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Evaluating Pension Portability Reforms: The Tax Reform Act of 1986 as a Natural Experiment

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SEDAP Research Paper No. 160

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Evaluating Pension Portability Reforms. The Tax Reform Act of 1986 as a Natural Experiment.

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Abstract

This paper uses the Tax Reform Act of 1986 as a natural experiment to evaluate the job mobility response of prime-aged US employees participating in employer sponsored defined benefit pension plans to a reduction in the vesting period for pension rights accrual. We apply difference-in-differences methods using data from the Survey of Income and Program Participation to estimate the treatment impact of this policy change. We find that on average 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 difference-in-differences estimators.

Keywords: Labour mobility, Employer Provided Pension Plans, Vesting, Program Evaluation, Propensity Score Matching.
JEL classification: J24, J44, J62, J63, J68

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Evaluating Pension Portability Reforms: The Tax Reform Act of 1986 as a Natural Experiment

Résumé

Cet article utilise l’expérience naturelle issu de la reforme fiscale de 1986 pour évaluer l’incidence d’une réduction de la période d’acquisition des droits de pension sur la mobilité des travailleurs américains couverts par un régime de pension à prestations déterminées. Nous appliquons des méthodes de différence des différences sur les données du *Survey of Income and Program Participation* pour estimer l'impact de cette réforme sur le groupe affecté. Nos résultats suggèrent que la réforme n'a eu aucun effet significatif sur la mobilité volontaire du travail du groupe de traitement. Nos résultats sont robustes à l'utilisation de différents groupes témoins et estimateurs de différence des différences.

1 Introduction

Federal policy regarding the portability of pension rights has been an important issue of public debate in the US since the 1960s. 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 (TRA86) further reduced the maximum vesting period required for pension rights' accrual from 10 to 5 years of plan participation.

Since this policy debate, which was driven by efficiency and equity considerations, most of the empirical investigations have focused on the likely effects of enhanced pension portability on retirement income and employee productivity. On the one hand simulation studies[1] suggest that despite shorter vesting periods, benefit losses associated with job turnover are still significant in some cases for workers participating in defined benefit (DB) plans. On the other hand, while participation in employer-sponsored pension plans is found to be strongly correlated with less frequent quits and layoffs,[2] there is disagreement over whether reduced job mobility arising from nonportable pensions enhances or reduces the efficiency of labor markets.[3]

Policy reforms reducing the length of the vesting periods have been enacted in the last two decades in most industrialized countries with widespread employer-provided pension coverage,[4] yet no empirical study has formally evaluated the impact of these policies. This paper contributes to fill this gap using the natural experimental design introduced by TRA86 to study the impact of a reduction in vesting provisions on the voluntary job mobility of US private sector prime-aged employees using data from the Survey of Income and Programme Participation (SIPP). Given the typical structure of

[1] See Clark and McDermed (1988); Employee Benefit Research Institute (1987).

[2] Mitchell (1983); Allen et al. (1988, 1993); Andrietti and Hildebrand (2001).

[3] See Dorsey (1995) for a review of the literature.

[4] See Andrietti (2002) for a review of the reforms implemented in the United States, Canada, Ireland, the United Kingdom and the Netherlands.

US employer-provided pension plans, this reform affects workers participating in a DB plan with 5 to 10 accrued years of service, but is not expected to have any effect on workers with similar years of service who do not participate in a DB plan. The latter group includes workers participating in an employer-provided defined contribution (DC) plan and all those without any employer's pension coverage. In order to identify the effect of the reform, we adopt a difference-in-differences strategy, which compares the pre-post reform change in voluntary job mobility for workers treated by the reform against workers with similar characteristics who are not affected and therefore taken as the control group. Our results suggest that the reform has no statistically significant impact on voluntary job mobility of the DB workers. This finding is robust to model specifications, the choice of different control groups and pre-post reform samples.

The paper is organized as follows. The next section describes employer provided plans and their vesting provisions in the US. Section 3 introduces our empirical strategy. Section 4 describes the data. Section 5 presents the results. Section 6 concludes.

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

Employer-provided pension plans typically fall into one of two broad categories: DB and DC. 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 the final salary received with the pension promise usually being funded through employers' contributions. DC plans provide for periodic contributions into an individual pension account for each worker, where contributions may be made by the firm and/or the workers. The level of benefit at retirement is determined by the total amount of contri-

butions and the rate of return of each individual's retirement assets. Although different types of DC plans[5] are offered in the US, most of them have the so-called *401(k) option*, which allows participant employees to make pre-tax contributions.[6] In principle, employers can establish 401(k) plans that rely entirely on voluntary employee contributions. However, they usually offer matching contributions up to a prespecified limit.

Individuals enrolled in either DB or DC type pension plans are subject to a vesting period before being fully entitled to their pension rights. Once vested, a worker can quit her job and retain the legal right to receive the future pension benefit to which she has been contributing. 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 the age of 25, those working less than 1,000 hours annually and those within 5 years of normal retirement age (not to exceed age 65).[7] 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, reaching 100 percent vesting after 15 years. Finally, under the third vesting standard known 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*, which were defined as profit sharing, stock bonus, or money purchase plans providing for an employee's

[5]Money purchase plans, saving and thrift plans, profit sharing plans, stock bonus plans and employee stock ownership plans.

[6]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.

[7]Employee Benefit Research Institute (1986).

rights to contributions for each plan year separately. In this case, the plan had to ensure a 5-year vesting schedule for employer contributions with 100 percent vesting no later than the end of the fifth plan year. In addition, plans could allow participants to vest more quickly than the minimum standards set by law. 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. The *class vesting* schedule was eliminated. These changes became effective for plan years beginning on January 1, 1989.

Graham (1988) uses data from the 1986 Employee Benefit Survey administered to medium and large private-sector firms to determine the influence of ERISA on vesting schedules. According to his survey, the vast majority of individuals participating in DB plans were subject to a 10-year cliff vesting schedule, 13 percent were subject to graded vesting schedules and overall only 10 percent of participants were offered more liberal vesting time schedules than those prescribed by ERISA. This is compared to DC plan participants where more than one quarter of workers were given immediate full vesting while a minority of participants were offered cliff vesting within 5 years. Finally, most *graded* and *class* vesting schedules were providing full vesting after 5 years of service. Similar figures are provided by Kotlikoff and Smith (1983). This constitutes evidence that the vesting schedules of nearly all DB plans needed to be amended to comply with the new standards introduced by the TRA86, while most DC plans already provided much more liberal vesting schedules than those prescribed by ERISA and therefore were already in compliance with the new legislation. Currently, most DC plans allow for the immediate vesting of employee contributions, while virtually all DB plans impose five year vesting periods (Woods, 1993).

3 Empirical Strategy

The preceding evidence suggests that reductions in the vesting period required to become eligible to claim accrued pension rights introduced by TRA86 almost exclusively affected workers enrolled in DB plans with 5 to 10 years of service. Thus, the reform provides a transparent exogenous source of variation that determines treatment assignment. We exploit this naturally occurring experimental design to evaluate job mobility responses of pension-covered workers to the reduction in vesting provisions using a difference-in-differences (DD) strategy. This commonly used quasi-experimental estimator allows us to measure the impact of TRA86 by comparing the difference in voluntary job mobility rates between workers enrolled in employer-provided DB plans with 5 to 10 years of service (the treatment group) with comparable respondents not participating in any DB plan (the control group).

Both DC-covered workers and workers in nonpension jobs (with 5 to 10 years of service) provide potentially suitable comparison groups because these workers were not affected by TRA86. However, it is well known that workers in nonpension jobs differ from pension-covered workers in both observable and non-observable characteristics (Gustman and Steinmeier, 1993). In particular, there is widespread evidence that pension-covered workers are on average better educated, command higher wages than their non-covered counterparts and are intrinsically less mobile (Gustman and Steinmeier, 1993). The existence of common time-specific shocks across both groups, which is a critical assumption underlying the validity of the DD approach, is more likely to hold when both the control and the treatment groups share comparable characteristics.[8] Thus, DC-covered workers constitute our preferred control group given its similarities with the treatment group. However, we also integrate workers in nonpension jobs as an alternative control group to assess the robustness of our estimates

[8] See, for instance, Meyer (1995) for a detailed discussions.

to different sources of potential bias. In particular, given the differences in observables between nonpension workers and the treatment group, one might suspect that both groups could potentially exhibit differential responses to macroeconomic shocks, thus violating a fundamental identifying assumption of the DD estimator. A robust set of results from multiple control groups would, however, strengthen our belief that we are actually identifying the true effects of the vesting reform and not merely the effect of other contemporaneous changes or trend differences between the control and treatment groups.

We first implement a conventional linear DD estimator and offer additional robustness checks from difference-in-differences matching (DDM) following the methodology outlined in Heckman et al. (1997) and Heckman et al. (1998). The standard unadjusted (raw) difference-in-differences model is captured by the following equation:

$$M_i = \gamma_0 + \gamma_1 DB_i + \gamma_2 Post_i + \gamma_3 Post_i \cdot DB_i + \varepsilon_i \tag{1}$$

where dummy variables DB_i and $Post_i$ denote respectively whether individual i is in the treatment group (DB-covered) and observed after the policy reform. Common unobservable differences among groups are controlled for by the variable DB_i, while $Post_i$ controls for common macroeconomic effects. The coefficient γ_3 of the interaction between these two variables measures the raw impact of the reform. The outcome variable M_i is a binary variable equal to one if a worker experienced a voluntary job-to-job transition.

We also consider an augmented specification by adding a vector of demographic and job-related characteristics X_i:

$$M_i = \gamma_0 + \gamma_1 DB_i + \gamma_2 Post_i + \gamma_3 Post_i \cdot DB_i + \beta X_i + \varepsilon_i \tag{2}$$

The adjusted DD model allows us to control for differences in observables between observations in the treatment and the control groups. It has also the advantage of reducing the residual variance of the regression, which yields more efficient estimates (Meyer, 1995). Given the dichotomous nature of our dependent variable M_i, we assume that the equation errors are normally distributed and estimate a probit model.

In addition to standard DD estimates, we provide further robustness checks from difference-in-differences matching. DDM combines traditional matching methods with difference-in-differences.[9] This estimator offers more flexibility than the above mentioned traditional DD estimator as one does not need to impose a linear functional form to estimate the conditional expectation of the outcome of interest. Unlike traditional matching, DDM is robust to the existence of systematic time-invariant unobserved differences between the control and the treatment groups (Heckman et al., 1997, 1998). Given the nature of our data, we implement our DDM estimator on repeated cross-sections as a two-way propensity score matching by pairing each worker covered by a defined benefit pension with a member(s) in the control groups that exhibit similar observables both in the pre- and the post reform periods. Formally, following the notation of Smith and Todd (2005), the estimated effect of the reform is given by:

$$\hat{\gamma}_{_{DDM}} = \frac{1}{n_{1A}} \sum_{i \epsilon I_{1A} \cap S_P} \left\{ Y_{1i}^{A} - \hat{Y}_{0i}^{A} \right\} - \frac{1}{n_{1B}} \sum_{i \epsilon I_{1B} \cap S_P} \left\{ Y_{0i}^{B} - \hat{Y}_{0i}^{B} \right\} \tag{3}$$

where I_{1A}, I_{1B} denote the sets of DB-covered workers in the periods following and preceding the implementation of TRA86 and S_P is the region of common support. Both n_{1B} and n_{1A} capture the number of DB-covered workers for which we find a match in the pre and post-reform periods. Y_{0i}^{B} (Y_{1i}^{A}) is a dichotomous variable equal to one if a DB-covered respondent experienced a voluntary job transition in the pre

[9] See among others, Rosenbaum and Rubin (1983), Heckman et al. (1997), Blundell and Dias (2000) and Smith and Todd (2005) for detailed discussion on matching methods and difference-in-differences matching.

(post)-reform period. $\hat{Y}_{0i}^{A}$ and $\hat{Y}_{0i}^{B}$ denote the corresponding counterfactual outcomes which are constructed as the weighted average outcomes of seemingly *comparable* non treated workers. It can be expressed as

$$\hat{Y}_{ki}^{t} = \sum_{j \epsilon I_{0t} \cap S_P} w_{ij} Y_{0j}^{t}, \quad t = \{A, B\}, \ k = \{1, 0\} \tag{4}$$

where I_{0B} (I_{0A}) denotes the sample size of the control group in the pre (post) -reform period and w_{ij} denotes the specific weights assigned to each control j in the estimation of the counterfactual outcome of treated respondent i. The value of the latter depends on the distance between the propensity scores of i and j and the choice of the matching algorithm. To check the sensitivity of our results to the choice of matching estimator, we consider four different matching procedures; single nearest neighbor, radius matching, kernel and local linear matching.[10]

4 Data

The data used for this analysis is drawn from pooling the 1984, 1996, 1990, 1992 and 1996 panels of the Survey of Income and Program Participation (SIPP). Each survey year is a short rotating panel made up of 8 to 12 waves of data – collected every 4 months – for approximately 14,000 to 36,700 U.S. households.[11] The choice of these particular panel years is motivated by the timing of the implementation of TRA86 and the availability of pension coverage information. Both the 1984 and 1986 panels cover the pre-reform period, while subsequent surveys cover the post-reform period. By

[10] All our estimates were obtained using the *psmatch2 STATA* module of Leuven and Sianesi (2003)

[11] Each survey year typically covers a time span ranging from 2 1/2 years to 4 years. In particular, SIPP 1984 spans over 32 months from October 1983 to July 1986; SIPP 1986 spans over 28 months from January 1986 to April 1988, SIPP 1990 spans over 32 months from February 1990 to September 1992, SIPP 1992 spans over 40 months from February 1992 to April 1995 and SIPP 1996 spans over 48 months from April 1996 to March 2000.

pooling these panel years, we construct a unique synthetic panel, which allows us to fully exploit the quasi-experimental design of TRA86 using difference-in-differences methods.

Each SIPP panel collects *core* module questions that are common to each wave and *topical* module questions that provide in-depth information on particular topics that are usually not updated in each wave. We draw pension data from the topical module on pension coverage.[12] We use this information to assign each worker a (mutually exclusive) pension participation status; either participating in a DB plan, participating in a DC plan or not participating in any pension arrangement. Since specific individual plan characteristics of pension-covered respondents are not available in the SIPP data, we rely on figures provided by Graham (1988) and Kotlikoff and Wise (1985) and assume that the typical vesting schedule of DB plans before TRA86 was a *cliff* with 10-year vesting and where the typical vesting schedule of DC plans provided full vesting within 5 years.

We exploit the longitudinal design of the core module questions to identify job transitions for each respondent over a four-wave period. In particular, our observation period spans waves four to seven of the 1984, 1986, 1990 and 1992 panels, and waves seven to ten of the 1996 panel.[13] Employees who experienced a voluntary job transition are the most pertinent units for our analysis. However, prior to the 1996 panel, SIPP did not collect explicit information regarding the reasons behind a job change. As a result, we have constructed a proxy measure that considers whether a move is voluntary when a worker switches jobs without any intervening spell of unemployment over the period during which we observed each respondent.

[12] Topical modules on pension coverage were collected in wave 4 of each panel year used in this paper with the exception of SIPP 1996, which collected pension coverage information in wave 7.

[13] The first wave of observation corresponds to the wave in wich the pension coverage module was collected in each survey year. We restricted the observation period to four waves given that the 1986 survey collected seven waves of data and that SIPP 84 lost an entire rotating group after wave 7.

We restrict our 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 by unions in the form of multiemployer plans.[14] The latter eliminates 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 continuously for the same employer. Thus, construction workers usually combine high turnover with little discontinuity of pension coverage. In the same fashion, public sector workers are excluded, as they experience different patterns of turnover and because public pension plans usually have more generous portability provisions.[15] Finally, to avoid sample selection issues related to labor market entry at a young age and exit at an advanced age, we restrict our sample to prime-age workers between 25 and 50.

As previously discussed, we consider two control groups: workers with 5 to 9 years of job tenure covered by a DC plan and nonpension workers with similar years of service. We estimate our models on two different samples. The first sample (Sample 1) uses the 1984 and 1986 panels as pre-reform data and the 1990, 1992 and 1996 panels as post-reform data. In order to account for the progressive enactment of the reform, we also consider a more restrictive sample in which only the 1992 and 1996 panels are used to cover the post-reform period (Sample 2).

Table 1 reports the weighted mean values of relevant characteristics of the treatment and control groups for both samples. We find differences in job mobility rates across all groups of workers. Consistent with Gustman and Steinmeier (1993), the reported differences are significantly more important between nonpension (control group

[14] See Weinstein and Wiatrowski (1999)
[15] See Foster (1994).

2) and pension-covered workers (treatment group and control group 1). As expected, pension-covered workers and nonpension workers appear to differ along other dimensions as well. DB and DC-covered workers earn on average higher wages than nonpension workers, are better educated, are more likely to own their own home and to be working in large firms. Nonpension workers are significantly less likely to be union members than their pension-covered counterparts. However, a significant difference in union coverage is also observed between workers covered by DC and DB plans. About 30 percent of DB-covered workers are union members. This figure drops to 13 percent amongst DC-covered workers and just below 9 percent for nonpension workers. This finding is consistent with the well-documented union *advantage* in fringe benefits – including pension coverage – and the fact that most union-negotiated pension plans are of the defined-benefit type (U.S. Bureau of Labor Statistics, 2005). Overall, the figures reported in Table 1 corroborate findings from earlier studies that both DB and DC-covered workers share similar observable characteristics, which allows the latter to exist as a particularly suitable comparison group.

Figure 1 illustrates the incidence of voluntary job mobility of workers from our treatment and control groups over our observation period. Interestingly, it shows a sudden surge in job mobility of DB-covered workers in the period following TRA86 implementation. However, the mobility trend of DC-covered workers displays an almost identical pattern. This latest observation could suggest that the observed surge in the job mobility of DB-covered workers reflects a response to contemporaneous macroeconomic shocks that are common to pension-covered workers independently of TRA86 reforms. This finding provides additional support to our identifying assumption that both groups of pension-covered workers are indeed equally affected by macroeconomic shocks. On the contrary, unlike pension-covered workers, nonpension workers experienced a fairly smooth and constant increase in job mobility, which sug-

gests that macroeconomic shocks might have a differential impact on this latter group.

Table 2 summarizes the trends displayed in Figure 1 and presents the average voluntary job mobility rates for the treatment and control groups with their respective sample sizes in the pre- and post-reform periods for each of our two samples. The figures reported confirm the observed higher voluntary job mobility for all groups of workers in the period following TRA86 implementation. More precisely, the increase in mobility of DB-covered workers ranges from 2.28 percentage points in sample 1 to 3.25 percentage points in sample 2. We find similar figures for DC-covered workers with an increase of 2 percentage points in sample 1 and 2.92 percentage points in sample 2, while nonpension workers' job mobility increased by about 4 percentage points.

Both the trends in mobility across groups reported in Figure 1 and the statistics reported in Table 2 provide preliminary evidence that the pension reforms introduced by TRA86 probably did not trigger the observed surge in voluntary job mobility of DB-covered workers. The difference-in-differences estimates, discussed in the next section, provide a more rigorous evaluation of this preliminary assessment.

5 Estimation Results

Tables 3 and 4 report standard difference-in-differences results based on the estimation of three model specifications on two samples using DC-covered workers and nonpension workers respectively as comparison groups. Columns 1 and 4 report the unadjusted (raw) difference-in-differences marginal effects of the reform from estimating equation (1). Note that the latter are easily derived from the mobility rates reported in Table 2 by subtracting the pre/post mobility differential of each control group from that of DB workers. These unadjusted estimates confirm our preliminary observation

that the reduction of the vesting requirements did not yield a statistically significant increase in the job mobility of DB-covered workers relative to those covered by a DC plan. Similar results are found when nonpension workers are used as the comparison group (see Table 4).

Because the treatment group and the control groups may differ in demographic and job-related characteristics, the observed differences in job mobility outcomes could simply reflect the underlying differences between the treatment and control groups rather than a treatment effect. Hence, controlling for demographic and job-related characteristics is important if the composition of the treatment and control groups changes over time and some of these characteristics are correlated with the outcome of interest. In addition, controlling for demographic and job-related characteristics reduces the residual variance of the regression and produces more efficient estimates. To account for these differences and to check the robustness of our estimates to model specification, we provide additional sets of results from the estimation of two alternative specifications of equation (2). The first specification includes controls for personal and family-related characteristics[16]proxying for mobility costs. The second specification is augmented by the addition of job-related[17] and local labour market[18] characteristics. Industry dummies - with the manufacturing industry as the reference group - and occupation dummies - with blue-collar occupations as the reference group - are included to proxy for industry and occupation specific turnover rates faced by individuals. We report the adjusted marginal effects of the policy in columns 2 and 3 for sample 1 and in columns 5 and 6 for sample 2.[19] Overall, we find that the unadjusted estimates are robust to the inclusion of controls for demographics and job related characteristics.

[16]In particular, we control for job potential experience, education, race, marital status, the number of children aged less than 18, family size and geographical areas (SMSA as well as regions).

[17]Included are dummy variables for firm size, union status, industry and occupation, and continuous variables for log-hourly wage and job tenure.

[18]State unemployment rates.

[19]Full estimation results are available from the authors upon request.

Regardless of the sample and the comparison group considered, we consistently find no statistically significant evidence that TRA86 pension reform had any impact on the job mobility decisions of DB-covered workers.

Finally, further robustness checks are provided from difference-in-differences matching by estimating equation (3). Following the methodology outlined in Section 3, we use several matching algorithms to pair each DB-covered respondent – in the region of common support – with a weighted average of the control group's respondents based on the value of their propensity scores[20] in the pre- and post-reform periods. Each resulting individual counterfactual outcome is then used to derive an estimate of the mean difference in outcomes in both periods. The difference-in-differences matching estimates are obtained by subtracting the post- and the pre-reform mean difference estimates in job mobility, and are reported in Tables 5 and 6. These estimates corroborate unambiguously both the unadjusted and adjusted regression results; the vesting reform of TRA86 did not succeed in its main objective of fostering the voluntary job mobility of workers "tied" to jobs offering an occupational pension plan with a long vesting period. Moreover, the fact that the treatment effect coefficient (γ) is robust to the inclusion of additional controls and estimation methods implies that any changes in the composition of the treatment and control groups that occurred over time are most likely uncorrelated with the treatment.

6 Conclusions

This paper exploits the natural experimental design of the reduction in vesting period as introduced by TRA86 to evaluate the voluntary job mobility response of the affected

[20]The variables used to determine propensity scores include personal family-related characteristics, job-related characteristics and local labour markets characteristics. Full estimation results are available upon request from the authors.

respondents; workers participating in a DB plan with 5 to 10 years of service. We adopt a difference-in-differences strategy that compares the pre-post reform change in voluntary job mobility for workers affected by the reform relative to two groups of workers: DC-covered workers and nonpension workers who are not affected by the reform and therefore taken as the control group.

Our results unambiguously support that TRA86 had a no statistically significant effect on the voluntary job mobility rate of the treated group. Our estimates are robust to the choice of control groups, pre/post reform samples and estimation methods. Though the reform reduced the pension loss of workers participating in DB plans, it was ineffective in its main purpose of fostering the job mobility of workers "tied" to jobs by an employer-provided plan with a long vesting period. This is perhaps not surprising in the light of earlier contributions which failed to establish that the lack of pension portability was a key factor in explaining the well documented lower job mobility rate of pension-covered workers (Gustman and Steinmeier, 1993; Andrietti and Hildebrand, 2001). In addition, pension-covered workers are more likely to hold jobs offering better level of compensation and more generous fringe benefits, including health insurance, which is believed to generate a non-negligeable amount of job lock (Madrian, 1994; Buchmueller and Valletta, 1996). This evidence casts doubt on the effectiveness of policies targeting one particular program in isolation, and support the view that workers in "good jobs" are simply less likely to move.

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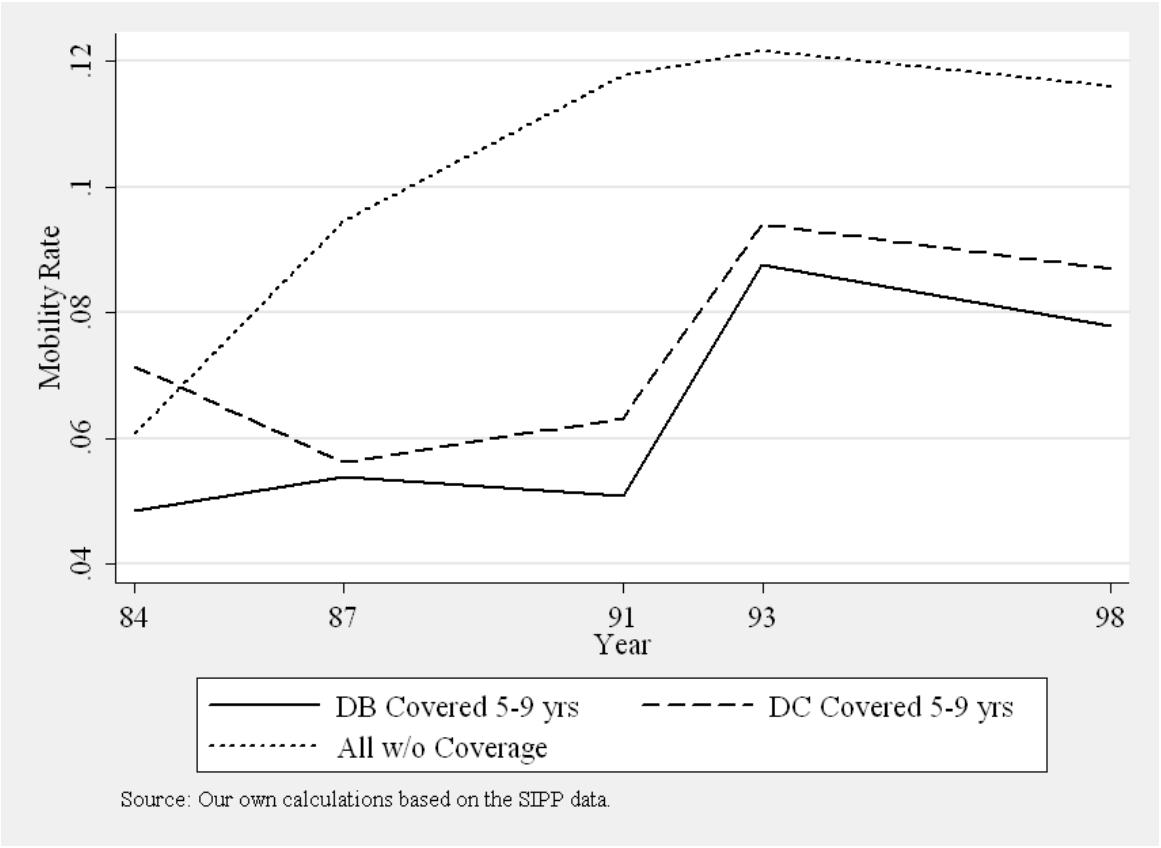

Figure 1: Job Mobility Rate of Treatment and Control Groups

Table 1: Sample Means of Characteristics for Treatment and Comparison Groups

	Treatment DB:5-9 yrs		Group 1 DC:5-9 yrs		Group 2 No Pension	
	s1	s2	s1	s2	s1	s2
Demographics						
Job Mobility (%)	6.6	6.9	7.5	7.9	10.6	10.2
Hourly wage	12.3	12.4	12.0	12.1	9.6	9.7
Age	35.3	35.2	35.0	35.1	34.7	34.7
Education (years)	14.2	14.1	14.1	14.1	13.1	13.0
Non-single (%)	75.2	75.2	72.8	73.2	65.4	65.7
Children<18 (%)	58.4	59.3	57.4	58.4	51.5	51.8
Black (%)	11.7	11.7	10.1	10.4	13.4	13.7
Family size	3.2	3.2	3.1	3.1	3.1	3.2
Housing tenure (%)	74.4	74.1	72.9	72.6	63.1	63.1
Job tenure	6.9	6.9	6.7	6.7	6.5	6.5
Firms<100 (%)	54.9	48.6	53.7	48.2	29.7	25.3
Union coverage (%)	28.6	27.8	13.5	13.0	8.8	8.9
Health coverage (%)	92.8	93.2	90.1	90.1	76.0	75.1
Industry						
Mining	1.3	1.5	1.6	1.8	1.2	1.4
Manufacturing	37.5	37.0	34.8	34.9	30.0	29.9
Transp.,Comm.	16.0	16.3	10.0	9.8	10.1	9.3
Wholesale trade	6.5	6.7	9.5	9.3	9.6	9.8
Retail trade	7.0	7.4	13.3	14.2	18.4	18.1
Finance, Insur.	5.8	5.3	5.8	5.1	2.4	2.5
Other services	24.9	24.9	24.1	23.8	26.5	26.9
Occupation						
Professional	35.5	34.1	35.1	35.9	21.9	21.8
Tech., Service	27.9	28.1	30.0	28.9	35.0	34.7
Blue Collar	36.6	37.8	34.9	35.2	43.1	43.6
Region						
SMSA	59.8	58.7	60.0	58.6	62.6	63.2
Midwest	25.9	25.7	29.6	29.2	26.0	26.4
Northeast	23.1	22.2	18.9	19.0	21.5	21.8
South	30.9	31.7	31.8	32.1	29.8	29.5
West	19.5	19.6	18.9	18.6	22.0	21.3
N	1823	1454	1496	1135	1478	1068

Note: All means weighted using SIPP person weights. S1 incluces data from the 1984, 1986, 1990, 1992 and 1996 SIPP surveys. S2 includes data from the 1984, 1986, 1992 and 1996 SIPP surveys. Hourly wage deflated using Monthly CPI-U BLS, Base=82-84.

Table 2: Pre-Post Reform Mobility Rates of Treatment and Control Groups

	Sample 1		Sample 2	
	Pre	Post	Pre	Post
DB-Covered	5.14 (545)	7.42 (1278)	5.14 (545)	8.39 (909)
DC-Covered	6.18 (366)	8.18 (1130)	6.18 (366)	9.10 (769)
All w/o Coverage	8.14 (397)	11.89 (1081)	8.14 (397)	11.96 (671)

Note: All means weighted using SIPP person weights. The number of observations in each cell is reported in parentheses. Sample 1: Pre reform=1984 & 1986 panels and Post reform=1990,1992 & 1996 panels. Sample 2: Pre reform=1984 & 1986 panels and Post reform=1992 & 1996 panels. Sample 3: Pre reform=1984 panel and Post reform=1996 panel

Table 3: Difference-in-Difference Estimates: DC- Covered Workers

	Sample 1			Sample 2		
	Raw	M1	M2	Raw	M1	M2
POST*DB	0.285 (0.886)	0.620 (0.772)	0.453 (0.829)	0.340 (0.877)	0.798 (0.729)	0.695 (0.759)
DB	-1.045 (0.517)	-1.331 (0.474)	-0.845 (0.644)	-1.045 (0.517)	-1.431 (0.454)	-1.089 (0.563)
POST	2.001 (0.194)	1.859 (0.217)	1.540 (0.310)	2.916 (0.087)	2.783 (0.092)	3.172 (0.076)

Note: All estimations are weighted using the SIPP person weights. P-values are reported in parentheses. Sample 1: Pre reform=1984 & 1986 panels and Post reform=1990,1992 & 1996 panels. Sample 2: Pre reform=1984 & 1986 panels and Post reform=1992 & 1996 panels.

Table 4: Difference-in-Differences Estimates: Nonpension Workers

	Sample 1			Sample 2		
	Raw	M1	M2	Raw	M1	M2
POST*DB	-1.468	-0.905	-0.937	-0.568	-0.177	0.142
	(0.509)	(0.689)	(0.675)	(0.815)	(0.941)	(0.952)
DB	-3.002	-3.451	-2.441	-3.002	-3.407	-3.072
	(0.087)	(0.088)	(0.225)	(0.087)	(0.091)	(0.126)
POST	3.754	3.407	2.986	3.824	4.012	3.969
	(0.041)	(0.026)	(0.049)	(0.054)	(0.015)	(0.018)

Note: All estimations are weighted using the SIPP person weights. P-values are reported in parentheses. Sample 1: Pre reform=1984 & 1986 panels and Post reform=1990,1992 & 1996 panels. Sample 2: Pre reform=1984 & 1986 panels and Post reform=1992 & 1996 panels.

Table 5: Difference-in-Differences Matching Estimates (Sample 1)

	1-Nearest Neighbor	Radius[a] Matching			Kernel[b] Matching			Local Linear[c] Regression Matching		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Group 1	1.01	0.54	0.69	0.82	0.53	0.54	0.82	1.28	0.21	0.44
	(2.286)	(2.258)	(2.084)	(2.073)	(2.310)	(1.910)	(2.014)	(2.617)	(2.131)	(2.087)
Group 2	-0.36	0.91	-1.39	-1.13	1.10	-0.99	-1.14	1.74	-0.05	-0.33
	(2.342)	(3.556)	(2.842)	(2.141)	(3.437)	(2.736)	(2.152)	(3.553)	(3.081)	(3.183)

Note: Own calculation from the SIPP data. Bootstrap standard errors in parentheses. Triming level 5.[a]Caliper=0.02,0.2 and 2.[b]Kernel Bandwidth = 0.02,0.2 and 2. [c]Local Linear regression bandwidth is 0.02,0.2 and 2

Table 6: Difference-in-Differences Matching Estimates (Sample 2)

	1-Nearest Neighbor	Radius[a] Matching			Kernel[b] Matching			Local Linear[c] Regression Matching		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Group 1	1.63	0.86	1.00	1.33	0.74	0.85	1.33	1.24	0.97	0.86
	(2.477)	(2.398)	(2.319)	(2.212)	(2.425)	(2.232)	(2.112)	(2.761)	(2.458)	(2.288)
Group 2	1.44	2.21	0.48	-0.27	2.42	0.52	-0.26	2.87	1.61	0.72
	(2.703)	(3.844)	(2.836)	(2.321)	(3.906)	(3.265)	(2.344)	(3.884)	(3.423)	(3.491)

Note: Own calculation from the SIPP data. Bootstrap standard errors in parentheses. Triming level 5.[a]Caliper=0.02,0.2 and 2.[b]Kernel Bandwidth = 0.02,0.2 and 2. [c]Local Linear regression bandwidth is 0.02,0.2 and 2

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