-0.043 (3.72)**	-0.043 (3.69)**	-0.029 (2.67)**	-0.028 (2.61)**
Year 92	-0.015 (1.41)	-0.015 (1.39)	-0.018 (1.38)	-0.017 (1.36)	-0.021 (1.76)	-0.020 (1.75)
Pension Loss		0.056 (1.49)		0.049 (1.17)		0.050 (1.31)
Post Reform*Post Pension Loss		-0.009 (0.16)		-0.013 (0.23)		0.016 (0.32)
Log Likelihood	1.015.8	1.014.3	773.1	772.2	588.3	586.6
Observed P	6.09	6.09	6.45	6.45	4.98	4.98
Predicted P(X)	5.26	5.24	5.5	5.46	4.26	4.22
Sample Size	4.644	4.644	3.409	3.409	3.113	3.113

Table 13: Probit Job Mobility Equation. Sample 2. Males

	Control Group 1		Control Group 2		Control Group 3	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Post Reform*DB Plan	0.008 (0.39)	-0.009 (0.13)	-0.001 (0.04)	-0.017 (0.24)	0.012 (0.53)	-0.032 (0.55)
DB Plan	-0.017 (1.02)	-0.058 (1.03)	-0.016 (0.87)	-0.083 (1.14)	-0.008 (0.51)	-0.027 (0.43)
Post Reform	0.041 (1.62)	0.041 (1.63)	0.055 (1.71)*	0.056 (1.72)*	0.063 (2.06)**	0.066 (2.14)**
Age 25-40	0.021 (0.85)	0.022 (0.89)	0.016 (0.55)	0.019 (0.63)	0.044 (1.62)	0.046 (1.71)
Post Reform*Age 25-40	-0.022 (0.96)	-0.023 (0.98)	-0.024 (0.88)	-0.026 (0.93)	-0.051 (2.11)**	-0.054 (2.22)**
Black	-0.005 (0.32)	-0.005 (0.34)	-0.010 (0.55)	-0.010 (0.57)	0.007 (0.38)	0.007 (0.36)
Education	0.008 (2.81)**	0.008 (2.77)**	0.009 (2.43)*	0.008 (2.36)*	0.006 (1.78)*	0.006 (1.82)*
Experience	-0.001 (0.38)	-0.002 (0.44)	-0.002 (0.38)	-0.002 (0.46)	-0.003 (0.81)	-0.003 (0.86)
Experience Squared	0.000 (0.31)	0.000 (0.36)	0.000 (0.15)	0.000 (0.23)	0.000 (0.93)	0.000 (1.02)
Log Hourly Wage	-0.036 (3.09)**	-0.036 (3.09)**	-0.026 (1.82)*	-0.025 (1.78)*	-0.040 (3.05)**	-0.040 (3.06)**
Job Tenure	-0.011 (2.82)**	-0.014 (2.88)**	-0.009 (2.02)**	-0.015 (2.23)**	-0.008 (1.95)*	-0.011 (1.89)*
Employer Size	0.003 (0.25)	0.003 (0.23)	0.016 (1.16)	0.016 (1.15)	0.000 (0.01)	0.000 (0.02)
Employer Training	0.002 (0.11)	0.002 (0.10)	0.006 (0.27)	0.006 (0.28)	-0.009 (0.46)	-0.010 (0.49)
Employer Health Insurance	-0.013 (0.79)	-0.013 (0.79)	-0.024 (1.30)	-0.023 (1.27)	-0.010 (0.46)	-0.010 (0.50)
DC Plan	-0.012 (0.88)	-0.011 (0.81)				
Union Member	-0.023 (1.65)*	-0.023 (1.67)*	-0.033 (2.05)**	-0.033 (2.09)**	-0.025 (1.75)*	-0.025 (1.76)*
State Unemployment Rate	-0.002 (0.61)	-0.003 (0.64)	0.001 (0.12)	0.000 (0.07)	-0.003 (0.62)	-0.003 (0.60)
SMSA	0.028 (2.63)**	0.028 (2.65)**	0.024 (1.92)*	0.024 (1.92)*	0.024 (2.00)**	0.024 (2.04)**
Year 84	0.011 (0.65)	0.011 (0.64)	0.004 (0.22)	0.004 (0.21)	0.017 (0.91)	0.016 (0.85)
Pension Loss		0.042 (0.78)		0.058 (0.95)		0.017 (0.31)
Post Reform* Post Pension Loss		0.022 (0.26)		0.021 (0.24)		0.063 (0.77)
Log Likelihood	653.4	652.8	447.8	447.1	423.2	322.7
Observed P	7.78	7.78	7.96	7.96	6.9	6.9
Predicted P(X)	6.88	6.88	6.65	6.65	5.9	5.9
Sample Size	2.506	2.506	1.721	1.721	1.783	1.783



Table 14: Probit Job Mobility Equation. Sample 2. Females

	Control Group 1		Control Group 2		Control Group 3	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Post Reform*DB Plan	0.026 (1.19)	0.057 (0.76)	0.031 (1.26)	0.038 (0.52)	0.017 (0.72)	0.054 (0.79)
DB Plan	-0.036 (2.30)**	-0.072 (1.36)	-0.044 (2.42)**	-0.076 (1.11)	-0.002 (0.11)	-0.046 (0.74)
Post Reform	-0.003 (0.14)	-0.003 (0.17)	0.002 (0.10)	0.002 (0.10)	-0.007 (0.27)	-0.009 (0.35)
Age 25-40	-0.028 (1.16)	-0.028 (1.16)	-0.039 (1.39)	-0.039 (1.36)	-0.029 (1.05)	-0.029 (1.07)
Post Reform*Age 25-40	0.030 (1.37)	0.032 (1.41)	0.019 (0.78)	0.020 (0.77)	0.049 (1.92)	0.053 (2.00)*
Black	-0.009 (0.69)	-0.009 (0.69)	0.001 (0.04)	0.001 (0.04)	-0.012 (0.85)	-0.012 (0.85)
Education	0.004 (1.42)	0.004 (1.38)	0.004 (1.07)	0.004 (1.04)	0.002 (0.70)	0.002 (0.63)
Experience	0.003 (1.02)	0.003 (0.99)	0.007 (1.84)*	0.007 (1.83)*	0.001 (0.24)	0.001 (0.23)
Experience Squared	0.000 (1.21)	0.000 (1.22)	0.000 (2.12)**	0.000 (2.12)**	0.000 (0.32)	0.000 (0.37)
Log Hourly Wage	-0.017 (1.46)	-0.017 (1.47)	-0.029 (2.10)**	-0.029 (2.12)**	-0.018 (1.39)	-0.018 (1.41)
Job Tenure	-0.003 (0.86)	-0.005 (1.02)	-0.003 (0.81)	-0.005 (0.90)	-0.001 (0.33)	-0.003 (0.58)
Employer Size	-0.013 (1.20)	-0.013 (1.21)	-0.012 (0.93)	-0.012 (0.93)	-0.022 (1.84)	-0.022 (1.85)
Employer Training	0.022 (1.10)	0.022 (1.10)	0.028 (1.22)	0.028 (1.20)	0.002 (0.11)	0.002 (0.12)
Employer Health Insurance	-0.002 (0.20)	-0.003 (0.22)	0.009 (0.60)	0.008 (0.59)	-0.038 (2.36)**	-0.039 (2.41)**
DC Plan	-0.027 (2.21)**	-0.027 (2.19)**				
Union Member	-0.029 (2.08)**	-0.029 (2.09)**	-0.025 (1.59)	-0.026 (1.60)	-0.023 (1.70)*	-0.023 (1.71)*
State Unemployment Rate	0.001 (0.30)	0.001 (0.28)	0.003 (0.54)	0.002 (0.53)	0.002 (0.37)	0.001 (0.34)
SMSA	0.019 (1.86)*	0.019 (1.85)*	0.014 (1.13)	0.013 (1.12)	0.019 (1.76)*	0.019 (1.76)*
Year 84	0.009 (0.56)	0.009 (0.58)	0.009 (0.52)	0.010 (0.52)	0.017 (0.92)	0.017 (0.95)
Pension Loss		0.036 (0.73)		0.026 (0.46)		0.037 (0.73)
Post Reform*Post Pension loss		-0.031 (0.40)		-0.007 (0.09)		-0.040 (0.56)
Log Likelihood	580.2	580.2	438.8	438.7	342.4	342.1
Observed P	6.9	6.9	7.4	7.4	5.65	5.65
Predicted P(X)	6.06	6.05	6.2	6.2	4.76	4.75
Observations	2.422	2.422	1.769	1.769	1.663	1.663

Table 15: Probit Job Mobility Equation. Sample 3. Males

	Control Group 1		Control Group 2		Control Group 3	
	Model 1	Model 2	Model 1	Model 2	Model 2	Model 2
Post Reform*DB Plan	0.011 (0.40)	0.007 (0.09)	-0.005 (0.17)	-0.015 (0.20)	0.017 (0.61)	-0.017 (0.25)
DB Plan	-0.015 (0.66)	-0.062 (0.92)	-0.008 (0.31)	-0.113 (1.28)	-0.016 (0.73)	-0.011 (0.15)
Post Reform	0.053 (1.69)*	0.053 (1.68)*	0.063 (1.72)*	0.065 (1.76)*	0.113 (2.44)**	0.116 (2.48)**
Age 25-40	0.036 (1.12)	0.037 (1.14)	0.027 (0.72)	0.030 (0.82)	0.097 (2.36)**	0.099 (2.39)**
Post Reform*Age 25-40	-0.036 (1.18)	-0.035 (1.15)	-0.025 (0.72)	-0.027 (0.75)	-0.118 (2.65)**	-0.121 (2.69)**
Black	0.004 (0.22)	0.004 (0.21)	-0.001 (0.05)	-0.001 (0.06)	0.020 (0.97)	0.020 (0.96)
Education	0.009 (2.79)**	0.009 (2.74)**	0.012 (2.77)**	0.011 (2.67)**	0.006 (1.56)	0.006 (1.60)
Experience	0.000 (0.05)	0.000 (0.11)	0.000 (0.03)	-0.001 (0.14)	-0.004 (0.78)	-0.004 (0.81)
Experience Squared	0.000 (0.03)	0.000 (0.08)	0.000 (0.01)	0.000 (0.10)	0.000 (0.87)	0.000 (0.94)
Log Hourly Wage	-0.039 (2.87)**	-0.039 (2.86)**	-0.021 (1.33)	-0.020 (1.25)	-0.045 (3.11)**	-0.045 (3.12)**
Job Tenure	-0.010 (2.26)**	-0.013 (2.35)**	-0.011 (2.11)**	-0.020 (2.49)**	-0.005 (1.14)	-0.007 (0.97)
Employer Size	0.005 (0.37)	0.005 (0.36)	0.012 (0.78)	0.012 (0.78)	0.009 (0.67)	0.009 (0.65)
Employer Training	0.051 (1.76)*	0.051 (1.75)*	0.063 (1.83)*	0.064 (1.85)*	0.033 (1.08)	0.032 (1.06)
Employer Health Insurance	-0.027 (1.46)	-0.027 (1.46)	-0.040 (1.92)*	-0.040 (1.90)*	-0.018 (0.79)	-0.018 (0.81)
DC Plan	-0.005 (0.32)	-0.004 (0.25)				
Union Member	-0.021 (1.28)	-0.021 (1.29)	-0.026 (1.40)	-0.027 (1.45)	-0.024 (1.50)	-0.024 (1.51)
State Unemployment Rate	0.002 (0.40)	0.002 (0.39)	0.005 (0.76)	0.005 (0.74)	0.003 (0.53)	0.003 (0.56)
SMSA	0.30 (2.39)**	0.3 (2.39)**	0.21 (1.43)	0.21 (1.41)	0.27 (1.93)*	0.26 (1.94)*
Pension Loss		0.050 (0.76)		0.092 (1.26)		-0.005 (0.07)
Post Reform* Post Pension Loss		0.002 (0.02)		0.010 (0.10)		0.045 (0.50)
Log Likelihood	520	519.5	355.63	355.48	336.2	336.04
Observed P	8.23	8.23	8.35	8.35	7.44	7.44
Predicted P(X)	7.2	7.2	6.9	6.9	5.9	5.89
Sample Size	1.930	1.930	1.330	1.330	1.371	1.371

Table 16: Probit Job Mobility Equation. Sample 3. Females

	Control Group 1		Control Group 2		Control Group 3	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Post Reform*DB Plan	0.011 (0.41)	0.014 (0.19)	0.010 (0.35)	-0.017 (0.23)	0.015 (0.53)	0.017 (0.24)
DB Plan	-0.016 (0.75)	-0.033 (0.52)	-0.020 (0.80)	0.007 (0.09)	0.001 (0.03)	-0.033 (0.45)
Post Reform	-0.006 (0.23)	-0.006 (0.22)	-0.007 (0.21)	-0.006 (0.17)	-0.019 (0.54)	-0.018 (0.52)
Age 25-40	-0.014 (0.44)	-0.013 (0.43)	-0.034 (0.92)	-0.032 (0.86)	-0.016 (0.47)	-0.016 (0.45)
Post Reform* Age 25-40	0.017 (0.62)	0.017 (0.61)	0.022 (0.69)	0.019 (0.57)	0.033 (1.11)	0.034 (1.09)
Black	-0.012 (0.78)	-0.012 (0.78)	0.001 (0.03)	0.001 (0.04)	-0.013 (0.80)	-0.013 (0.78)
Education	0.003 (0.81)	0.003 (0.80)	0.004 (0.88)	0.004 (0.92)	0.000 (0.01)	0.000 (0.02)
Experience	0.004 (1.11)	0.004 (1.10)	0.009 (1.84)*	0.009 (1.87)*	0.001 (0.14)	0.001 (0.14)
Experience Squared	0.000 (1.26)	0.000 (1.25)	0.000 (1.97)**	0.000 (1.96)**	0.000 (0.26)	0.000 (0.28)
Log Hourly Wage	-0.021 (1.56)	-0.021 (1.58)	-0.036 (2.25)**	-0.036 (2.24)**	-0.018 (1.19)	-0.019 (1.22)
Job Tenure	-0.004 (0.92)	-0.005 (0.91)	-0.003 (0.63)	-0.003 (0.35)	-0.004 (0.84)	-0.007 (0.95)
Employer Size: >100	-0.019 (1.56)	-0.019 (1.55)	-0.019 (1.27)	-0.019 (1.28)	-0.028 (2.00)**	-0.028 (1.98)**
Employer TRaining	-0.012 (0.48)	-0.012 (0.49)	-0.009 (0.29)	-0.010 (0.32)	-0.026 (1.07)	-0.026 (1.08)
Employer Health Insurance	-0.010 (0.70)	-0.010 (0.71)	0.004 (0.24)	0.004 (0.25)	-0.046 (2.49)**	-0.046 (2.51)**
DC Plan	-0.020 (1.34)	-0.020 (1.33)				
Union Member	-0.028 (1.74)*	-0.028 (1.75)*	-0.021 (1.13)	-0.021 (1.11)	-0.022 (1.35)	-0.022 (1.37)
State Unemployment Rate	-0.001 (0.19)	-0.001 (0.19)	0.000 (0.06)	0.000 (0.07)	0.001 (0.15)	0.001 (0.15)
SMSA	0.014 (1.14)	0.014 (1.14)	0.006 (0.41)	0.006 (0.40)	0.015 (1.14)	0.015 (1.13)
Pension Loss		0.017 (0.29)		-0.025 (0.36)		0.031 (0.49)
Post Reform*Post Pension Loss		-0.005 (0.05)		0.039 (0.40)		-0.004 (0.05)
Log Likelihood	465.5	465.45	347.9	347.85	286.6	286.46
Observed P	7.17	7.17	7.69	7.69	6.2	6.2
Predicted P(X)	6.36	6.36	6.64	6.64	5.22	5.21
Sample Size	1.883	1.883	1.353	1.353	1.306	1.306