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EVALUATING PENSION PORTABILITY REFORMS. THE TAX REFORM ACT OF 1986 AS A NATURAL EXPERIMENT ABSTRACT

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This article exploits a change in the vesting rules for employer-sponsored pension plans introduced by the Tax Reform Act of 1986 to identify the causal effect of pension portability legislation on workers' voluntary mobility decisions. We pool data from different years of the Survey of Income and Program Participation to estimate the impact of this reform using difference-in-differences methods. Our results suggest that the reform had a positive and significant impact on voluntary job mobility of the treatment group. (*JEL* J24, J44, J62, J63, J68)

I. INTRODUCTION

With increasing mobility of the labor force,[1] portability of pension rights is an important policy issue for those countries where employer-sponsored pension plans play a major role in the provision of retirement income. In most of these countries the nature of employer-sponsored pension plans has gradually shifted, in the last two decades, from traditional defined benefit (DB) to more portable defined contribution (DC) types. While the shift has been particularly significant in the United States, DB plans remain dominant in countries such as Canada, Germany, Ireland, and the United Kingdom, where they still account for up to two thirds of workers' participation in employer-sponsored pension plans.[2] Moreover, countries such as Canada, Germany, Ireland, and the United Kingdom have recently passed reforms aimed at improving pension portability through shorter vesting periods and/or indexation of DB plans' vested pension rights (Andrietti 2002). In the United States, the Tax Reform Act of 1986 (TRA '86), a bill not explicitly focused on pension reforms, brought about a sharp cut in the maximum length of the vesting period required for full accrual of pension rights.[3]

While a large number of studies have focused on the impact of employer-sponsored pension plans and the incentives they create for job mobility choices,[4] very little is known about the role of pension portability features *per se* in job mobility decisions. In particular, to the best of our knowledge, no study has explicitly examined the extent to which vesting rules affect labor mobility. This article fills this gap by exploiting the

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1. Farber (2010) discusses the available evidence on recent declines in worker tenure and the incidence of long-term employment in the U.S. private sector. Among the studies indicating a decline in job stability, Jaeger and Stevens (2000) find a significant increase in the probability of a worker having fewer than ten years of tenure, while Neumark, Polsky, and Hansen (2000) provide evidence of a significant decline in eight-year worker retention rates.

2. See Munnell (2006) for a review, and OECD (2012), Office for National Statistics (2014), Statistics Canada (2014), and Wiatrowski (2012) for country-specific figures on plan participation rates.

3. Although TRA '86 introduced numerous policy changes—mostly focused on lowering marginal tax rates and broadening the tax bases (Auerbach and Slemrod 1997)—no other provisions specifically aimed at fostering job mobility.

4. See Dorsey (1995) and Ashok and Spataro (2014) for reviews of the early and more recent literatures, respectively.

ABBREVIATIONS

DB: Defined Benefit
DC: Defined Contribution
DD: Difference-in-Differences
DDM: Difference-in-Differences Matching
ERISA: Employee Retirement Income Security Act of 1974
HIPAA: Health Insurance Portability and Accountability Act of 1996
SIPP: Survey of Income and Program Participation
TRA '86: Tax Reform Act of 1986

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sharp change in vesting rules introduced by TRA ’86. This change in legislation offers a transparent source of plausibly exogenous variation that allows us to identify a group of employees who are vested under the new rules but would not have been vested otherwise and to use them as the treatment group.

Our results suggest that the change in vesting rules had a positive and significant impact on the voluntary job mobility of the treatment group. Moreover, an empirical assessment of robustness to potential measurement/classification errors suggests that the treatment effect estimated by pairing our preferred treatment/control groups may represent a lower estimation bound. Finally, the robustness of our findings to a number of falsification tests lends further support to a causal interpretation of our results.

The link between employer pension coverage and labor mobility is complex. Employer-sponsored pensions are often thought to be a significant impediment to worker mobility. This belief originated in early U.S. studies, which consistently reveal a significant negative association between employer pension coverage and job mobility (see, among others, Bartel and Borjas 1977; Mitchell 1982, 1983).

The “new pension economics” literature hypothesizes three possible causal pathways to explain this empirical finding. The conventional view, framed in implicit contract theory, relates this negative association to tenure-related quit costs imposed on workers who leave a DB plan before retirement due to long vesting periods and backloaded benefit accruals (Ippolito 1985).

The empirical support for this view (Ippolito 1987; Clark and McDermed 1988; Allen, Clark, and McDermed 1988, 1993), which contributed to its popularity among policy makers and academics alike, has been challenged by a number of studies that found little or no role of portability losses in explaining quit decisions. Gustman and Steinmeier (1993) argue that the compensation premium accruing to pension plan participants plays a central role in explaining their lower turnover, as evidenced by no significant difference between the turnover patterns of DB and DC plan participants. The latter finding contradicts the “implicit contract view” of pensions, given that DC pensions entail no quit costs.

To reconcile these seemingly conflicting views, Ippolito (2002) argues that compared to job switchers, job stayers exhibit a higher propensity to save and would therefore self-select into pension-covered jobs. The important finding—that savers appear to be “better workers” than non-savers—provides the link needed to reconcile the previous views. This suggests that pension plan participants receive compensation premiums due to their superior job performance, and explains at least some of the lower quit rates of pension-covered workers, even of the DC type. There is, however, little empirical evidence on the relevance of this selection issue. Haverstick et al. (2010) reveal that workers participating in DC plans in the 5–9-year tenure brackets are significantly more likely to switch jobs than workers participating in DB plans, even after the inclusion of a risk aversion index aimed at capturing the selection effect. In contrast, in a recent study relying on a natural experiment occurring at a single employer, Goda, Jones, and Manchester (2013) find that while enrolment in DC plans appears to be positively related to unobservable mobility tendencies, DC plan participants are also less likely to switch jobs than participants in DB plans. The latter finding suggests that unobservable attributes may dominate the mobility incentives created by higher portability.

Despite the abundance of empirical research focusing on improving our understanding of the pension-mobility nexus, no study to date has analyzed the role of vesting *per se* in explaining mobility decisions. Our difference-in-differences (DD) framework allows us to investigate the independent effects of vesting on labor mobility while controlling for selection on observable mobility-deterring factors such as compensation premiums, health insurance coverage, union status, and time-invariant unobservables.

Our study represents an important contribution to the literature. For one thing, it sheds further light on the complex pension-mobility nexus, confirming a significant impact of pension portability policies on voluntary job mobility, as posited by the implicit contract theory of pensions. Furthermore, our results are consistent with the “job lock” effects found in most of the literature analyzing the effects of employer-sponsored health insurance on job mobility, see, among others, Gruber and Madrian (1994) and Bansak and Raphael (2008).

II. BACKGROUND

Employer-sponsored pension plans typically fall into one of two broad categories: DB and DC plans. In a DB plan, employee pension rights’ accruals are based on earnings and years of service. In a DC plan, the employee and/or the employer contribute to the employee's individual account set up under the plan, with pension rights' accruals corresponding to the actuarially fair value of the contributed amounts.[5]

The DB/DC nature of the plan has implications for the portability of pension rights, defined as the ability of a worker to move to a different employer while preserving the actuarial value of her accrued pension rights. First, while individuals in both DB and DC plans gain non-forfeitable and inalienable (vested) rights to pension benefits only after meeting specific requirements for length of service, the latter are typically shorter in DC plans. Second, while the backloaded accrual of DB pension rights implies that a vested employee leaving a DB plan would still be incurring a loss (Ippolito 1985), a vested employee leaving a DC plan is always entitled to claim the actuarially fair value of her individual account.

This article exploits a sharp cut in the maximum length of the vesting period of tax-qualified pension plans under TRA '86 to uncover the role of vesting provisions in explaining voluntary job mobility of U.S. private-sector workers in employer-sponsored pension plans. To identify potential treatment and control groups and evaluate the impact of the reform, it is useful to define the different vesting schedules available to tax-qualified employer-sponsored pension plans.

A vesting schedule specifies the rate at which an employee qualifies to receive pension benefits or employer contributions to her plan. Employee contributions, typically not required in DB plans, are always vested immediately. There are two primary schedules: deferred full (*cliff*) vesting and graduated (*graded*) vesting. Under the former, benefits are not vested until a certain number of years of employment or service have been completed, after which benefits are 100% vested. When conditions for cliff vesting are satisfied, all accrued benefits are receivable at a later date (such as retirement). Under graded vesting, participants initially qualify for a percentage of accrued benefits, and the vested percentage increases with additional years of service.

5. More than 90% of the employer-sponsored DC plans offered in the United States are either savings/thrift or profit-sharing (see Andrietti 2015). Whereas savings/thrift plans require employee contributions, which are in most cases matched by employer contributions, profit-sharing plans do not usually require employee contributions, and the employer may determine, annually, how much will be contributed to the plan (out of profits or otherwise). Most DC plans include a cash or deferred agreement that allows employees to make tax-deferred contributions under Sec. 401(k) of the Internal Revenue Code.

TABLE 1
Minimum Vesting Requirements under ERISA and TRA '86

	ERISA	**TRA '86**
Cliff vesting	100% after 10 years	100% after 5 years
Graded vesting	100% after 15 years: - 25% after 5 years - 5% in years 6–10 - 10% in years 11–15	100% after 7 years: - 20% after 3 years - 20% in years 4–7
Alternative graded vesting	*Rule of 45*: - 50% if age + service = 45 after (min.) 5 or (max.) 10 years - 10% in each of the next 5 years	Eliminated
Class-year vesting	Each plan-year vested within 5 years	Eliminated

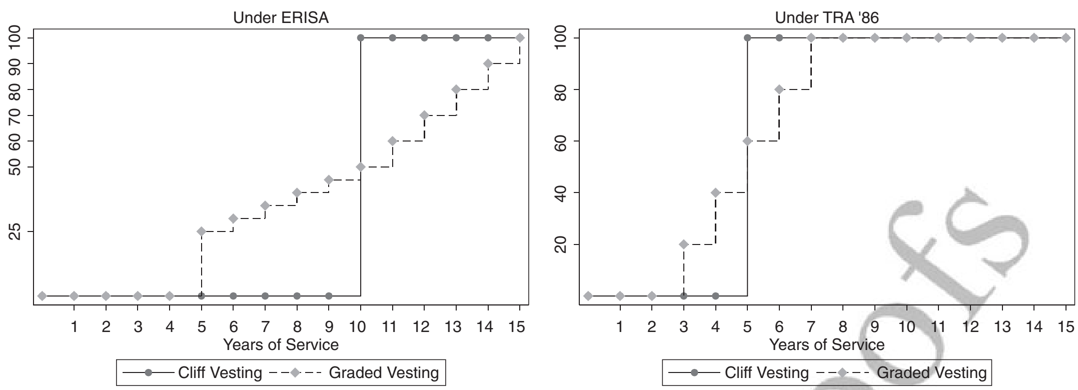
Prior to the enactment of the Employee Retirement Income Security Act of 1974 (ERISA), there were no federal statutory requirements governing the vesting of pension benefits. As a consequence, a high rate of benefit ineligibility was typical for participants in traditional DB plans, the dominant plan type at the time. According to tabulations of U.S. Department of Labor data for 1974—reported in Thompson (2005)—, 84% of active participants were enrolled in plans with benefits fully vested after 10 years of service. The remaining 12% were in plans that lacked provisions for vesting benefits prior to retirement. That is, employees who leave their employer before reaching retirement would forfeit their accrued pension benefits.

Table 1 displays the different types of vesting schedules available under ERISA and TRA '86. Figure 1 illustrates the shift in minimum vesting requirements following TRA '86 enactment. ERISA first introduced minimum vesting standards for private-sector pension plans (column 1 of Table 1 and left panel of Figure 1). The minimum vesting standards required certain accrued benefits to be fully (or partially) vested upon satisfaction of specific conditions.

Cliff vesting plans were required to vest 100% of accrued benefits at (or before) 10 years of service. *Graded* vesting plans were required to vest 100% of accrued benefits at 15 years of service; 25% of benefits were vested after 5 years, followed by 5% each additional year for the next 5 years, and an additional 10% each subsequent year for the next five.[6] ERISA vesting requirements went into effect on January 1,

6. An alternative graded vesting schedule—the *rule of 45*—and a shorter vesting schedule—*class-year* vesting—both rarely adopted under ERISA, were subsequently eliminated by TRA '86.

FIGURE 1
Minimum Vesting Requirements under ERISA and TRA ’86



Source: Our own elaboration on figures reported in Table 1.

1976. According to the figures reported earlier, virtually all DB plans had to comply with the new standards.

TRA ’86 tightened the minimum vesting standards established by ERISA while reducing the available vesting schedules (column 2 of Table 1 and right panel of Figure 1). Private single employer plans were allowed to provide either a 5-year cliff or a 7-year graded vesting schedule. The new vesting standards became effective for plan years beginning on January 1, 1989. They applied to all accrued benefits earned before and after the effective date, with the exception of plans that were part of a collective agreement and multiemployer plans. For the former, the new vesting rules applied in plan years beginning no later than January 1, 1991 (Tax Reform Act of 1986, publication 99-514). For the latter, the new vesting standard became effective only on January 1, 1997—after passage of the Small Business Job Protection Act of 1996—and employers were required to comply no later than January 1, 1999 (Small Business Job Protection Act of 1996, publication 104-188).

Data from the *Employee Benefits in Medium and Large Firms, 1986* survey (Bureau of Labor Statistics 1987) have been used to analyze the impact of ERISA on employer-sponsored plan vesting schedules as well as the potential impact of the change in vesting schedules introduced by TRA ’86 (Graham 1988). According to this survey—whose relevant figures are summarized in Table 2—87% of DB plan participants were in plans that offered a 10-year cliff vesting schedule, while 10% were offered graded vesting schedules of at least 10 years. By contrast, 25% of DC plan participants (either in thrift/savings or profit-sharing plans) were offered immediate full vesting, and 64% of thrift/savings participants were offered a choice among full, graded, or class vesting within 5 years. However, about 50% of profit-sharing plan participants were offered graded vesting schedules of at least 10 years. According to these figures, the vesting schedules offered by nearly all DB plans had to be revised to comply with the new standards introduced by TRA ’86, while the majority of DC plans were already complying with the new schedules.

TABLE 2
Vesting Schedules Offered to Participants in 1986, by Type of Plan

		DC	
	DB	**Savings/ Thrift**	**Profit-Sharing**
Cliff vesting			
10 years	87	–	–
6–9 years	–	1	1
1–5 years	–	19	1
Graded vesting			
11–15 years	4	1	25
10 years	6	4	24
6–9 years	–	4	14
1–5 years	–	17	3
Rule of 45	3	–	–
Class vesting			
1–3 years	–	25	3
4–5 years	–	3	1
Immediate vesting	–	26	29

Source: Employee Benefits in Medium and Large Firms, 1986 survey (Bureau of Labor Statistics 1987). Figures in %.

TABLE 3
Vesting Schedules Offered to Participants in 1991, by Type of Plan

		DC	
	DB	**Savings/ Thrift**	**Profit-Sharing**
Cliff vesting			
10 years	16	–	–
5 years	75	31	19
Graded vesting			
>7 years	2	–	–
≤7 years	7	38	41
Immediate vesting	–	31	40

Source: Employee Benefits in Medium and Large Private Establishments, 1991 survey (Bureau of Labor Statistics 1993). Figures in %.

Table 3 summarizes the vesting schedule distribution in 1991, after TRA '86 full enactment, reported in the *Employee Benefits in Medium and Large Private Establishments, 1991* survey (Bureau of Labor Statistics 1993).

The figures in Tables 2 and 3 offer clear evidence that TRA '86 significantly shifted the distribution of vesting schedules offered to DB plan participants and—to a lesser extent—to DC plan participants. By 1991, 75% of DB plan participants were offered a 5-year cliff vesting schedule, and 7% were offered 7-year graded vesting. The 16% of DB plan participants that were still offered a 10-year cliff schedule in 1991 were in multiemployer plans, whose vesting schedules were not affected by TRA '86.

Overall, these figures suggest that by 1991 the vesting schedules offered to all DB single employer plan participants were amended to comply with the new standards introduced by TRA '86. By contrast, the vesting schedules offered to DC plan participants were already much more liberal than those prescribed by ERISA and therefore only partially affected by the new legislation. We use the above evidence to motivate and discuss the main identifying assumptions characterizing our empirical approach. First, we assume that all DB plan participants with 5–9 years of service (DB: 5–9) were affected by the less restrictive vesting schedules introduced by TRA '86. Second, we initially assume that DC plan participants with 5–9 years of service (DC: 5–9) were not affected by the new vesting standards. Under these assumptions, DB: 5–9 provides a natural treatment group and DC: 5–9 a potential control group. However, while our first assumption is strongly supported by the aforementioned evidence, the second assumption relies on less solid evidence. On the one hand, defining DC: 5–9 as the control group may arguably introduce classification error—that is, some of the employees assigned to DC: 5–9 were actually treated. On the other hand, DC: 5–9 may not be comparable to DB: 5–9. We discuss these issues at length in the next section, where we propose additional (and preferred) control groups.

III. IDENTIFICATION STRATEGY

The evidence discussed in the previous section suggests that the cut in the vesting period introduced by TRA '86 almost exclusively affected DB plan participants with 5–9 years of service (DB: 5–9), who were vested under TRA '86 but who would not have been vested under ERISA. Thus, the reform provides a plausibly exogenous source of variation that determines treatment assignment that can be exploited using a DD strategy. This commonly used quasi-experimental estimator allows us to measure the impact of vesting by comparing the difference in job mobility between respondents in the DB: 5–9 treated group and observationally comparable untreated respondents.

Our DD model is captured by the following equation:

$$Y_{it} = \beta_0 + \beta_1 \text{Treat}_i + \beta_2 \text{Post}_t + \beta_3 X_{it} + \gamma \text{Post}_t \times \text{Treat}_i + \varepsilon_{it} \quad (1)$$

where Y_{it} is the outcome of interest for individual i surveyed at time t, set to one if an employee experienced a voluntary job-to-job transition. Treat_i is a dummy variable set to one for employees with 5–9 years of tenure in a DB plan. It controls for unobservable differences among groups in the pre-reform period. Post_t is an indicator that equals one if the individual was surveyed in the post-reform period, and zero otherwise. It controls for time fixed effects common to the treatment and the control groups. X_{it} is a vector of demographic and job-related characteristics and time trends. ε_{it} is an individual-specific error term. The interaction term coefficient γ measures the impact of the reform on the treated group after covariate adjustment.

Several potential threats to internal validity arise when estimating the DD model just described. The key identifying assumption is that, in the absence of treatment, the difference in outcomes between treatment and comparison groups remains constant over time (parallel

TABLE 4
Treatment DB: 5−9 and Potential Control Groups

Group Type	Group Definition	Potential Problems
Treatment (DB: 5−9)	Employees in DB plans: tenure [5−9]	Measurement error
Control (No Pension: 5−9)	Employees with no plan: tenure [5−9]	Non comparability
Control (DC: 5−9)	Employees in DC plans: tenure [5−9]	Measurement & classification error
Control (DB: 1−4)	Employees in DB plans: tenure [1−4]	Measurement error
Control (DB: 10−13)	Employees in DB plans: tenure [10−13]	Measurement error

trends assumption). The latter rests on the choice of a "comparable" control group (Meyer 1995). Although we cannot directly test this assumption, our empirical strategy relies on examining the robustness of our results to the use of several alternative control groups characterized by varying degrees of comparability to the treatment. We also run three falsification tests that consider placebo treatment groups whose job mobility is not expected to be affected by the reform: DB: 5−9 employees in the pre-reform period, involuntary job movers, and vested workers. We finally address potential measurement/classification error issues by examining the robustness of our results to the use of an alternative treatment group, represented by all pension-covered workers with 5−9 years of tenure (Pension: 5−9). Table 4 presents possible treatment and control groups and quickly summarizes their potential drawbacks.

Potentially suitable control groups to the DB: 5−9 treatment include workers with 5−9 years of service not covered by a workplace pension (No Pension: 5−9) as well as similarly tenured workers enrolled in a DC plan (DC: 5−9). The former would qualify because vesting schedules only affect pension-covered workers. The latter would qualify because the majority of DC plans already complied with the new vesting rules introduced by TRA '86.

The use of No Pension: 5−9 and DC: 5−9 as control groups is, however, open to criticism. First, the distribution of observable and non-observable characteristics between pension-covered workers and their non-covered counterparts may differ significantly. There is widespread evidence that pension-covered workers are on average better educated, earn higher wages, and are intrinsically less mobile (see, among others, Gustman and Steinmeier 1993). Second, it is also well documented that DC and DB plan participants bear different risks and that the relative value of these plans depend on individual preferences of risk and plan attributes, demographic characteristics, and expected mobility (Bodie, Marcus, and Merton 1988). In particular, workers may opt into DC plans because of an intrinsically higher quit propensity. While the empirical evidence on the relevance of this selection issue is limited, Goda, Jones, and Manchester (2013) find evidence of a positive selection into DC plans based on unobservable mobility tendencies. Finally, as shown in Section II, 50% of the workers participating in DC plans of the profit-sharing type were offered graded vesting schedules of at least 10 years. Assigning those workers to the control group would give rise to a classification problem, as discussed below.

The existence of common time-specific shocks across treatment and control groups is more likely to hold when both groups share similar observable and unobservable characteristics (see, for instance, Meyer 1995, for a detailed discussion). If workers in DB plans were intrinsically less mobile than workers with similar characteristics but different pension arrangements—either DC or no plan—they would likely exhibit smaller responses to cyclical or secular changes in the labor market. In this case, the estimated job mobility response to shorter vesting by our DD model could suffer from a sizable selection bias.

To address this issue, we consider additional control groups that exploit the sources of discontinuity characterizing our natural experiment. First, workers covered by DB plans were randomly assigned to different vesting treatments based on years of service (job tenure)—only DB plan participants with 5−9 years of service (DB: 5−9) were assigned a vesting status. Second, the assignment was based on a temporal forcing variable—DB covered workers within the cutoffs defined by the reform were given different vesting treatments at adjacent points in time (before/after January 1, 1989). Under this *quasi-discontinuity* design, DB covered workers with years of service just below and/or just above our treatment cutoffs possibly constitute additional relevant control groups.

In the spirit of a regression discontinuity approach, we consider two potentially suitable control groups at or near the treatment threshold. The first includes all DB plan participants with 1–4 years of tenure (DB: 1–4) since their years of service randomly place them just below the lower cutoff to receive treatment. The second group includes already vested DB plan participants. We consider DB plan participants with 10–13 years of tenure (DB: 10–13) since they lie just above the upper cutoff of the forcing area.[7] Comparing individuals under the same plan type (DB) allows us to disentangle the effect of vesting by controlling for other plan characteristics known to affect mobility, such as pension wealth accrual inherent to plan type. It also allows us to account for potential selection effects, wherein employees with different underlying unobserved mobility tendencies select across different plan types (Goda, Jones, and Manchester 2013). Comparing estimates for our two control groups of DB workers further allows us to assess the robustness of our results to a possible secular relationship between job mobility outcomes and our forcing variables: time and tenure.

Measurement error in self-reported pension data and classification error in treatment/control group assignment are further potential sources of bias that may threaten the internal validity of our identification strategy. There is a growing concern in the recent pension literature that self-reported pension data may be subject to widespread measurement error, particularly related to plan type identification by pension-covered workers (Gustman and Steinmeier 2005; Gustman, Steinmeier, and Tabatabai 2009). Several studies matching SIPP data to W-2 IRS tax records provide evidence of substantial misreporting of DC plan participation (Dushi and Iams 2010; Dushi, Iams, and Lichtenstein 2011; Turner, Muller, and Verma 2003). While no evidence based on SIPP data is available on the relevance of measurement error for DB plan participants, evidence from other data sources indicates that measurement error is less relevant for workers participating in DB plans (Mitchell 1988; Sunden 1999) and for male workers (Gustman and Steinmeier 2005). Taken together, this evidence provides further support for our choice of DB: 1–4 and DB: 10–13 as preferred control groups. However, it may still be the case that the pools of DB plan participants representing our treatment and preferred control groups include employees actually enrolled in DC plans. We address this issue below.

Besides measurement error, our identification strategy may also suffer from classification error occurring when a worker is assigned to the wrong treatment/control group. For example, an employee in a DC plan with a pre-reform 10-year cliff vesting schedule would—under the identifying assumptions outlined in Section II—be erroneously assigned to the control group, while an employee in a DB plan with pre-reform 5-year cliff vesting schedule would be erroneously assigned to the treatment group. Unfortunately, the SIPP public use files do not include matched employer-employee records that could help to address the aforementioned issues. Focusing our empirical analysis on a DB treated group versus more comparable DB control groups also helps in minimizing potential classification error issues.

Overall, measurement/classification error issues should not be a cause for concern in our empirical analysis. Furthermore, if measurement/classification errors were randomly distributed, they would likely attenuate the estimated impact of the reform,[8] which would therefore still be informative as a lower estimation bound. Nonetheless, to further investigate the sensitivity of our results to measurement/classification errors, we use an alternative treatment group including all pension-covered workers—either of the DB or the DC type—with 5–9 years of service (Pension: 5–9). In this case, potential control groups—reported in Table 5—include non-covered workers with 5–9 years of service (No Pension: 5–9), all pension-covered workers with 1 to 4 of service (Pension: 1–4), and all pension-covered workers with 10–13 years of service (Pension: 10–13).

These alternative treatment/control groups are not expected to suffer from plan type measurement error. This should lead to a reduction of the attenuation bias. By contrast, the incorrect assignment of a large number of untreated DC workers to the treatment group would increase the attenuation bias proceeding from classification

7. The choice of these particular thresholds around the cutoffs in the definition of our alternative control groups is aimed at reaching a balance between internal validity and sample size (see Murname and Willett, 2011, for a detailed discussion). However, the use of narrower—2- and 3-year—bandwidths brings qualitatively similar results (available from the authors upon request).

8. As surveyed in Bound, Brown, and Mathiowetz (2001), classification error (measurement error in a binary variable) usually leads to bias toward zero, unless classification error is so prevalent that the sign of the estimates actually changes. In the difference-in-differences context, the classification bias affects both the coefficient of the treatment group indicator and the interaction coefficient of interest, implying that we may underestimate the true impact of the reform.

TABLE 5
Treatment Pension: 5–9 and Potential Control Groups

Group type	Group definition	Potential problems
Treatment (Pension: 5–9)	Employees in DB | DC plans: tenure [5–9]	Classification error
Control (No Pension: 5–9)	Employees with no plan: tenure [5–9]	Non comparability
Control (Pension: 1–4)	Employees in DB | DC plans: tenure [1–4]	Classification error
Control (Pension: 9–13)	Employees in DB | DC plans: tenure [10–13]	Classification error

error. Which of these two counter-effects dominate is an empirical question.

In addition to standard DD estimates, we provide further robustness checks through difference-in-differences matching (DDM). DDM combines traditional matching methods with DD.[9] This estimator offers more flexibility than a traditional DD estimator as it does not 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, Ichimura, and Todd 1997; Heckman et al. 1998). In addition, Smith and Todd (2005) show that the DDM estimator performs the best among non-experimental matching-based estimators.

Given the nature of our data, we implement the DDM estimator on repeated cross-sections as two-way propensity score matching by pairing each worker in the treatment group with members of the control group that exhibit similar observables in both 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:

$$(2)\qquad \hat{\gamma}_{\mathrm{DDM}} = \frac{1}{n_{1A}} \sum_{i \in I_{1A} \cap S_{\mathrm{P}}} \left\{ Y_{1i}^{A} - \hat{Y}_{0i}^{A} \right\} - \frac{1}{n_{1B}} \sum_{i \in I_{1B} \cap S_{\mathrm{P}}} \left\{ Y_{0i}^{B} - \hat{Y}_{0i}^{B} \right\}$$

where I_{1B}, I_{1A} denote the sets of treated pension plan participants in the periods preceding and following the implementation of TRA '86, and S_{P} is the region of common support. n_{1B} and n_{1A} capture the number of treated pension plan participants for whom 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 treated participant experienced a voluntary job transition in the pre(post)-reform period. $\hat{Y}_{0i}^{B}$ and $\hat{Y}_{0i}^{A}$ denote the corresponding counterfactual outcomes, constructed as the weighted average outcomes of seemingly comparable non-treated workers. It can be expressed as:

$$(3)\qquad \hat{Y}_{0i}^{t} = \sum_{j \varepsilon I_{0t} \cap S_{P}} w_{ij} Y_{0j}^{t}, \qquad t = \{A, B\},$$

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 weight assigned to each control j in the estimation of the counterfactual outcome for 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]

IV. DATA

A. *The Sample*

This analysis uses data from the Survey of Income and Program Participation (SIPP). The SIPP data are a collection of independent nationally representative longitudinal surveys of U.S. households. Each survey year is a short rotating panel made up of 7–12 waves of data—collected every 4 months[11]—covering a time span ranging from 2.5 years to 4 years for approximately 14,000–36,700 households. Each panel is comprised of *core* and *topical* modules. The former are common to each wave, while the latter provide in-depth information on particular topics that are usually wave-specific. The topical module on pension coverage asks pension participants whether their pension benefits are determined

9. See, among others, Rosenbaum and Rubin (1983), Heckman, Ichimura, and Todd (1997), Smith and Todd (2005), Blundell and Costas Dias (2009) for detailed discussions on matching methods and difference-in-differences matching.

10. All our estimates are obtained using the psmatch2 Stata module of Leuven and Sianesi (2003).

11. SIPP respondents are grouped into four mutually exclusive *rotation groups* for interviewing purposes. Each rotation group is interviewed in a different month, in successive 4-month periods.

either by earnings and years of service or by the amount contributed to the plan. We use this information to assign each worker a (mutually exclusive) pension participation status: participating in a DB (DC) plan, or not participating in any employer-sponsored pension arrangement (See Appendix A for a detailed description of our categorization of respondents' pension plan status assignment). As individuals' vesting schedule data are not available in the SIPP public use files,[12] following our discussion in Section II, we assume that all DB: 5–9 employees were not vested before TRA '86, unlike all DC: 5–9.

We use data from the 1984, 1986, 1990, 1992, and 1996 panels.[13] Our choice of panel years is guided by the availability of relevant pension information and the ability to measure labor mobility over the longest possible time frame common to all survey years. By pooling these survey years, we construct a unique synthetic panel, which allows us to fully exploit the quasi-experimental design offered by TRA '86 using DD methods.

Our sample is restricted to full-time male employees working in private-sector—non-agricultural, non-construction—firms in the last month of the reference period, who report hourly wages between \$3 and \$55, expressed in constant (82–84) dollars. We exclude agricultural and construction workers because of the idiosyncratic nature of job turnover in these sectors. These workers are unique in both the highly seasonal nature of their work and the tendency of their pension plans to be provided by unions in the form of multiemployer plans (Weinstein and Wiatrowski 1999). As a result, they usually exhibit high turnover rates with little discontinuity in pension coverage.[14]

We exclude public sector workers because their pension plans usually offer more generous portability provisions and because they also exhibit idiosyncratic turnover patterns (Foster 1994). 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 55.

Our main sample uses the 1984 and 1986 panels as *pre-reform* data and the 1990, 1992, and 1996 panels as *post-reform* data (sample 1). We explore the sensitivity of our results to the choice of survey years by considering a second sample that excludes data from the 1996 panel (sample 2). Excluding the 1996 panel—and therefore limiting the post-reform period to a time frame spanning 24–48 months after TRA '86 enactment—allows us to test whether our results are driven by contemporaneous reforms that could have affected our treatment and control groups differentially.[15]

B. Measuring Job Mobility

We exploit the longitudinal structure of the core modules to identify job transitions for each individual respondent. Our period of observation starts at the wave in which the relevant pension coverage information is first collected. The longest observation window common to all survey years satisfying this constraint spans four consecutive waves (see Appendix B for a detailed description of the construction of our measure of job mobility). 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 a move voluntary when a worker switches jobs without experiencing any unemployment spell over the four consecutive waves of observation. All involuntary movers are dropped from the analysis.

12. Likewise, participants were not asked whether their plan required a waiting period before becoming eligible to participate in the plan. As a consequence, tenure in the plan for participating workers is assumed to be equal to the years of tenure with the current employer.

13. SIPP 1984 spans 32 months from October 1983 to July 1986; SIPP 1986 spans 28 months from January 1986 to April 1988, SIPP 1990 spans 32 months from February 1990 to September 1992, SIPP 1992 spans 40 months from February 1992 to April 1995, and SIPP 1996 spans 48 months from April 1996 to March 2000.

14. As discussed in Section II, TRA '86 vesting schedules were not enforced in multiemployer plans before January 1, 1999. Under the assumption that DB plan participants in the agricultural and construction sectors are predominantly enrolled in multiemployer plans, those participants with 5 to 9 years of tenure provide another potential comparison group. We thank an anonymous referee for bringing this to our attention. Unfortunately, the combined sample size of voluntary movers from these two sectors is too small to further exploit this group. While lacking statistical power, these results—available from the authors upon request—corroborate our main findings.

15. In response to growing concern over the potential "job lock" suffered by workers with employer-provided health insurance, Congress enacted the Health Insurance Portability and Accountability Act of 1996 ("HIPAA"). Effective on July 1, 1997, HIPAA attempted to increase health insurance portability by limiting preexisting condition exclusions, prohibiting discrimination against individuals based on health status, and guaranteeing renewability and availability of certain types of insurance plans.

TABLE 6
Sample Means for Treatment DB: 5–9 and Control Groups (Pre - Post-Reform)

	Treatment		Control Groups		
	DB: 5–9	**DB: 1–4**	**DB: 10–13**	**DC: 5–9**	**No Pens: 5–9**
Pre-Reform					
Job mobility					
Voluntary Movers	0.05 [0.01]	0.16* [0.02]	0.04 [0.01]	0.06 [0.01]	0.08‡ [0.01]
Demographics					
Black	0.11 [0.02]	0.13 [0.02]	0.10 [0.02]	0.10 [0.02]	0.10 [0.02]
Education (years)	14.00 [0.12]	14.30‡ [0.12]	13.51† [0.16]	14.00 [0.14]	13.12* [0.14]
Experience	15.15 [0.35]	15.14 [0.40]	18.58* [0.40]	14.83 [0.41]	15.96 [0.45]
Non-single	0.75 [0.02]	0.78 [0.02]	0.79 [0.02]	0.77 [0.02]	0.69‡ [0.02]
Spouse empl.	0.32 [0.02]	0.37 [0.02]	0.33 [0.02]	0.29 [0.02]	0.21* [0.02]
Children < 18	0.60 [0.02]	0.54‡ [0.02]	0.65 [0.02]	0.59 [0.03]	0.50* [0.03]
Family size	3.28 [0.07]	3.25 [0.07]	3.35 [0.07]	3.19 [0.08]	3.18 [0.08]
Housing tenure	0.73 [0.02]	0.70 [0.02]	0.84* [0.02]	0.75 [0.02]	0.64* [0.03]
Family health pbl.	0.03 [0.01]	0.06 [0.01]	0.05 [0.01]	0.04 [0.01]	0.06‡ [0.01]
Job related					
Hourly wage	11.33 [0.26]	10.90 [0.24]	11.82 [0.25]	11.35 [0.32]	9.45* [0.31]
Job tenure	6.90 [0.06]	2.48* [0.05]	11.29* [0.06]	6.76 [0.07]	6.64* [0.07]
Firms < 100	0.69 [0.02]	0.69 [0.02]	0.70 [0.02]	0.69 [0.02]	0.36* [0.02]
Union coverage	0.29 [0.02]	0.25 [0.02]	0.44* [0.03]	0.17* [0.02]	0.10* [0.01]
Empl. health insurance (HI)	0.95 [0.01]	0.94 [0.01]	0.95 [0.01]	0.93 [0.01]	0.73* [0.02]
Spouse health insurance (SHI)	0.23 [0.02]	0.27 [0.02]	0.21 [0.02]	0.20 [0.02]	0.23 [0.02]
Local market					
State unemp. rate	6.64 [0.08]	6.53 [0.08]	6.70 [0.09]	6.55 [0.09]	6.47 [0.09]
Obs	579	547	449	387	423
Post-Reform					
Job mobility					
Voluntary movers	0.07 [0.01]	0.11* [0.01]	0.03* [0.01]	0.08 [0.01]	0.12* [0.01]
Demographics					
Black	0.12 [0.01]	0.14 [0.01]	0.11 [0.01]	0.10‡ [0.01]	0.15‡ [0.01]
Education (years)	14.30 [0.08]	14.09‡ [0.09]	14.35 [0.09]	14.11‡ [0.09]	12.97* [0.09]
Experience	16.84 [0.24]	17.00 [0.27]	18.57* [0.26]	16.24‡ [0.24]	17.50‡ [0.27]
Non-single	0.76 [0.01]	0.69* [0.02]	0.74 [0.02]	0.71† [0.01]	0.64* [0.02]
Spouse empl.	0.35 [0.01]	0.32 [0.01]	0.35 [0.02]	0.34 [0.01]	0.22* [0.01]
Children < 18	0.54 [0.01]	0.50‡ [0.02]	0.59† [0.02]	0.55 [0.02]	0.48* [0.02]
Family size	3.10 [0.04]	3.02 [0.05]	3.22 [0.06]	3.05 [0.05]	3.04 [0.05]
Housing tenure	0.76 [0.01]	0.68* [0.02]	0.84* [0.01]	0.73 [0.01]	0.64* [0.02]
Family health pbl.	0.05 [0.01]	0.05 [0.01]	0.05 [0.01]	0.05 [0.01]	0.06 [0.01]
Job related					
Hourly wage	11.36 [0.17]	10.47* [0.20]	12.35* [0.24]	11.07 [0.17]	8.34* [0.15]
Job tenure	6.82 [0.04]	2.56* [0.04]	11.44* [0.04]	6.68† [0.04]	6.50* [0.04]
Firms < 100	0.47 [0.01]	0.56* [0.02]	0.54* [0.02]	0.46 [0.02]	0.27* [0.01]
Union coverage	0.29 [0.01]	0.25‡ [0.01]	0.30 [0.02]	0.12* [0.01]	0.09* [0.01]
Empl. health insurance (HI)	0.92 [0.01]	0.88* [0.01]	0.93 [0.01]	0.89† [0.01]	0.69* [0.01]
Spouse health insurance (SHI)	0.22 [0.01]	0.20 [0.01]	0.19‡ [0.01]	0.22 [0.01]	0.19‡ [0.01]
Local market					
State unemp. rate	6.27 [0.05]	5.93* [0.05]	6.19 [0.06]	6.03* [0.05]	6.36 [0.05]
Obs	1367	1217	831	1195	1172

Notes: All means weighted using SIPP person weights. Standard errors are given in brackets. *, † and ‡ indicate significance at 1%, 5% and 10% levels, respectively, for the test of equality of means with the treatment group. Pre-Reform period includes the 1984 and 1986 SIPP surveys. Post-Reform period includes the 1990, 1992 and 1996 SIPP surveys. Hourly wages expressed in real terms using Monthly CPI-U BLS, Base = 82–84.

V. EMPIRICAL RESULTS

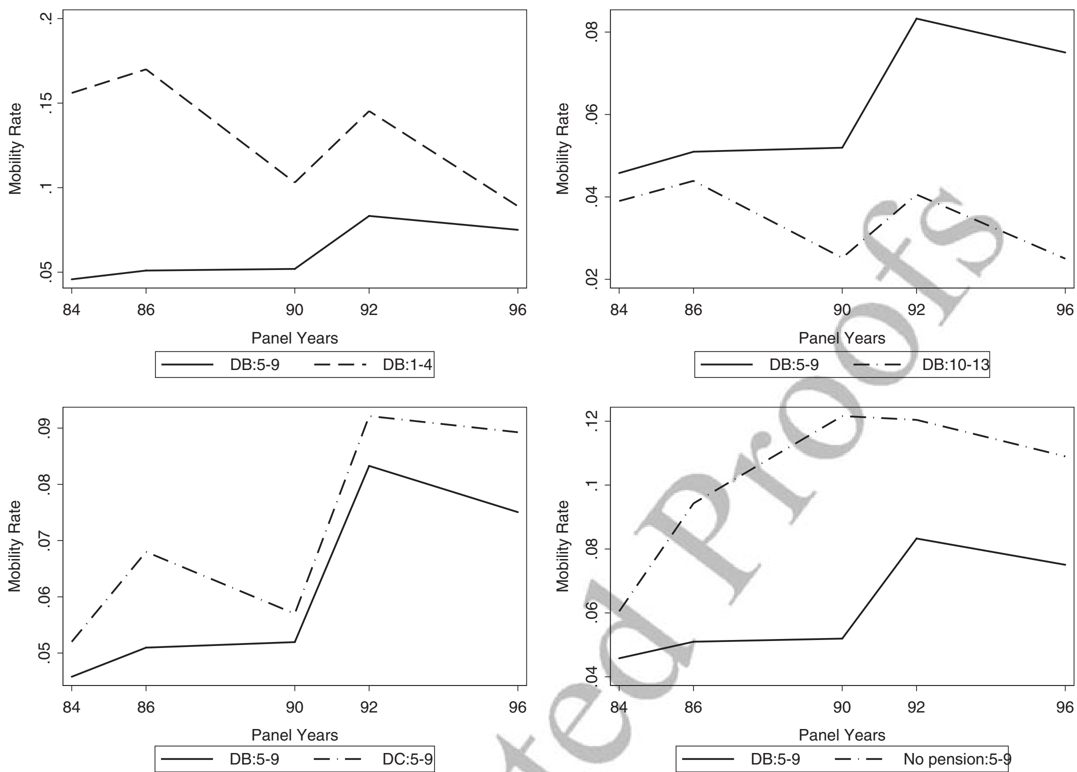
A. *Descriptive Statistics*

Table 6 presents descriptive statistics of relevant characteristics—before (upper panel) and after the reform (lower panel)—along with an indicator for the test of equality of means between the treatment and each control group. All means are weighted using SIPP person weights.[16]

Depending on the group, voluntary job mobility ranges between 4 and 16% in the pre-reform period and between 3 and 12% in the post-reform period. While not strictly comparable to other

16. Weights are used to account for SIPP oversampling of the low-income population.

FIGURE 2
Voluntary Mobility by Panel Years



studies due to differences in sample selection criteria,[17] our figures are largely in line with previous job mobility studies exploiting various SIPP panel years (Gustman and Steinmeier 1993; Hamersma and Kim 2009; Haverstick et al. 2010).

The validation of our identification strategy relies on the existence of a common time trend in voluntary job mobility between our treatment and comparison groups. Figure 2 reveals broadly comparable pre-reform trends for pension-covered workers, albeit not strictly parallel, between the treatment group and our preferred comparison groups (DB: 1–4 and DB: 10–13) while exhibiting more significant divergences in the post-reform period. By contrast, the DC: 5–9 and the No Pension: 5–9 control groups exhibit divergent trends in the pre-reform period. In addition, we discuss placebo test results corroborating this visual evidence, in subsection C.

Table 6 also reveals differences of varying magnitudes in other relevant dimensions between all our treatment/control group pairings. Consistent with our expectations, No Pension: 5–9 exhibits marked differences from the treatment group and the other control groups of pension-covered workers (DB: 1–4, DB: 10–13, and DC: 5–9) in almost all dimensions. Non-pension-covered workers earn, on average, lower wages than their pension-covered counterparts, have lower educational attainment, are less likely to be married, to own their own home, to be covered by health insurance, to report union membership, and to be working in large firms. These findings corroborate Gustman and Steinmeier (1993). Given these significant differences, we exclude this group from the remainder of our analysis.[18]

17. We use prime-age male workers in specific employer-sponsored pension arrangements and job tenure brackets.

18. These results are available upon request from the authors.

TABLE 7

Raw Before, After, and Difference-in-Differences Estimates (in percent)

	Treatment		Controls	
	DB: 5–9	**DB: 1–4**	**DB: 10–13**	**DC: 5–9**
Sample 1				
Before	4.855 (0.009)	16.397 (0.017)	4.194 (0.009)	5.903 (0.012)
After	7.175 (0.007)	11.013 (0.010)	3.074 (0.006)	7.984 (0.008)
Diff	2.320‡ (0.012)	– 5.384* (0.020)	– 1.120* (0.011)	2.081* (0.015)
DiD		7.704* (0.023)	3.440† (0.017)	0.239† (0.019)
Obs:	1946	1764	1280	1582
Sample 2				
Before	4.855 (0.009)	16.397 (0.017)	4.194 (0.009)	5.903 (0.012)
After	7.054 (0.009)	12.312 (0.015)	3.327 (0.008)	7.568 (0.010)
Diff	2.199‡ (0.013)	– 4.085‡ (0.022)	– 0.868‡ (0.013)	1.665‡ (0.016)
DiD		6.284† (0.026)	3.067‡ (0.018)	0.534‡ (0.020)
Obs:	1491	1206	966	1133

	Pension: 5–9	**Pension: 1–4**	**Pension: 10–13**
Sample 1			
Before	5.282 (0.007)	14.900 (0.013)	5.394 (0.009)
After	7.548 (0.006)	11.591 (0.007)	3.980 (0.006)
Diff	2.266† (0.009)	– 3.310† (0.015)	– 1.413† (0.011)
DiD		5.575* (0.017)	3.679† (0.014)
Obs:	3528	3431	2077
Sample 2			
Before	5.282 (0.007)	14.900 (0.013)	5.394 (0.009)
After	7.284 (0.007)	12.638 (0.010)	4.382 (0.009)
Diff	2.002† (0.010)	– 2.262* (0.016)	– 1.011* (0.013)
DiD		4.264† (0.019)	3.013‡ (0.016)
Obs:	2624	2198	1511

Notes: *, † and ‡ indicate significance at 1%, 5% and 10% levels, respectively. All means weighted using SIPP person weights. Standard errors are given in parentheses. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys. Sample 2 excludes data from the 1996 SIPP surveys.

By contrast, pre-reform samples of pension-covered workers are on average largely comparable with the treated units. Significant differences are limited to job tenure and education (DB: 1–4, DB: 10–13), children (DB: 1–4), years of experience, housing tenure (DB: 10–13), and union coverage (DC: 5–9, DB 10–13).[19] Interestingly, more pronounced dissimilarities appear in the post-reform samples, in particular when DB: 1–4 and DC 5–9 are used as comparison groups. This could indicate a compositional shift over time between the *before* and *after* samples. As a result, any significant differences in raw job mobility over time between the treatment and control groups ought to be interpreted with caution. These differences could merely reflect the differential effects of shocks unrelated to the new vesting schedules on individuals with different observable characteristics.

Table 7 presents raw (voluntary) mobility rates before and after the implementation of TRA ’86, the implied *after-before* differences, and associated DD estimates,[20] separately for each sample and for each treatment-control pairing.

The upper panel of Table 7 shows that pre-reform voluntary mobility differs significantly between the treatment and DB: 1–4, while exhibiting rates of comparable magnitude with DB: 10–13 and DC: 5–9. This observation corroborates earlier findings that job tenure and/or pension plan participation are both negatively associated with labor mobility (Haverstick et al. 2010). DB: 5–9 and DC: 5–9 exhibit lower job mobility (4.86% and 5.90%) than workers with similar job tenure not covered by a pension

19. Note, however, that some of these differences are simply due to the idiosyncratic nature of each group. For instance, differences in job tenure and/or other life-cycle-related outcomes between DB: 1–4, DB: 10–13, and the treatment group are expected by construction.

20. The reported raw difference-in-differences estimates are equivalent to those that would have been obtained estimating Equation (1) without controls as a linear probability model.

(8%, see Table 6), whereas DB: 1–4 exhibits the highest mobility.

Interestingly, in the post-reform period our treatment group (DB: 5–9) experiences a significant increase in job mobility of about 2.32 percentage points. This observation contrasts with the decline in mobility of DB: 1–4 and DB: 10–13.[21] As a result, we find positive and significant raw DD estimates when both DB: 1–4 (7.70%) and DB: 10–13 (3.44%) are used as control groups. This is in stark contrast to the finding of a significant positive effect of negligible magnitude of DC: 5–9.

Similar patterns emerge from the lower panel of Table 7, where our alternative treatment group (Pension: 5–9) is paired with the relevant control groups.

Overall, the impact of the reform emerging from these raw estimates seems to be qualitatively consistent across our DB control groups, and also robust to the use of an alternative treatment group (Pension: 5–9) as well as to the exclusion of the SIPP 1996 sample. These preliminary estimates suggest that the changes in vesting rules introduced by TRA '86 may have positively impacted the job mobility of the treated group (DB: 5–9).

B. *Difference-in-Differences Estimates*

To some extent, our descriptive statistics reveal that the treatment and control groups differ in demographic and job-related characteristics. As a result, our preliminary results may simply (or partly) reflect underlying differences between the treatment and the control groups rather than the treatment effect. Hence, controlling for demographic and job-related characteristics is important if the composition of the treatment and the control groups changes over time and if some of these characteristics are correlated with the outcome of interest.

To account for these differences and to assess the robustness of our results, we consider three model specifications of Equation (1). Our choice of control variables is guided by numerous theoretical perspectives (e.g., human-capital theory, search theory, matching theory, and labor market segmentation theory) and by the related empirical literature focusing on the determinants of job turnovers (see Sousa-Poza and Henneberger 2004, for a review).

Our baseline model (SP1 in Table 8) controls for personal and family-related characteristics as proxies for mobility costs including race, years of schooling, potential work experience, marital status, spousal employment status, spousal health insurance, the number of children aged less than 18, family size, house tenure, race, regional variables (SMSA as well as regions), and a proxy measure family health problems.[22] Employment-specific factors also affect mobility decisions. To account for these factors, we extend our baseline model (SP2) to include job- and firm-specific characteristics—hourly wage, job tenure, firm size, union status, employer-sponsored health insurance, and its interaction with mobility costs proxies such as family health and spousal health insurance. We also include a set of dummies to control for industry- and occupation-specific turnover rates. Finally, our third specification (SP3) includes a state unemployment rate variable and its interaction with the post-reform dummy to control for potential idiosyncratic responses of the treatment and control groups to contemporaneous changes in economic conditions—business cycle fluctuations and other unknown shocks.[23] All model specifications include a set of panel year dummies and a set of birth year dummies to control for cohort specific shocks.[24]

Table 8 reports the marginal effects of the probit estimates with robust standard

21. Note that DC: 5–9 also experiences a significant increase in mobility of 2.08 percentage points. This may indicate the existence of classification and measurement error issues raised in Section III.

22. In short, we capture health problems in the family by an indicator variable measuring whether (1) respondents indicate that one or more children under 18 in the household suffer a long-lasting physical or mental health condition, and/or (2) the spouse reports a health condition limiting her/his ability to work, and/or (3) the spouse rates her/his health in general to be "fair" or "poor." This measure is constructed following Berger, Black, and Scott (2004). Unlike Berger, Black, and Scott (2004), our measure does not include spousal functional limitations as this information was not collected in pre-1990 SIPP data. See Berger, Black, and Scott (2004) for further details.

23. Auerbach and Slemrod (1997) present convincing arguments for an event study of TRA'86. First, the economy was relatively stable shortly before, during, and after its passage, so that a comparison of the pre-TRA '86 period to the post-TRA '86 period would not be affected by peaks and troughs in a business cycle. Second, and for the same reason, it would also be difficult to argue that the passage of TRA '86 was occasioned by particular macroeconomic circumstances, whether favorable or unfavorable, that would complicate the identification of causal direction.

24. We report the full set of parameter estimates for our most comprehensive model specification (SP3) in Tables B1 and B2, Appendix B. All other results are available from the authors upon request.

TABLE 8
DD Estimates: Voluntary Job Mobility (in percent)

	Control Groups								
Treatment	**DB: 1–4**			**DB: 10–13**			**DC: 5–9**		
DB: 5–9	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Sample 1									
Post × Treat	7.243*	6.727*	6.419*	3.770†	3.709†	3.657†	0.671	0.426	0.454
	(0.023)	(0.023)	(0.023)	(0.019)	(0.019)	(0.018)	(0.021)	(0.021)	(0.021)
Obs		3710			3226			3528	
Sample 2									
Post × Treat	5.915†	5.173†	4.954†	3.311‡	3.100	3.036	0.983	0.646	0.467
	(0.026)	(0.025)	(0.025)	(0.020)	(0.020)	(0.020)	(0.021)	(0.021)	(0.021)
Obs		2697			2457			2624	

	Control Groups					
Treatment	**Pension: 1–4**			**Pension: 10–13**		
Pension: 5–9	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Sample 1						
Post × Treat	5.506*	5.043*	4.779*	4.156*	4.232*	4.232*
	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
Obs		6959			5605	
Sample 2						
Post × Treat	4.269†	3.352‡	3.133‡	3.500†	3.502†	3.430†
	(0.019)	(0.019)	(0.018)	(0.017)	(0.017)	(0.017)
Obs		4822			4135	

Notes: *, † and ‡ indicate significance at 1%, 5% and 10% levels, respectively. Robust standard errors are in parentheses. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys. Sample 2 excludes data from the 1996 SIPP surveys. All model specifications include a set of panel year dummies and a set of birth year dummies. Full estimation results for SP3 can be found in Appendix 8.

errors of γ —the coefficient associated with Post × Treat —capturing the average impact of the reform on the treated group.[25] DB: 5–9 estimates—reported in the upper panel—constitute the key empirical findings of this study. Pension: 5–9 results provide additional sensitivity checks.

DB: 5–9 results reveal the existence of a statistically and economically significant impact of the reform on the average job mobility of the treated group. Estimated changes in mobility range between 3.66 and 7.25 percentage points when DB: 10–13 and DB: 1–4 are used as control groups, respectively. The more conservative estimates using DB: 10–13 as control group are likely more reliable. For one thing, this control group exhibits pre-reform mobility rates that are considerably closer to those of the treatment group. Furthermore, workers in this control group are already vested and, by definition, their quit behavior is not affected—directly or indirectly (i.e., through selection or expectations)—by the vesting reform.

The robustness of the estimated treatment effect to specifications including controls for job- and firm-specific characteristics (SP2) and business cycle fluctuations (SP3) is consistent with the validity of the parallel trends assumption. Moreover, the insignificance of the Treat and Post dummies estimated coefficients—reported in Table B1, Appendix B—for our preferred control group (DB: 10–13) lends further support to the parallel trends assumption.

Interestingly, we find a statistically insignificant impact of negligible magnitude, ranging between 0.45 and 0.67 percentage points, when DC: 5–9 is used as the control group. This finding supports our view that DC: 5–9 may not provide an adequate control group. As outlined in Section III, the use of DC: 5–9 to identify the effect of the reform may suffer from both an attenuation bias arising from classification error—incorrectly assuming that all DC: 5–9 were already complying with the new rules

25. Puhani (2012) shows that in a nonlinear (probit) difference-in-differences model, the parameter of interest is not a simple cross difference (Ai and Norton, 2003) but a difference between cross differences simplifying to the incremental effect of the interaction term coefficient. In this case, the treatment effect on the treated has the same sign as the interaction effect. We compute the treatment effect on the treated—at the sample means of covariates—using the margins Stata command.

before the reform—and a selection bias arising from unobservables—that is, a higher quit propensity among DC-covered workers that may have induced a differential response to common macroeconomic shocks (Goda, Jones, and Manchester 2013).

Comparing the results for samples 1 and 2 suggests that our findings are robust to the choice of survey years. The impact of the reform, however, appears slightly smaller in magnitude and statistical significance once SIPP 1996 data is excluded from our sample. Despite preserving its economic significance, the impact of the reform using our preferred control group (DB: 10–13) is estimated less precisely. This precision loss, arising from a reduction in sample size and thus relatively less variation in the outcome of interest (voluntary job mobility), may hamper our ability to detect a reform effect. Nonetheless, the impact of the reform with DB: 10–13 as control group is still significant at the 10% level if we specify our *a priori* expectations (that the reform had a positive effect) as the alternative of a one-sided test.

On the basis of the above discussion, we provide supplementary evidence to further establish whether our findings are robust and rest on secure assumptions. We use Pension: 5–9 as an alternative treatment group to further examine the sensitivity of our results to alternative treatment/control group pairings. In this exercise—that also allows us to address the measurement error issue discussed in Section III—we use Pension: 1–4 and Pension: 10–13 as alternative control groups. However, as discussed in Section III, Pension: 5–9 likely assigns a treatment status to a number of untreated units since we believe that most DC: 5–9 were not affected by the reform. Our results—reported in the lower panel of Table 8—largely corroborate our earlier findings. We find a statistically significant impact of comparable magnitude when both Pension: 1–4 and Pension: 10–13 are used as control groups. While the direction and the extent of the possible contamination bias are difficult to gauge, these results provide further evidence of a positive and significant effect of the vesting reform, mostly arising from the change in mobility of DB: 5–9. Moreover, they suggest that the use of DB: 5–9 as treatment group may yield a lower estimation bound.

Taken together, our findings are robust and consistent with our characterizations of the strengths and weaknesses of each treatment/control group pairing, suggesting that the reform of the vesting period had a significant impact on the voluntary job mobility of the treatment group. In particular, the DD estimates reported in Table 8 reveal that, in relative terms, the impact of the reform was important, varying between 75% and 130% (when using DB: 10–13 and DB: 1–4 as control group, respectively) of the treatment group (DB: 5–9) baseline pre-reform mobility rate (reported in Table 7).

While we cannot rule out a possible violation of the parallel trend assumption, we provide three falsification tests relying on placebo treatment groups whose voluntary job mobility was not affected by the reform to further examine this issue.

C. *Placebo Results*

Our first placebo test estimates Equation (1) on the pre-reform sample by pretending that the reform was introduced between 1984 and 1986. Results reported in Table 9 do not detect any significant placebo effect, lending further support to our parallel trend assumption.

The second falsification test replicates our main results on a sample of *involuntary* movers. If the observed change in mobility of DB: 5–9 genuinely reflects a behavioral response to the change in vesting rules, we posit that only voluntary movers would be affected by the reform. The results of this exercise are presented in the upper panel of Table 10. Reported DD estimates are statistically insignificant and of negligible magnitude for all treatment/control group pairings across all specifications and samples considered. This provides tangible support for our initial conjecture.

The third experiment uses DB: 10–13 as placebo treatment. As these workers were already vested under the old rules, their voluntary job mobility should not be affected by a sharp cut in the vesting schedule. Consistent with this expectation, our estimates, reported in the lower panel of Table 11, reveal an insignificant placebo response to the reform. Overall, the results from these three experiments provide further empirical support for the parallel trend assumption and lend further credibility to our estimates on voluntary movers.

D. *Matching Results*

As a final robustness check, we estimate Equation (2) using DDM. Following the methodology outlined in Section III, we use several matching algorithms to pair each treated individual—in the region of common

TABLE 9
Placebo Test on Pre-Reform Voluntary Movers (in percent)

	Control Groups								
Treatment	**DB: 1–4**			**DB: 10–13**			**DC: 5–9**		
DB: 5–9	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Sample 1									
Post × Treat	– 0.864	– 1.073	– 1.209	0.019	0.025	0.048	– 0.982	– 0.749	– 0.932
	(0.038)	(0.037)	(0.037)	(0.040)	(0.039)	(0.039)	(0.028)	(0.027)	(0.027)
Obs		1126			1028			966	

				Control Groups		
Treatment	**Pension: 1–4**			**Pension: 10–13**		
Pension: 5–9	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Sample 1						
Post × Treat	– 0.540	– 0.478	– 0.376	– 0.871	– 1.065	– 0.947
	(0.028)	(0.028)	(0.028)	(0.032)	(0.032)	(0.032)
Obs		1883			1658	

Notes: *, † and ‡ indicate significance at 1%, 5% and 10% levels, respectively. Robust standard errors are given in parentheses. SIPP 1984: pre-reform sample and SIPP 86: post-reform sample.

TABLE 10
Difference-in-Differences Estimates on Involuntary Movers (in percent)

	Control Groups								
Teatment	**DB: 1–4**			**DB: 10–13**			**DC: 5–9**		
DB: 5–9	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Sample 1									
Post × Treat	1.451	1.156	1.190	0.468	0.280	0.278	– 0.883	– 0.935	– 0.880
	(0.014)	(0.013)	(0.013)	(0.012)	(0.011)	(0.011)	(0.014)	(0.014)	(0.013)
Obs		3458			3107			3363	
Sample 2									
Post × Treat	1.760	1.446	1.406	0.710	0.380	0.349	– 0.723	– 0.644	– 0.590
	(0.015)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.013)
Obs		2500			2364		2506		

				Control Groups		
Treatment	**Pension: 1–4**			**Pension: 10–13**		
Pension: 5–9	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Sample 1						
Post × Treat	1.794	1.508	1.552	– 0.150	– 0.217	– 0.204
	(0.012)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
Obs		6485			5378	
Sample 2						
Post × Treat	1.943	1.473	1.468	0.091	– 0.132	– 0.132
	(0.012)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
Obs		4472			3962	

Notes: *, † and ‡ indicate significance at 1%, 5% and 10% levels, respectively. Robust standard errors are given in parentheses. All model specifications include a set of panel year dummies and a set of birth year dummies. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys. Sample 2 excludes data from the 1996 SIPP survey.

support—with a weighted average of the control group's respondents based on the value of their propensity scores[26] in the pre- and post-reform periods. Each resulting individual counterfactual outcome is then used to estimate the mean difference in outcomes in both periods.

The DDM estimates—reported in Tables 12 and 13—corroborate both the unadjusted and

26. The variables used to determine propensity scores include personal family-related characteristics, job-related characteristics, and local labor market characteristics. Full estimation results are available from the authors upon request.

TABLE 11

Placebo Difference-in-Difference on Voluntary Movers (in percent)

	Control Group DB: 1–4					
	Sample 1			Sample 2		
Treatment DB: 10–13	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Post × Treat	1.257	0.965	0.653	0.600	0.163	0.068
	(0.026)	(0.026)	(0.026)	(0.029)	(0.029)	(0.029)
Obs		3044			2172	

	Control Group Pension: 1–4					
	Sample 1			Sample 2		
Treatment Pension: 10–13	**SP1**	**SP2**	**SP3**	**SP1**	**SP2**	**SP3**
Post × Treat	−0.080	−0.413	−0.673	−0.263	−0.902	−0.998
	(0.021)	(0.021)	(0.021)	(0.025)	(0.024)	(0.024)
Obs		5508			3709	

Notes: *, † and ‡ indicate significance at 1%, 5% and 10% levels, respectively. Robust standard errors are given in parentheses. All model specifications include a set of panel year dummies and a set of birth year dummies. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys. Sample 2 excludes data from the 1996 SIPP surveys.

TABLE 12

Difference-in-Differences Matching Estimates, DB: 5–9 (in percent)

	1-Nearest Neighbor	Radius Matching[a]			Kernel Matching[b]			Local Linear Regression Matching[c]		
Sample 1										
DB: 1–4	7.664*	6.809*	7.297*	7.464*	6.794*	7.373*	7.460*	6.770*	6.911*	6.900*
	(0.023)	(0.022)	(0.022)	(0.020)	(0.024)	(0.021)	(0.021)	(0.022)	(0.021)	(0.021)
DB: 10–13	3.698‡	2.971‡	3.513†	4.023†	2.904	3.206‡	4.017†	2.898	2.758	2.889
	(0.020)	(0.017)	(0.017)	(0.017)	(0.019)	(0.017)	(0.016)	(0.018)	(0.017)	(0.018)
DC: 5–9	1.041	0.445	0.646	0.964	0.638	0.541	0.962	0.523	0.667	0.693
	(0.022)	(0.022)	(0.019)	(0.019)	(0.021)	(0.020)	(0.018)	(0.022)	(0.021)	(0.020)
Sample 2										
DB: 1–4	6.234†	5.346†	5.775†	5.791†	5.424†	5.762†	5.790†	5.538†	5.532†	5.568†
	(0.027)	(0.025)	(0.024)	(0.023)	(0.026)	(0.026)	(0.025)	(0.024)	(0.025)	(0.025)
DB: 10–13	2.190	1.696	2.514	2.958	1.780	2.266	2.952	1.709	1.912	1.985
	(0.022)	(0.020)	(0.018)	(0.018)	(0.020)	(0.018)	(0.019)	(0.019)	(0.019)	(0.018)
DC: 5–9	0.622	0.550	0.597	0.829	0.592	0.492	0.826	0.658	0.574	0.551
	(0.022)	(0.024)	(0.021)	(0.020)	(0.024)	(0.021)	(0.020)	(0.023)	(0.022)	(0.022)

Notes: Bootstrap standard errors with 500 replications in parentheses. Trimming level: 5. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys. Sample 2 excludes data from the 1996 SIPP surveys.

[a] Caliper = 0.02, 0.2 and 02.

[b] Kernel bandwith = 0.02, 0.2 and 02.

[c] Local Linear regression bandwith 0.02, 0.2 and 02.

adjusted regression results. Taken together, our results present convincing evidence that the vesting reform of TRA ’86 successfully fostered the voluntary job mobility of treated workers. Moreover, the robustness of the estimated treatment effect to the inclusion of additional controls and estimation methods provides further indications that any changes in the composition of the treatment and control groups that occurred over time are most likely uncorrelated with the treatment.

VI. CONCLUSIONS

This study represents the first effort to evaluate the labor mobility impact of reforms aimed at improving the portability of pension rights. It offers a significant contribution to the literature by showing that policy-related variation in the factors that tend to tie workers to their jobs can be exploited to shed further light on the complex pension-mobility nexus.

We exploited, as a natural experiment, a pension reform brought about by the Tax Reform Act

TABLE 13

Difference-in-Differences Matching Estimates, Pension: 5–9 (in percent)

	1-Nearest Neighbor	Radius Matching[a]			Kernel Matching[b]			Local Linear Regression Matching[c]		
Sample 1										
Pension: 1–4	5.093*	5.164*	5.367*	5.019*	5.135*	5.427*	5.023*	4.850*	4.562*	4.701*
	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.015)	(0.017)	(0.016)	(0.017)
Pension: 10–13	3.613†	2.725‡	4.083*	4.274*	2.641‡	3.891*	4.274*	2.867‡	3.306†	3.485†
	(0.015)	(0.015)	(0.014)	(0.014)	(0.015)	(0.014)	(0.013)	(0.015)	(0.015)	(0.015)
Sample 2										
Pension: 1–4	3.535‡	4.189†	4.319†	3.709†	4.256†	4.325†	3.721†	4.033†	3.394‡	3.675†
	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.018)
Pension: 10–13	3.090‡	2.052	3.520†	3.502†	1.899	3.348†	3.505†	2.302	2.802‡	3.073‡
	(0.018)	(0.017)	(0.016)	(0.015)	(0.017)	(0.015)	(0.015)	(0.016)	(0.017)	(0.016)

Notes: Bootstrap standard errors with 500 replications in parentheses. Trimming level: 5. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys. Sample 2 excludes data from the 1996 SIPP surveys.

[a]Caliper = 0.02, 0.2 and 02.

[b]Kernel bandwith = 0.02, 0.2 and 02.

[c]Local Linear regression bandwith 0.02, 0.2 and 02.

of 1986. This reform induced a sharp cut—from 10 to 5 years—in the length of the vesting period required for full accrual of pension rights. We used this transparent, plausibly exogenous source of variation to identify our treatment group as employees who were vested under the new rules but would not have been vested otherwise (DB participants with 5–9 years of tenure). We further explored the sensitivity of our results to different model specifications and the use of different treatment/control groups, samples, outcomes, and econometric techniques. Overall, our findings provide robust evidence that the reform had a positive and significant effect on voluntary job mobility of the treatment group.

Our results confirm a significant impact of pension portability policies on voluntary job mobility, as posited by the implicit contract theory of pensions. Our preferred estimate—with DB: 10–13 as control group—indicates that, compared to the pre-reform baseline, the reduction in the vesting period increased voluntary job mobility of the treated group (DB: 5–9) by 75%.

To some extent, this result corroborates the significant "job lock" effects found in the literature exploiting similar policy-related variation among workers with employer-sponsored health insurance.[27] We provide further evidence suggesting that strengthening portability provisions may reduce labor market distortions caused by job lock.

In this perspective, our results have significant value for policy makers. First, despite the decline of DB plans over the last 30 years, as of 2011 about 20% of the private-sector workforce were still covered by these plans in the United States (Wiatrowski 2012). Second, DB plans remain important in other countries, including Canada, Germany, and the United Kingdom, where the trend toward DC plans has been less pronounced. Finally, whereas further cuts to the vesting schedules have been introduced in DC plans since the passage of the Pension Protection Act of 2006, DB plans still have the vesting standards introduced by TRA '86. This leaves room for further reductions in the vesting schedules currently offered by DB plans.

Our findings are important from a policy perspective because they show that shorter vesting periods reduce the barriers to job change for DB plan participants by reducing the retirement income losses of early leavers. Further research is needed to determine whether this translates into efficiency gains, and if so, to measure the empirical magnitude of these gains.

APPENDIX A: MEASURING AND DEFINING JOB MOBILITY AND PENSION PLAN COVERAGE

A. PENSION PLAN COVERAGE

In the SIPP public use files, specific job information is recorded for up to two jobs simultaneously held by respondents. We focus on the primary job, for which comprehensive

27. Estimates of job-lock effect due to the lack of portability of employer-provided health insurance broadly range from 10% to 72%; see Gruber and Madrian (1994), Bansak and Raphael (2008) among others.

pension information is collected in the *Retirement and Pension Plan Coverage* topical module.[28]

Prior to SIPP 1996, this topical module was first collected in wave 4. First, respondents holding a job were asked whether *their employer or union had a retirement plan for any of its employees*. Respondents reporting that their employer offered such a plan were asked whether *they were included in the plan*, and, if so, whether *the retirement benefits of their plan were determined by years of service and pay or by the amount of contributions to the plan*. Respondents were further asked whether their *employer offered a salary reduction plan (401 K or 403B plan)*, and, if so, whether *they participated in this plan*.

The *Retirement and Pension Plan Coverage* module of the 1996 SIPP was collected at a later wave (wave 7) using the CAPI system. It collected pension coverage information comparable to earlier panel years, as well as a number of additional questions.

As in earlier panel years, respondents holding a job were asked whether *their employer had any kind of pension or retirement plans for anyone in their company or organization*. Those reporting that their employer offered such a plan were also asked whether *they were included in the plan*, and whether *the retirement benefits of their plan were defined by a formula involving their earnings and years on the job or contributions made by them and/or their employer into an individual account*. Respondents were further asked whether *a 401 k plan was offered by their employer*, and, if so, whether *they were included in such plan*.

Unlike earlier panel years, plan participants were also asked a series of detailed follow-up questions, including whether *their contributions were tax-deferred* or *matched by the employer*, whether respondents *are able to choose how any of the money in the plan is invested and how much*, and whether *they did—or could—take any money out of their plan in the form of a loan*. In principle, this supplementary information could be used to determine pension participation status more precisely by further minimizing measurement error arising from potential misreporting (Copeland 2002). However, we chose not to exploit this information for the sake of ensuring comparability of the pension participation status definition used across all panel years in our analysis.[29]

We use the aforementioned questions to assign each respondent a (mutually exclusive) pension participation status—participating in a DB (DC) plan, or not participating in any employer-sponsored pension arrangement. Participants in a plan whose benefits are based on a formula involving years of service or salary were assigned a DB plan participation status. Participants in a plan whose benefits are based on the amount contributed to the plan were assigned a DC plan participation status. Respondents who reported participation in a 401 k plan and were not enrolled in any DB plan were also assigned a DC plan participation status.[30] Respondents who did not report participation in any employer-sponsored pension plan were assigned a non-participation status.

Pension plan participants were not asked about the specific vesting schedule offered by their plan, or whether their plan required a waiting period for participation eligibility.[31] As a consequence, tenure in the plan for participating workers is assumed to be equal to the years of tenure with the current employer, and, following our discussion in Section II, we assume that all DB participants with 5–9 years of tenure (DB: 5–9) were not vested before TRA ’86, unlike all DC: 5–9 employees.

B. JOB MOBILITY

We measure labor mobility over 4 consecutive waves of data. This is the largest continuous time window that can be used to construct a comparable measure across all SIPP panel years as further detailed *supra*.

In each wave of core data, a respondent who reported having an employer was assigned a unique job identification number. Hence, a job corresponds to a respondent-employer match. This unique job identification number remains constant across waves unless a change of employer from the previous wave occurs.

We use the reference month of the wave which collects employer-sponsored pension information as our starting point to measure mobility. Only seven waves of data were collected in SIPP 1986, making the use of four consecutive waves of data the largest possible time window to consistently measure job mobility across all panel years used in this study.

More precisely, we measure job mobility between August 1984 and November 1985 in SIPP 1984 (waves 4–7), December 1986 and March 1988 in SIPP 1986 (waves 4–7), January 1991 and April 1992 in SIPP 1990 (waves 4–7), January 1993 and April 1994 in SIPP 1992 (waves 4–7) and March 1998 and June 1999 in SIPP 1996 (waves 7–10).

Each respondent with a job was assigned a mover status if their unique job identification number changed between waves 4 and 7 in SIPP 1984, 1986, 1990 and 1992, or between 7 and 10 in SIPP 1996.

Prior to SIPP 1996 and the use of CAPI system, which significantly improved data quality, assigned job identification numbers were not always consistently recorded across waves (Stinson 2003). As a result, to improve the accuracy of our job mobility variable, we further exploit additional variables relevant to this study.

In the seventh-wave topical module of SIPP 84 and 86, respondents were also asked to report whether they were working for the same employer during the fourth wave. We use this information and assign a mover status to respondents who reported in the seventh wave that they were not working for the same employer as in the fourth wave. This approach follows Gustman and Steinmeier (1993).

In 2006, the Census Bureau released newly edited, longitudinally consistent job identification numbers for SIPP 1990–1993; see Stinson (2003) for further details. We used these newly corrected job identification numbers to construct our job mobility variable for SIPP 1990 and 1992.

APPENDIX B: FULL REGRESSION RESULTS (MODEL SPECIFICATION 3)

Tables B1. and B2 report the full set of parameter estimates for our most comprehensive specification (SP3). Full

28. This module was collected in waves 4 and 7 of SIPP 1984 and 1986, in wave 4 of SIPP 1990 and 1992 and in wave 7 of SIPP 1996.

29. Note, as discussed at length in Section II, that any measurement error in self-reported pension plan type would contribute to a downward bias of our coefficient of interest, preserving its interpretation as a lower estimation bound.

30. This definition assumes the DB plan as the primary plan for participants holding both a DB and a DC plan.

31. When required by the plan, waiting periods are usually short. ERISA sets a minimum eligibility requirement of 1 year of service. However, employers may and usually do offer a more generous eligibility cutoff.

TABLE B1
Model 3: Marginal Effects

Sample 1	**Control Groups**		
DB: 5–9	**DB: 1–4**	**DB: 10–13**	**DC: 5–9**
Post × Treatment	0.064** (0.023)	0.037** (0.018)	0.005 (0.021)
Treatment	−0.045* (0.024)	0.000 (0.020)	−0.008 (0.018)
Post	−0.229** (0.051)	−0.053 (0.041)	0.028 (0.048)
Black	−0.001 (0.016)	−0.010 (0.013)	−0.005 (0.014)
Education (years)	0.004 (0.003)	0.003 (0.002)	0.002 (0.002)
Non-single	−0.021 (0.016)	−0.002 (0.014)	−0.016 (0.015)
Children < 18	−0.008 (0.015)	−0.014 (0.013)	−0.002 (0.014)
Family size	0.009* (0.005)	0.006 (0.004)	0.006 (0.004)
Spouse empl.	−0.004 (0.015)	−0.011 (0.012)	0.000 (0.012)
Housing tenure	−0.006 (0.012)	−0.009 (0.010)	−0.008 (0.011)
Family health pbl.	0.037 (0.071)	0.019 (0.062)	0.092 (0.062)
Experience	0.001 (0.001)	−0.001 (0.001)	−0.001 (0.001)
SMSA	−0.001 (0.011)	0.001 (0.009)	0.010 (0.010)
Northeast	−0.012 (0.016)	−0.010 (0.012)	−0.004 (0.013)
South	−0.013 (0.014)	−0.006 (0.011)	−0.009 (0.011)
West	−0.019 (0.016)	−0.010 (0.012)	0.006 (0.014)
Log hourly wage	−0.016 (0.011)	−0.017* (0.010)	−0.015 (0.011)
Job tenure	−0.013** (0.004)	0.000 (0.003)	−0.005* (0.003)
Firms < 100	−0.015 (0.011)	−0.006 (0.009)	−0.010 (0.009)
Union coverage	−0.024* (0.013)	−0.007 (0.010)	−0.009 (0.012)
Empl. health insurance (HI)	−0.023 (0.024)	−0.032 (0.020)	−0.011 (0.021)
HI × fam. health	−0.027 (0.074)	0.007 (0.064)	−0.079 (0.065)
Spouse health insurance (SHI)	−0.002 (0.033)	−0.016 (0.029)	0.006 (0.028)
HI × SHI	0.019 (0.035)	0.026 (0.030)	−0.001 (0.029)
Transp., Comm.	−0.012 (0.016)	−0.003 (0.012)	−0.010 (0.015)
Wholesale trade	−0.055** (0.023)	−0.030 (0.019)	−0.019 (0.018)
Retail trade	0.002 (0.018)	−0.005 (0.015)	−0.002 (0.015)
Finance, Insur.	−0.012 (0.022)	0.004 (0.017)	−0.008 (0.018)
Professional	−0.007 (0.015)	0.003 (0.011)	0.017 (0.013)
Tech., Service	0.010 (0.014)	0.006 (0.011)	0.027** (0.012)
State Unemp. Rate	−0.018** (0.005)	−0.002 (0.004)	0.001 (0.006)
State Unemp. × Post	0.027** (0.007)	0.005 (0.006)	−0.002 (0.007)
Obs	3710	3226	3528

Notes: ** and * indicate significance at 5% and 10% levels, respectively. Robust standard errors in parentheses. All model specifications include a set of panel year dummies and a set of birth year dummies. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys.

TABLE B2
Model 3: Marginal Effects

Sample 1	**Control Groups**		
Pension: 5–9	**Pension: 1–4**	**Pension: 10–13**	**No Pension: 5–9**
Post × Treatment	0.048** (0.017)	0.042** (0.016)	−0.009 (0.019)
Treatment	−0.025 (0.019)	−0.025 (0.017)	−0.021 (0.018)
Post	−0.139** (0.039)	−0.010 (0.036)	0.032 (0.045)
Black	0.009 (0.012)	0.003 (0.011)	−0.007 (0.013)
Education (years)	0.006** (0.002)	0.002 (0.002)	0.006** (0.002)
Non-single	−0.011 (0.012)	−0.009 (0.011)	−0.011 (0.013)
Children < 18	−0.007 (0.011)	−0.002 (0.010)	−0.012 (0.012)
Family size	0.007* (0.004)	0.003 (0.003)	0.004 (0.004)
Spouse empl.	−0.010 (0.010)	−0.010 (0.009)	−0.008 (0.011)
Housing tenure	−0.006 (0.009)	−0.006 (0.009)	0.003 (0.009)
Family health pbl.	0.020 (0.058)	0.116** (0.055)	0.011 (0.041)
Experience	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
SMSA	0.012 (0.008)	0.008 (0.007)	0.013 (0.009)
Northeast	−0.008 (0.012)	0.001 (0.010)	−0.008 (0.012)
South	−0.005 (0.010)	0.000 (0.009)	−0.011 (0.010)
West	−0.006 (0.011)	0.011 (0.010)	−0.003 (0.013)
Log hourly wage	−0.024** (0.008)	−0.012 (0.008)	−0.022** (0.010)
Job tenure	−0.014** (0.003)	−0.004 (0.003)	−0.007** (0.003)
Firms < 100	−0.018** (0.008)	−0.010 (0.007)	−0.005 (0.009)
Union coverage	−0.015 (0.011)	−0.009 (0.009)	0.002 (0.012)

TABLE B2
Continued

Sample 1	Control Groups		
Pension: 5–9	**Pension: 1–4**	**Pension: 10–13**	**No Pension: 5–9**
Empl. health insurance (HI)	−0.032** (0.016)	−0.017 (0.015)	−0.017 (0.014)
HI × fam. health	−0.015 (0.060)	−0.099* (0.057)	−0.002 (0.045)
Spouse health insurance (SHI)	0.002 (0.022)	−0.004 (0.022)	−0.005 (0.021)
HI × SHI	0.020 (0.023)	0.016 (0.023)	0.013 (0.023)
Transp.,Comm.	−0.016 (0.013)	−0.002 (0.011)	−0.019 (0.014)
Wholesale trade	−0.017 (0.015)	−0.010 (0.013)	−0.005 (0.015)
Retail trade	0.010 (0.013)	−0.002 (0.012)	0.009 (0.012)
Finance, Insur.	−0.004 (0.016)	0.007 (0.014)	0.016 (0.019)
Professional	−0.004 (0.012)	0.010 (0.011)	0.017 (0.012)
Tech., Service	0.017* (0.010)	0.016* (0.009)	0.019* (0.011)
State Unemp. Rate	−0.008* (0.004)	0.002 (0.004)	0.000 (0.005)
State Unemp. × Post	0.016** (0.006)	−0.003 (0.005)	−0.001 (0.006)
Obs	6959	5605	5122

Notes: ** and * indicate significance at 5% and 10% levels, respectively. Robust standard errors are in parentheses. All model specifications include a set of panel year dummies and a set of birth year dummies. Sample 1 includes data from the 1984, 1986, 1990, 1992 and the 1996 SIPP surveys.

parameter estimates for specifications 1 and 2 are omitted for the sake of brevity; complete results are available upon request from the authors.

Most of the estimated coefficients have the expected signs, although they often lack statistical power. Consistent with the literature, our results suggest that being married and having children are positively associated with voluntary turnover. Unexpectedly, we find a significant positive association with family size. Years of schooling is positively associated voluntary turnover. This is consistent with the conjecture that a higher level of education could foster labor mobility by offering better labor market alternatives. Our estimates are, however, often statistically insignificant.

The estimated coefficients for job-related predictors are also in line with those reported in the literature. In particular, we find that voluntary turnover is negatively associated with job tenure, current wage, and working in a small firm.

Being member of a union and being covered by employer-sponsored health insurance are both negatively associated with voluntary turnover, unlike spousal health insurance coverage. Our results suggest that having family members with health problems is positively associated with voluntary turnover. However, job turnover appears to be negatively affected by the interaction of family health with having employer-sponsored health insurance, and positively affected by the interaction of employer-sponsored health insurance and spousal health insurance coverage. The latter result is largely in line with the job-lock literature (see Madrian 1994; Berger, Black, and Scott 2004, among others).

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