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ABSTRACT

Many employers have implemented dependent verification (DV) programs, which aim to reduce employee benefits costs by ensuring that ineligible persons are not enrolled in their health plan as dependents. We evaluate a DV program using a panel of health plan enrollment data from a large, single-site employer. We find that dependents were 2.7 percentage points less likely to be reenrolled in the year that DV was introduced, indicating that this fraction of dependents was ineligibly enrolled prior to the program's introduction. We show that these dependents were actually ineligible, rather than merely discouraged from re-enrollment by compliance costs.

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1. Introduction

In the United States, most workers who receive health insurance through an employer also enroll one or more dependents.[1] A significant fraction of employers' healthcare expenditure is attributable not to employees themselves, but to these dependents, who typically account for about half of the population enrolled in employer plans (US Census Bureau [2011]). Because employers often heavily subsidize the costs of insuring dependents, these health benefits can create incentives for workers to try to enroll individuals who do not actually qualify as dependents.[2] Typical employer subsidies yield thousands of dollars in expected benefits that an employee could exploit by claiming (say) a niece or a nephew as a child.[3] On the other hand, outside of their contribution to the premium, the expected cost to employees of falsely claiming a dependent is about zero—in general, the only consequence is simply removal of the ineligible individual from the policy. In short, strong incentives exist to claim ineligible dependents.

[1] The 2010 Kaiser Family Foundation Survey of Employer Benefits.

[2] Figures from the Medical Expenditure Panel Survey show that in a typical employer plan, the average insurance payout for a dependent was around $4,700 in 2010, while the average incremental worker contribution for enrolling that dependent ranged from $0 to $1,390, depending on the plan structure (Crimmel [2011]).

[3] For the employer that we study in this paper, we can make a more precise statement: the average insurer payment for a dependent in 2009 was $2,780. The incremental cost of adding a dependent ranged from $0 (if adding an additional child to a family plan) to $2028 (if moving from "employee only" to "employee plus spouse" in the most expensive plan).

To help rein in costs and prevent abuse, in recent years many employers have implemented dependent verification (DV) programs, which aim to ensure that ineligible persons are not enrolled in their health plan as dependents. Typically, DV policies require employees to present documentation proving the status of dependents, like marriage licenses for spouses and birth certificates for children.

While almost non-existent a decade ago, DV programs are gaining popularity in both the public and private sectors. As one news account noted:

> "Dependent eligibility audits," in which companies demand proof that spouses and children qualify for medical benefits, are swiftly becoming both fashionable and financially rewarding for companies frantic to curb the runaway costs of health coverage. Companies such as Boeing General Motors and American Airlines have been asking workers to send in marriage licenses, birth certificates, student IDs, and tax returns. The goal: to cull the benefits rolls of ineligibles, which could include ex-spouses, stepchildren who live elsewhere, or 29-year-old college grads still being claimed as dependents. In the last year, the number of benefit audits "has just exploded," says Watson Wyatt human resources consultant Susan Johnson (Epstein and McGregor [2007]).

Of the 507 large firms surveyed in 2010 by the management consulting firm Towers Watson [2010, p. 7], 55 percent had a dependent verification program in their health plan in 2008, 61 percent in 2009, and 69 percent in 2010.

These programs are potentially important not only to the bottom lines of private sector enterprises, but to the public finances as well. Health benefits make up more than 10 percent of state and local government employee compensation (Bureau of Labor Statistics [2012]). Weeding out ineligible dependents is often seen as an easy way to keep benefits costs lower and to trim public sector compensation budgets. By 2012, at least 38 states had implemented

dependent verification or audit policies for state employee health plans.[4] When justifying the imposition of these policies, states almost universally cited the cost burden of ineligible dependents as the motivating factor.

All of this begs the question of whether these programs are actually effective in reducing ineligible enrollees and the associated costs. Some anecdotal evidence suggests that they do. According to news reports, for example, "Goodyear Tire & Rubber trimmed 13% of its 70,000 dependents, due to ineligibility, in its 2005 audit, saving 6% on costs" (Epstein and McGregor [2007]). Similarly, "HRAdvance, a Dallas-based human-resources company, completed an audit of a large retailer's health plan. The audit revealed that 12.6% of the dependents didn't meet the plan's eligibility requirements and their coverage was dropped, producing projected first-year savings to the employer of $25 million net of the audit's cost." (Knight [2009]). More generally, according to an industry consultant, "employers remove an average of 8% to 12% of dependents after conducting an audit, of which 60% to 70% are children." (Silva [2008]). Some evidence suggests that DV programs have been quite effective in the public sector as well. In West Virginia, auditors have reported that over 8 percent of enrolled dependents in a population of 137,000 dependents of state workers were ineligible (Healthcare Data Management, Inc. [2011]).

Despite these claims, we are aware of no independent econometric work documenting either the need for or the efficacy of dependent verification. Knowing the benefits of DV

[4] Source: Authors' count, primarily tallied from state employee handbooks and web material available from public employee benefits authorities.

programs in terms of reduced claims is important, because their administration is not without cost. Employees have the bother of finding and presenting the relevant documentation: "Even those whose dependents are eligible may have to work hard to prove it," (Epstein and McGregor [2007]). For employers there are explicit costs of running the program, borne either internally or in the form of payments to external consultants, which could be $20 to $30 per worker (Epstein and McGregor [2007]). Employers may also have to expend time and resources negotiating dependent verification policies with their employees' unions, especially in the public sector. As well, some reports indicate that DV programs lead to morale problems. When Minnesota instituted a DV program for all state employees, press reports indicated that it caused "headaches—and a lot of anxiety." The president of the Minnesota Association of Professional Employees said, "What we're upset about is that it just seems like harassment of state employees. And there's a presumption of guilt that's disturbing" (Grow [2012]). Morale is not just an issue for the employees who have to verify their dependents' legitimacy. The Employer whose program is studied here recounted to us that the staff members who administered the program also were unhappy because of abuse they received from their fellow employees.

Beyond the costs and morale problems associated with implementing verification, DV programs could possibly discourage *eligible* dependents from enrolling due to the costs associated with gathering and presenting the required documentation. Even the enrollment of employees, themselves, could be affected by DV if such costs induce whole families to switch over to a spouse's employer plan.

While there has been no independent research on dependent verification programs for health insurance, several studies have examined the practice of falsely claiming dependents in

other contexts. For example, in tax year 1987, the federal government began requiring Social Security numbers for all dependents (five years and older) reported on individual tax returns. Several papers have estimated the effect of this provision on the number of dependents claimed.[5] Benefits under the Earned Income Tax Credit also are a function of the number of dependents; here too, there have been attempts to eliminate false claims (See Scholz [2008] and Holtzblatt and McCubbin [2004]). Other related literatures focus on insurance fraud in settings including auto (Tennyson and Salsas-Forn [2001]), home (Phua, Lee, Smith, and Gayler [2010]), and disability (Peng, *et al.* [2007]). Taken together, these papers show that when there is some gain to be had by falsely claiming dependents, a substantial number of people will do so. Nonetheless, it is not obvious that the insights from tax and insurance fraud studies translate to the employer health insurance setting. In the case of health insurance, the population of enrollees is relatively small and employees might have personal relationships with each other and with their employer. They might be disinclined to commit fraud in such an environment.

In this paper, we evaluate a DV program using health plan enrollment data from a large, single-site employer (hereafter referred to as "the Employer") who implemented it during the open enrollment period for 2010. Our basic strategy is to track the enrollment status of individuals before and after the policy was implemented, including both employees and dependents. Because by definition DV plans directly impact only dependents, the employees serve as a natural control for year-to-year trends in enrollment, allowing identification of the effect of DV on the enrollment of dependents.

[5] See for example, Szilagyi [1990], although because of data limitations, there is no econometric analysis

Our main findings are: 1) Relative to all other years, dependents were 2.7 percentage points less likely to be reenrolled in the year that DV was introduced. 2) DV did not induce employees to leave the Employer's plan and (say) put themselves and their dependents on the spouse's plan. 3) The disenrollment effect was about twice as strong for children as for spouses: 2.9 percentage points for children versus 1.5 percentage points for spouses. 4) The results differed among some subgroups of employees. The effects were especially large for dependents of maintenance and service staff, same-sex partners, and older children. Our estimates indicate that approximately 20 percent of children 24 and older who were enrolled in the program were actually ineligible. Effects for lower-earning employees were larger, but only marginally statistically different from higher-earners.

With the basic results on disenrollment patterns in hand, we explore several important ancillary issues. In particular, we find that: 1) Disenrollment occurred because dependents were actually ineligible, rather than because providing the documentation was inconvenient for the employees. 2) There is no evidence that the removal of ineligible dependents caused the disenrollment of *eligible* dependents within the same family or changed the family's choice of health plans. 3) The program generated annual cost savings of roughly $46 per enrolled employee, though as we discuss below, these are primarily private gains. 4) Finally, an important interaction with the Affordable Care Act of 2010 will likely attenuate the potential cost savings of all DV plans from 2011 onwards: We find that a substantial fraction of the cost savings of the DV program was generated by removing ineligible older children, and the Affordable Care Act essentially renders all older children up to age 26 eligible, removing them as targets of DV programs. Hence, as the state governments and private employers that have implemented DV

programs adapt to the new regulatory environment, the popularity of dependent verification programs may well diminish.

The remainder of the paper is organized as follows. In Section 2 we describe the provisions of the DV program. Section 3 discusses the data, and exhibits some preliminary calculations. In Section 4 we outline the econometric strategy, and present the basic results. Section 5 looks at whether compliance costs have an effect on disenrollment behavior, presents an estimate of the program's cost savings for the Employer, speculates about the welfare effects of the program, and discusses the interaction between DV programs and the Affordable Care Act of 2010. Section 6 concludes.

2. Description of the Dependent Verification (DV) Plan

We study the DV program of a large, single-site employer in the service sector that had about 6,000 employees over the study period, of whom about 5,100 were enrolled in its health insurance program each year. The Employer offered a standard set of insurances plans; employees could choose among PPO (preferred-provider organization), POS (point of service), or HMO (health maintenance organization) plans. Comparisons of the Employer's plan characteristics to national averages reported by the Kaiser Family Foundation show that employee take-up rates and employee contributions to premiums were typical among employers of similar size over the study period. As is also conventional, employees chose their health care plan every fall. Very few employees leave the Employer between December, when enrollment decisions are finalized, and the subsequent January, when they take effect.

Prior to open enrollment for 2010, which occurred in late 2009, employees did not need to provide documentation to verify that dependents on their health insurance policies were eligible for inclusion. In late 2009, the Employer announced that starting during the open enrollment period for 2010, employees would have to authenticate the status of their dependents.[6] Verification took place throughout the open enrollment period and was essentially complete by early December. Even for the one-off and problematic cases, all verification was complete before January 31, 2010.[7] Executives at the Employer report that they decided to implement the DV program as a cost saving measure in line with industry best practices. The DV program was entirely designed and administered by the Employer; no consultants were involved. The program applied different criteria for eligibility to various groups of dependents. The details are summarized in Figure 1. In all cases, some sort of documentary evidence was required. For spouses, for example, the employee was required to provide both a marriage certificate and a redacted tax return from the previous year, with the tax return serving to prove that the claimed marital status was still current.[8] In subsequent years, all new enrollees were required to provide

[6] The Employer implemented no other changes in compensation or benefits practices at this time. As far as we know, there were no relevant significant changes in state or federal law at the time that the DV program was implemented, but even if there were, their impact would be absorbed by the time effects in our econometric model.

[7] In cases where dependents were removed, they were allowed to be added back to the employee's coverage mid-year, provided that the proper documentation was received. However, such dependents were not enrolled retroactively. This situation arose in only a very few cases.

[8] This raises the question of why an individual would want to keep a divorced spouse on his or her policy. Our data do not allow us to provide a definitive answer, but it is important to note that

such documentation, but no attempts were made to re-verify that existing dependents (who had already passed their initial screen) were still legitimately enrolled, although the Human Resources Department made a special effort to prevent employees from adding someone who had been denied coverage in a previous audit.

The provisos relating to dependent children are particularly important. All biological, adopted and foster children up to age 18 who were claimed as dependents on federal tax returns were eligible enrollees. For many years, the Employer had also allowed older children to be included on an employee's policy up to the end of the year in which the child turned 25.[9] However, these older children either had to be full time students who provided less than half of their own support and who listed the employee's address as their own permanent address, or they had to live with the employee and the employee had to provide over one half of their support. These conditions were identical in the year prior to and the year following introduction of the DV program. However, prior to 2010, employees were not required to provide *evidence* that these conditions were met. In the absence of verification, an enrolled dependent "child" might have been ineligible for several reasons. First, the enrolled dependent may not have truly been a child, but rather a grandchild, some other family relation, or no relation at all. Second, even an actual (natural, adopted, or foster) child would have been ineligible if the child was older than

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divorce settlements sometimes require one spouse to continue providing health insurance for the other. The Employer explicitly notes that ex-spouses are ineligible, even if the employee has a Qualified Domestic Relations Order requiring him or her to provide health insurance coverage.

[9] This cutoff was the Employer's choice; there was no applicable federal or state mandate stating that employer-provided policies had to include 25 year old children.

25, or the child was under 25 but did not meet the other requirements. In short, ineligible children were either not the employee's children or they were children but did not satisfy the other criteria in Figure 1.

A final issue relates to same-sex domestic partners. Prior to the DV program, an employee who wanted to include a same-sex domestic partner on his or her policy had to provide an affidavit of domestic partnership. The Employer provided a list of 11 documents that could be used to support the affidavit (e.g., a Qualifying Domestic Partnership Agreement, joint bank account, joint mortgage agreement, durable property or health care power of attorney). The employee could in principle be requested at any time to confirm that the affidavit was truthful by producing any three documents from the list. In practice, though, such documents were never requested prior to the DV program. After introduction of the DV program, they routinely were.

3. Data

Our data are drawn from the Employer's administrative records. They are proprietary and were provided to us on a confidential basis. All identifying information, such as Social Security numbers, was removed from the records, although scrambled identifiers were provided to allow us to link enrollment records for families and individuals over time. The quality of the administrative data was excellent: only two observations were dropped because of missing data, and only a few observations were dropped due to apparent inconsistencies. Our data cover the period December 2007 to January 2012 for all enrolled employees and their enrolled dependents, if any. For each individual, the data include his or her relationship to the employee, age, gender, and the type of plan in which he or she was enrolled (HMO, PPO, POS). Finally, the data include each employee's job category and banded salary.

We observe enrollment status at two points in each calendar year. The computations of enrollment and disenrollment rates used throughout the analysis are based on comparisons of the enrollment status of individuals on December 1 of a given year (after open enrollment decisions are made for the following year but before they go into effect) and January 31 of the subsequent year (after the decisions are in effect).[10] In the analysis that follows, we examine re-enrollment across plan years, counting employees or dependents who were enrolled in December but not in the subsequent January as having disenrolled. It is possible that some individuals did not show up as enrolled because the employed family member left the Employer between December and January, but according to staff in the Human Resources department, such departures were no more common than between any other pair of adjacent calendar months. More importantly, as explained below, our econometric strategy takes into account any month-to-month turnover that is similar across years or is similar in a single year among both employees and dependents in the same family.

Our analysis sample includes only the employees and their family members who are enrolled in the Employer's plans at some point.[11,12] The unit of observation is a person-year.

[10] The new plan choices actually go into effect on January 1. However, the specialists in the Employer's benefits office reported that there are sometimes errors and discrepancies in the records at the start of the year, so it is safer to use the information as of January 31, after most of the corrections have been made.

[11]Because not all employees enroll, our analysis sample is only a subset of the entire population of employees. According to the Human Resources staff, the characteristics of the members of this subset are similar to those of the remainder of the employee population.

That is, each year that an individual was enrolled is associated with an observation for that individual. For example, if an individual (employee or dependent) was enrolled in 2008 and 2009, then he or she would account for two observations. Table 1 shows summary statistics computed over all observations. The first column shows the figures for enrolled employees, the second for dependent spouses and the third for dependent children.

The figures indicate that the number of enrolled dependents is slightly larger than the number of enrolled employees, underscoring the potential contribution of dependents to total plan costs. About half of enrollees are female, though women are somewhat more likely to be dependents than employees. The average employee who has insurance coverage through the Employer enrolls one dependent, generating an average family size of 2 in the sample.

The last row of Table 1 reports the fraction of individuals enrolled in December of a year who are still enrolled in January of the following year. The average re-enrollment rate among employees from December to January is 0.98. The rates are slightly lower for spouses and children.

Figure 2 provides a first look at the possible impacts of the DV program. It shows the reenrollment rates of employees and dependents from December to January for 2007-08 through 2011-12. We focus on re-enrollment, rather than all flows into enrollment in order to remove the

[12] This is a common feature of studies analyzing administrative health plan data, and one that poses no particular econometric challenge here, since our focus is on whether previously-enrolled employees and dependents (a group which we observe) re-enroll differentially across the policy change (an outcome which we also observe).

noise associated with new hires and other families who enter the system in a given year. Because our focus is on re-enrollment, any effects of the DV program should appear only in the policy change year: In the years after program implementation, any new dependents flowing into enrollment will have been verified as eligible, leaving no stocks of ineligibles for the program to bar during the annual re-enrollment period. The key question, therefore, is whether there are differences between re-enrollment in 2009-10, when the program was introduced, and the years preceding and following.

The first two panels show the results for employees without and with dependents, respectively. By definition, employees' eligibility for health insurance was not affected by the introduction of the DV program, so that, in effect, they serve as a control group. Consistent with this notion, regardless of whether or not they had dependents, their re-enrollment rate in 2009-10 is similar to the other years, between 98 and 99 percent. In contrast to the stability of re-enrollment behavior for employees, the third panel shows that the re-enrollment rate for dependents in 2009-10 is substantially lower (93.7 percent) than either before (96.1 percent for 2008-09) or after (97.1 percent for 2010-11).[13] Taken together, the three panels suggest that the increased disenrollment rates for dependents in the year the DV program was introduced were not due to some overall change in the environment that affected employees and dependents alike.

[13] After the implementation of the program, re-enrollment would return to its pre-program steady state so long as the natural re-enrollment rates of the pre- and post-DV populations do not differ. More precisely, the condition required to return to pre-DV re-enrollment rates is that the re-enrollment rate of eligible dependents be the same as re-enrollment rate of ineligibles. There is no *a priori* reason to expect differential re-enrollment.

Although the program targeted only dependents, it is conceivable that the enrollment of employees could be impacted indirectly by affecting their dependents. This could happen, for example, if in response to the policy an entire family migrated to a spouse's health plan at another employer. Nonetheless, as noted above, Panel B shows that, just as with employees without previously enrolled dependents, employees with such dependents continue to re-enroll themselves in 2009-2010 at rates similar to adjacent years. This provides further evidence of the suitability of using (all) employees as a control group.

The dependent re-enrollment rate in 2011 appears higher than in the policy change year (2010) or the two years preceding it. We provide some evidence below that this phenomenon is related to the dependent coverage provision in the Affordable Care Act (ACA), which went into effect in 2011 and mandated that employers must offer coverage to children up to age 26 regardless of whether the children reside with the parent or are dependent on the parent in any way. Indeed, as shown in the next section, the increased reenrollment in 2011 in our data is concentrated entirely among older children, which is consistent with the notion that this change was induced by the ACA. We discuss the interaction between the ACA and dependent verification programs in more detail below.

While Figure 2 is interesting and informative, it leaves some important loose ends. First, we don't know whether the apparently large difference that we observe in 2009-10 is due to ineligibles disenrolling or just random variation. Second, this coarse-grained summary does not allow us to examine in detail which groups of dependents and employees are associated with the most ineligible enrollments. Finally, the graphs exploit neither the panel nature of our data, nor the information that links family members to one another. As we show below, this information

can be used to make inferences about whether dependents who disenrolled due to DV were actually ineligible or merely discouraged from re-enrolling due to the costs of complying with the program. We now develop an econometric approach for addressing these issues.

4. Econometric Strategy and Results

4.1 Setup

We estimate a series of linear probability models that take the following form:

$$re\text{-}enroll_{ijt} = \alpha_0 + \gamma * dependent_i + \sum_{t=2009}^{2012} \delta_t * Year_t + \sum_{t=2009}^{2012} \lambda_t * dependent_i \times Year_t + \boldsymbol{X_{ijt}\beta} + \varepsilon_{ijt}$$

In this equation, individuals are indexed by *i* and families by *j*, and $re\text{-}enroll_{it}$ equals one if individual *i* re-enrolled in the Employer's health plan in January of year *t*. The sample is comprised only of individuals enrolled in December of year *t* – 1, and includes up to 5 enrollment transitions for each person: December 2007 to January 2008, December 2008 to January 2009, December 2009 to January 2010, December 2010 to January 2011, and December 2011 to January 2012. We focus on this narrow window around the start of the new year to reduce the likelihood of picking up disenrollment that is due to natural attrition over the course of a full year. The time effects capture any influences common to the enrollment decisions of all potential enrollees in a given year. $Year_t$ is an indicator for observations in January of year *t*; the omitted year is 2008. The indicator $dependent_i$ equals one if the individual is a dependent (either a spouse or child), and zero otherwise. Hence, the coefficients on the interactions between

dependent$_i$ and the time effects tell us whether there was a change in the probability of a dependent being enrolled in a given year relative to the change for employees. The coefficients on these interaction terms are analogous to the differences between the employee reenrollment and dependent reenrollment rates in Figure 2. In short, we are estimating a differences-in-differences model, in which the impact of the DV program is measured by the coefficient on *dependent*$_i$ x *2010*. A negative significant coefficient would indicate that the program was in fact curbing the re-enrollment of dependents who were ineligible but nonetheless were enrolled prior to the program.

The other covariates in X_{ijt} include individual *i*'s age and gender, and the employed family member's age, gender, salary (8 quantiles), staff group (8 categories), and business unit (3 categories).[14] The term ε_{ijt} is a random error; standard errors are clustered at the family level, which subsumes clustering observations for individuals across the panel.

Because the model does not assume linear time trends, even this basic specification allows for falsification tests. Specifically, a violation of our identifying assumption would show up as a non-zero coefficient on any of the interactions between dependent status and the 2009, 2011, and 2012 year effects (coefficients λ_{2009}, λ_{2011}, and λ_{2012}). That is, none of the coefficients on these variables should be significantly different from zero.

[14]This set of categories is more detailed than what we report in the summary statistics for tractability and in order to preserve the Employer's anonymity.

A slightly more complex specification allows us to take advantage of the panel nature of the data as well as to exploit the fact that we can identify individuals in the same family. This specification contains a family fixed effect interacted with each year effect:

$$re\text{-}enroll_{ijt} = \alpha_0 + \gamma * dependent_i + \sum_{t=2009}^{2012} \delta_t * Year_t + \sum_{t=2009}^{2012} \lambda_t * dependent_i \times Year_t + \boldsymbol{X_{ijt}\beta} + \sum_{t=2009}^{2012} \Delta_{jt} * family_j \times Year_t + \varepsilon_{ijt}$$

The coefficients on the family-by-year effects, Δ_{jt}, reflect any enrollment patterns that are common to all family members in any given year. The inclusion of the family-by-year effects thus ensures that the coefficient of interest (on *dependent*$_i$ x *2010*) captures only deviations of the enrollment of the dependent from the rest of the family, including and especially the employee.

4.2 Basic results.

The basic results are reported in Table 2. Column (1) shows the estimates when the only variables on the right hand side are the time effects, the dichotomous variable for dependent status (*dependent*$_i$), and their interactions. Column (2) adds the control variables; column (3) adds the family fixed effects, and column (4) adds interactions between the family and time effects. The results are remarkably similar across the various specifications, and can be summarized as follows. 1) The coefficient on *dependent*$_i$ x *2010* is between -0.023 and -0.027, depending on the specification, and in all cases it is significant at the 0.001 level. This 2.3 to 2.7 percentage point reduction in the probability of being enrolled (corresponding to around 120 to

140 individuals) is consistent with the notion suggested by Figure 2: the DV program succeeded in reducing the number of ineligible dependents enrolled. Although this estimate is lower than those reported by some organizations, it is in line with figures from others. It is, for example, just about equal to the proportion of ineligibles that an audit at the University of Nebraska claimed to have turned up several years ago (Lechner [undated]). 2) On the other hand, the coefficients on the interactions between $dependent_i$ and the time effects other than the year in which the DV plan was introduced are all insignificant. The coefficients on $dependent_i$ x *2009,* $dependent_i$ x *2011,* and $dependent_i$ x *2012* represent three opportunities to falsify the identifying assumption that dependent re-enrollment trends did not diverge from employee re-enrollment trends, except during the policy change year. Hence, this finding that they are not statistically different from zero (except for a single coefficient on $dependent_i$ x *2012* in column (3), which is not significant in any other specification) lends credibility to our identification strategy. 3) The coefficients on the time effects in columns (1) and (2) are small in magnitude and generally insignificantly different from zero. Thus, for employees there is no overall trend in reenrollment over time. In the column (3) specification these effects are significant, but even so there is no jump in 2010, implying that the policy change had no detectable impact on the enrollment of employees themselves. 4) The coefficients on $dependent_i$ indicate that, in general, dependents are 2 or 3 percentage points less likely to reenroll than employees. The higher turnover in enrollment for dependents is unsurprising, because spouses may find insurance through their own employers and may take children with them, and some fraction of enrolled children naturally age out of coverage eligibility every year. Although not unexpected, it is reassuring to see this intuitive result.

4.3 Results for subgroups.

Our basic model assumes that the effect of the DV program was the same across all types of dependents, regardless of their characteristics and the characteristics of the employees with whom they were associated. We now explore the outcomes when this assumption is relaxed.

Differences by type of dependent. Some dependents are spouses; others are children. To allow for different responses to the introduction of DV, we create the indicator variables $child_i$, which equals one if the dependent was a child, and $spouse_i$, which equals one if the dependent was a spouse. We include these variables (instead of $dependent_i$) in our basic regression, as well as interactions between them and the time effects. The results are in column (1) of Table 3. For brevity we report only the coefficients on the interaction terms. The results suggest that the disenrollment effect is about twice as strong for children as for spouses—2.9 percentage points for children versus 1.5 percentage points for spouses. Both are statistically significant at the 0.05 level.[15]

We next examine whether the effect of DV varies with the dependent's age. To do so, in column (2) we augment our model with triple interactions between the *child x [year]* variables and an indicator for turning 24 before the end of the plan year. While there is inevitably some arbitrariness in choosing an age cutoff, 24 years seems a natural threshold, given that, as indicated in Figure 1, it was the age at which the eligibility criteria became additionally

[15] In a model that includes all controls and family fixed effects, the difference between the child and spouse coefficients is significant (p = .01). In a model that includes family-by-year effects, the magnitude of the difference is similar but the standard errors are larger, rendering the difference insignificant (p = .14.)

stringent.[16] The additional interaction terms mean that the *child* x *[year]* coefficients in column (2) have a different interpretation from their counterparts in column (1), as the former reflect the enrollment trends of younger children only.

The results indicate that the effect of the program among younger children was small and not statistically significant. Among older children, however, the program impact was dramatic. Re-enrollment among children 24 and older declined by 19.5 (=18.6 + 0.9) percentage points in the year of the reform. On this basis, we think that the most likely explanation for disenrollment among children is not that employees were falsely claiming that (say) nieces and nephews were their children, because this could happen at any age. An alternative explanation is that employees were lying about the ages of their older children in order to meet the age requirements. However, staff from the Employer's Human Resources department told us that misrepresenting a child's age was highly unlikely, because the date of birth in the Employer's records was routinely cross checked against claims submitted by health providers. Another possibility is that the compliance costs of verifying dependent status were particularly high for older children, but this seems unlikely as well. For younger children, parents were required to provide birth certificates and redacted tax forms; the incremental effort for older children would generally involve nothing more than presenting a tuition bill or signing a statement attesting to the child's status. We conclude that the most likely reason for the large effects among older

[16] In any case, the substantive result that effects are larger among older children is not sensitive to the choice of age cutoff, although the effect is strongest at age 24, supporting the importance of the additional eligibility criterion at age 24. See the online appendix, in which we provide estimates with the age threshold for the interaction set to each age between 18 and 26.

children is that they did not meet the various criteria listed in Figure 1 that pertained specifically to older ages. That is, older children were probably ineligibly enrolled because they were not students, they were not living at home with the parent, and/or the parent wasn't providing at least half of the child's support.[17]

As an aside, we note that the Affordable Care Act of 2010 mandated that beginning in 2011, all children be eligible for inclusion on their parents' health plans until the end of the year at which the child turns 26, regardless of the child's marital status or whether the child is financially supported by his parents. The impact of this provision of the ACA is evident in column (2), where the coefficient on *child* x *older* x *2011* implies a 14.9 percentage point *increase* for children 24 and older. We return below to discussing the interactions between the ACA and DV programs.

Differences by type of employee. We next explore whether the disenrollment effects differ across groups of employees. Table 4 shows parameter estimates for subsamples based on employee's income and type of job. We first explore whether the effects of DV differ between higher-earning (above the 25th percentile) and low-earning (below the 25th percentile) employees. Comparing the figures in columns (1) and (2), we see that the point estimate of the disenrollment effect is much larger for families in which the employee's earning are below the 25th percentile (4.4 percentage points) than above it (1.8 percentage points). The difference between the two groups is marginally statistically significant (p=.10). Other income cutoffs produce qualitatively

[17] Unlike the case for children, triple interactions between the spouse's age and the *spouse x [year]* variables (reported in the appendix) suggested no robust differences in program impacts for older versus younger spouses.

similar results: thresholds set at the 10th, 25th, 40th, or 50th percentiles yield larger effects for the lower income group, but in all cases other than the 25th percentile, the differences by income are not statistically significant.[18]

In columns (3) and (4), we display results by job type. For tractability, we collapse the 3 business units and 8 staff groups into 2 job types, "maintenance/service" and "other," a category that includes professionals, managers, and support staff. The most striking finding is the size of the disenrollment effect for families in which employees are classified as maintenance/service. The program decreased re-enrollment among this group by 6.7 percentage points, an effect that is statistically different and about four times larger than for other types of employees.[19] It is not immediately clear why the disenrollment effects should be so large for maintenance and service workers. Because the regression controls for income, the fact that maintenance and service workers have relatively low incomes is probably not the explanation. The Employer's Human Resources staff pointed out to us that many of these employees are not native speakers of English, and conjectured that prior to the DV program, these workers simply might not have understood fully the criteria for putting a dependent onto one's health insurance policy.

Finally, in Table 5 we examine the differences between the impact of the DV program on spouses and partners in same-sex versus opposite-sex couples. Because the models with family-

[18] All percentiles are approximate. All computations assessing statistical significance in this paragraph are based on specifications in which all observations are pooled and the income cutoffs are interacted with *dependent*2010*.

[19] Further subdividing the non-maintenance workers in column (4) into more detailed subgroups yields similar coefficients to those in column (4), but they are estimated with less precision.

by-year effects are less precisely estimated, we include specifications both with and without them. The results indicate that disenrollment of spouses/partners is much larger among same-sex couples (14.0 percentage points) than among opposite-sex couples (1.7 percentage points), and the difference is statistically significant. Why was there such a large impact upon same-sex couples? Prior to the implementation of DV in 2010, employees who wanted to include a same-sex partner or spouse on their policy had to sign an affidavit attesting that all the qualifications (as outlined in Figure 1) had been met. The Employer reserved the right to check the relevant documentation, but in practice, never did so. However, under the DV program, the documents were actually checked. Apparently, a substantial proportion of same-sex relationships did not meet one of more of the qualifications, so these partners and spouses were dropped from the policies.

5. Mechanisms and Implications

5.1 Spillovers

A DV program might have impacts beyond the targeted ineligibles. In particular, there are at least three ways in which the program could have affected the enrollment decisions and plan choices of *eligible* employees and dependents. First, employees who could no longer enroll their ineligible dependents in the Employer's plans might have chosen to switch to a spouse's employer plan that accepted the barred dependent, in order to keep their family under a single

plan. However, Panel B of Figure 2 suggests this was not the case—there was no discernible change in employee re-enrollment patterns when DV was introduced.[20]

Second, the policy might have induced eligible enrollees to switch plans within the menu offered by the Employer. Because all family members must be on the same plan (HMO, PPO, or POS), removing an ineligible dependent could change a family's relative valuations of the plans by altering the composition of the family members who would be covered by them. We investigated this possibility by comparing plan-switching behavior of employees who enrolled at least one dependent in the prior year with that of employees enrolling only themselves, since the latter group could not be affected by this mechanism.[21] There was no significant difference between the two groups, again providing no evidence of unintended spillovers.

Third, even if the employee remains enrolled, families may have adjusted to the policy by removing both eligible and ineligible dependents to a spouse's plan at another employer. This would be particularly appealing for spouses whose employers do not charge incremental premiums for family members added to a family plan. This issue is discussed in the next subsection.

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[20] Supplemental regressions (available in the online appendix) confirm that there were no significant differences in re-enrollment between employees with and without dependents, offering no evidence of enrollment spillovers to employees.

[21] Specifically, we estimated a linear probability model in which the left hand side variable was an indicator for whether the employee changed plan choice, and the right hand side included the interactions *employee* x *[year]* as well as triple interactions of *employee* x *had dependents* x *[year]*.

5.2 Is Disenrollment a Symptom of Cheating or Compliance Costs?

So far, we have shown that the dependent verification program succeeded in reducing the number of enrolled dependents without causing the disenrollment of eligible employees. But this begs the question of whether these dependents were truly ineligible or if employees were simply discouraged from enrolling eligible family members because of the costs of finding and presenting the required documentation. It seems unlikely that any such compliance costs could exceed the anticipated benefits of enrolling legitimate dependents, since the average payout of the insurer on behalf of each enrollee was several thousand dollars. Nonetheless, some critics of DV programs have argued that they are intended specifically to discourage legitimate dependents from enrolling. A commenter on one online article opined that DV programs "disqualify legitimate dependents because they know that 5 to 8 % of employees will not read the paperwork or do anything about the audit until it is too late to fix it" (Hobson [2010]).

To investigate this issue, we begin with the following proposition: If an employee with several dependents incurs the compliance costs of re-enrolling one or two of them, then the marginal cost of enrolling an additional dependent is probably small. Consider, for example, an employee who enrolled a spouse and a child in 2009. To re-enroll the spouse in 2010, the employee would have to bring a marriage certificate and a redacted 2008 tax return to Human Resources. In order to re-enroll the child, the incremental paperwork would consist of only a birth certificate. No additional trip to the Human Resources department would be needed, and no additional tax document would be required, because the same 2008 tax return would serve to verify both the spouse and child. Therefore, if we were to observe an employee dropping one dependent while re-enrolling the other, it would provide some evidence against the compliance

costs story. Likewise, an employee frustrated or confused by the DV program would probably not choose to re-enroll only a subset of his family members.

In short, we can assess the importance of compliance costs by examining whether employees with several dependent family members dropped all or only some of them at the time of the policy change. To do so, we construct a sample that is limited to families with two or more enrolled dependents in the previous year, and then re-configure the data so that the unit of observation is a family-year (as opposed to a person-year, which is the case in the analyses presented up to now). We next use this sample to estimate a series of linear probability models of family-level re-enrollment. In our first regression, the left-hand-side variable takes a value of one if the employee drops all of his or her dependents in the plan year. The right hand side variables include year effects and our standard set of employee-level covariates. If employees chose to re-enroll none of their dependents because of frustration or high compliance costs, we would expect a positive coefficient on the indicator for 2010, the year when the program was introduced. In the second regression, the left-hand-side variable takes a value of one only if the number of dependents re-enrolled falls to some level above zero, and in the third, the dependent variable equals one only if exactly one dependent is dropped. The right hand side variables are the same as in the first model. If employees re-enroll some but not all of their dependents, we would expect a positive coefficient on the 2010 indicator in these latter two models. Such a finding would be consistent with the notion that compliance costs are not an important consideration.

The results are reported in Table 6.[22] The three columns correspond to the three models just described. In column (1), the coefficient on the indicator for 2010 is a precisely estimated zero, suggesting that the DV program had no discernible impact on the likelihood of employees dropping *all* of their family members. Columns (2) and (3) show that there was a significant jump in the probability of dropping some (but not all) dependents in 2010, compared to the years preceding and following. Taken together, the results in Table 6 indicate that it was not the inconvenience of gathering and delivering documentation to the Human Resources department that drove the decline in dependent re-enrollment we report above, because the employees who dropped a dependent in response to the policy change did, in fact, gather and deliver verification documents for their *other* family members.

Table 6 also provides some evidence that the program didn't cause spillover disenrollment among eligible dependents. Such an effect might occur, for example, if barring an ineligible child from enrollment induced an *eligible* spouse to decline coverage in order to enroll the child in his or her own employer's plan. Similarly, among families in which the spouse already had coverage from his or her own employer, if the family could move the ineligible child dependent to the spouse's plan, it would provide a strong incentive to move the remaining children to the spouse's plan as well, since the marginal cost of adding a second or third child to a family health plan is often zero (and the marginal savings of removing the remaining children from the Employer's plan would have been substantial). Column (1) of Table 6, which shows that

[22] The coefficients in this table are not directly comparable to others in the paper because the unit of observation is a family-year rather than a person-year.

employees were no more likely to remove their entire set of dependents from coverage in the policy change year than in other years, suggests these effects were not taking place.

5.3 Cost savings

So far, our focus has been on measuring the reduction of ineligible dependents induced by the DV program. Assessing the benefits of the program to the Employer, however, requires information about the associated decline in outlays for health care claims. The promoters of DV programs assert that these reductions are large. As one news report said, "Audit firms say companies are often surprised by the savings…[T]he average annual health-care cost is about $3,000 per dependent" (Epstein and McGregor [2007]). In this context, an important question is whether the dependents who were dropped from coverage due to the program were more or less expensive than average. One might expect that ineligible enrollees would be relatively expensive, since the value of illegitimately enrolling in a plan is highest for exactly those people who face high expected medical costs.

The Employer was able to provide us with data on costs only for plan year 2009, so we cannot follow our basic difference-in-difference strategy to estimate whether dependents who disenrolled in 2010 were higher cost than in other years. Instead, to investigate this issue, we simply compare the average claim amounts (payouts by the insurer on behalf of the enrollee) of 2010 renewers and non-renewers in the year prior to the change, 2009.[23] It is important to

[23] More precisely, cost differences are estimated from a regression of plan costs on re-enrollment status interacted with indicators for spouse and child, and in the case of children, further interacted with an indicator for age between 22 and 25.

understand the limitations of this exercise: The set of children and spouses who did not re-enroll in 2010 includes both ineligible dependents who were screened out by the DV program *and* other dependents who would have naturally aged out of eligibility or left coverage for other reasons. More precisely, our estimates imply that of all the children 24 or older who did not renew enrollment in 2010, only 57 percent were dropped as a result of the DV program and the rest left for other reasons. Likewise, only 32 percent of the non-renewals among spouses/partners can be attributed to the DV program.

With these caveats in mind, we compute that among children 24 to 25[24], the average healthcare claims in 2009 of those who were not re-enrolled in 2010 were $903 *lower* (p=.09) than of those whose coverage continued across the 2010 policy change. Similarly, among spouses and covered partners, the average costs for those who left coverage in 2010 were lower by $1410 (p=.13) than for those who remained enrolled. Thus, we find no apparent evidence that ineligibles tended to incur higher costs. Interestingly, this suggestive evidence that dependents who re-enrolled had larger health costs than those who did not is consistent with the notion of "adverse retention"—the tendency of individuals who remain in a plan to reinforce cost differentials between plans (Altman, Cutler and Zeckhauser [1998]).

In any case, even if ineligible dependents did not generate disproportionately large healthcare costs, their consumption of *any* health benefits nonetheless produces costs for both employers and employees (in proportion to the incidence of health insurance costs on each). To provide a rough gauge of the size of the total cost savings, we take the average 2009 cost of dependents

[24] We focus on children 24 and older because we found no significant disenrollment effects of the DV program on younger children.

and multiply it by our implied estimate of the number of ineligible dependents who were disenrolled due to the DV program.[25] Doing so yields a total savings of $233,900 in 2010, which amounts to $46 per enrolled employee.[26]

5.4 Welfare Issues

We have shown that the DV program reduced the number of ineligible dependents on insurance policies, but this is far from saying that it improved social welfare, the calculation of which involves costs and benefits accruing to a range of agents. First consider the potential winners from the policy. Employers that institute DV plans are better off because their expenditures for health benefits decrease. To the extent that the incidence of these expenditures falls on employees, the employees who were not carrying ineligible dependents share this gain. However, any such benefits must be net of the costs of administering the plans. Although the Employer examined in this paper was not able to provide us with a figure, other sources have suggested costs of about $20 to $30 per employee [Epstein and McGregor [2007]]. [27]

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[25] These figures are based on a regression similar to our main specification, but allowing separate coefficients for the effects on spouses and on children above and below age 24. We then multiply the implied disenrollment effects by the average 2009 costs of enrollees in each of these three groups.

[26] Alternatively, one could multiply the implied disenrollment effects by the average 2009 costs *among only the enrollees who do not re-enroll in 2010*. Doing so yields a total savings of $185,800 in 2010 , or $36 per enrolled employee.

[27] Though Epstein and McGregor [2007] are not specific on this point, these are likely the costs for each verification procedure, and would recurr whenever an employer chose to re-verify an

Who are the potential losers? If the dependents whose coverage is dropped remain uninsured, then even in the absence of illness they (and their families) experience a decline in utility because of the increased uncertainty they face. If an uninsured dependent does become ill, someone has to pay the cost—the dependent, the employee, taxpayers (if the dependent ends up on Medicaid), or a health care provider (if no one else will pay). If the dependent is put on a spouse's policy, then the spouse's employer will face larger health care expenditures. As before, depending on the incidence of employee health insurance expenditures, some of the burden may be shifted to employees in the form of lower wages.

Two other possible effects on welfare are worth noting. First, consider adults who respond to their ineligible status by signing up for health insurance through their own employers. Efficiency is reduced to the extent that "job-lock" hinders their mobility in the labor market (Monheit and Cooper [1994]). Second, while under a minimal set of assumptions it is true that less insurance reduces an individual's utility, we are operating in a second-best world in which employees and their families are likely over-insured because of the implicit subsidy to employer-provided health insurance embodied in the personal income tax. Hence, efficiency could be enhanced to the extent that ineligible dependents end up with more modest insurance coverage.

In short, a complete calculation of the impact of DV plans on social welfare requires that a variety of effects be taken into account. To our knowledge, there is no data set that would allow one to undertake an analysis like this. It is clear, though, that the reduction in employers' health insurance costs would play a central role in any such a calculation.

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employee's dependents. Some employers, including the one we study, authenticate their entire dependent population once, and then check only new dependent enrollees

5.5 The Affordable Care Act

The Affordable Care Act (ACA) mandated that beginning in 2011, all employer-sponsored health insurance must offer coverage to children up to age 26. No other criteria can be set in order to qualify. Evidence from the CPS indicates that the number of uninsured 19-25 year-olds fell by 7.4 percent from 2010 to 2011 (DeNavas-Walt, et al. [2012]).

The Employer's health care plan was grandfathered; it could have kept the previous age cutoff in place beyond the beginning of 2011. Nevertheless, the Employer chose to become ACA-compliant effective September 1, 2010, and offered a special enrollment period at that time.[28] As Figure 2 shows, this pattern is consistent with our data—dependent re-enrollment shows an uptick in 2011 (one year after the implementation of the DV program) and then a reversion in 2012 back to the pre-program trend. The reason for the one-time spike is that prior to 2011, the Employer covered children up to age 25. The ACA extended coverage to age 26 in 2011. Therefore, for 2011 only, there was a new inflow and retention of 26 year-olds without any outflow of 27 year-olds, creating a high net re-enrollment rate. In 2012, the outflow of 27 year-olds began, generating steady-state re-enrollment rates similar to the pre-2010 trend.

An alternative, though we believe incorrect, interpretation of the 2011 spike in re-enrollment is that it is simply due to a compositional effect—the rolls were purged of ineligible dependents, so that by definition the members of the remaining population were more likely to be eligible for re-enrollment. However, because all new dependents were screened for eligibility in every year

[28] In the weeks and months before September 1, there was no slackening in enforcement of the pre-ACA age cutoff.

after 2010, a compositional effect would have led to a steady-state increase in the re-enrollment rate that continued into 2012, not the spike that we actually observe for 2011 only.

As further evidence that the 2011 increase in re-enrollment was caused by the ACA, the estimates in Table 3 suggest that this change is driven entirely by older children: There is a large positive coefficient on *child* x *older* x *2011*, but no significant effects on the corresponding coefficient for younger children, and no significant effects on *spouse* x *2011*. All of this is consistent with a story in which 25-year-olds who otherwise would have aged out of the plan, or would have exited because they were done with full-time schooling, could instead re-enroll for an additional year because of the ACA.

Because some of the largest DV program effects were among older children, the ACA has muted the potential cost savings of the program. Based on the cost calculations above, about 43 percent of the cost savings were obtained by excluding employees' children who failed to meet some other dependency criterion. Hence, because the ACA rendered inoperative all employers' restrictions on dependent eligibility for children up to age 26, a significant fraction of the potential costs savings were erased. While we are not aware of any systematic surveys of the types of restrictions US employers placed on child dependents prior to 2011, our conversations with the Employer's benefits administrators lead us to believe that restrictions on the eligibility of older children were common. Hence, our finding is likely broadly applicable because, in effect, the ACA redefined a large set of formerly ineligible dependents as eligible under *all* employer-sponsored insurance plans. Further, children usually represent an even larger fraction of the enrolled ineligibles than we find in our setting (Silva [2008]). If so, this effect of the ACA on the cost-savings of DV programs will be widespread and substantial.

6. Conclusions

In order to reduce their health care costs, employers across the country have introduced dependent verification programs, whose purpose is to remove ineligible dependents from employees' insurance policies. As far as we know, this paper is the first attempt to assess the efficacy of such a program. Using panel data from a large, single-plant U.S. employer, we implement a differences-in-differences econometric strategy to estimate the effect of a dependent verification program that the Employer implemented in 2010. We find that the program reduced re-enrollment among dependents by 2.7 percentage points. The effect was particularly strong for children in their mid-20s, who likely did not meet the various conditions to qualify them as being dependent on the employees. Re-enrollment among same-sex partners and spouses fell by about 13.6 percentage points, presumably because they did not meet all the Employer's criteria for a *bona fide* relationship. We find no evidence that disenrollment was caused by the putative inconveniences associated with gathering and presenting the required verification. Rather, the dropped dependents were most likely simply ineligible. We also find no evidence that these policies spilled-over to induce disenrollment or plan switching among eligible individuals within the same families as the affected ineligibles.

Our data did not permit us to make a precise estimate of the cost savings associated with the DV plan, but a rough calculation suggests that it saved the Employer studied here about $46 per employee per year. However, the Affordable Care Act, which bars any enrollment restrictions on child dependents up to age 26, has erased some of the potential gains from DV programs. Because this provision affects all employers and because children who don't meet certain criteria have generally been a ripe target for dependent verification programs, we believe

this effect could be quite widespread. For the employer we study, we estimate the ACA reduced the DV program's potential cost savings by about 40 percent. Additionally, new non-group markets that might arise via the ACA could increase the number of options outside of traditional employer-provided policies, hence reducing the benefits from fraudulently enrolling ex-spouses and other ineligibles in employer plans. These institutional changes, taken together with our estimates of program effects and cost savings, suggest that employers and governments may perceive a weaker case for implementing or continuing dependent verification programs in the future.

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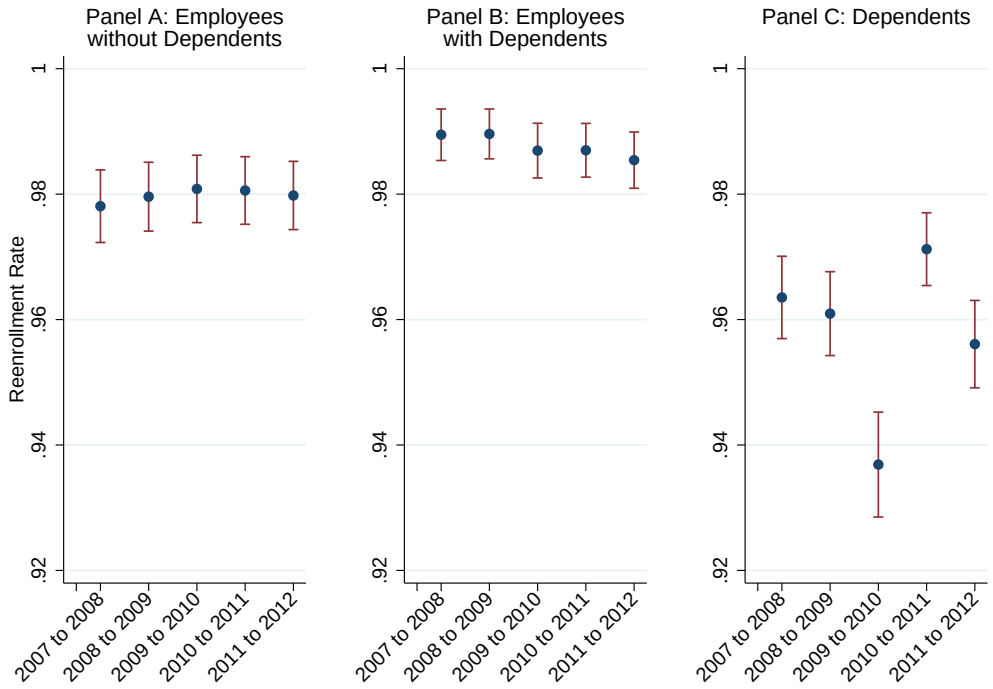
Figure 1: Program Details: Verification Documents

Group	Required Documentation
Spouse	Marriage certificate and previous year tax return
Child or ward under 19	Birth certificate (or adoption/guardianship/foster documentation), and previous year tax return showing child as dependent. Marriage certificate needed for stepchildren.
Child age 19-23 who is a full-time student	Documentation to prove dependency as described above, plus a document from the current school to prove full-time student status (e.g., letter from school or copy of tuition bill)
Child age 19-25 who is not a full-time student	Documentation to prove dependency as described above, and a sworn affidavit certifying that employee provides over half the child's support and the child lives with employee
Civil union partner	Civil union certificate
Same-sex domestic partner	Affidavit of domestic partnership and 3 pieces of supporting documentation (e.g., Qualifying Domestic Partnership Agreement, joint bank account, designation of domestic partner as a primary beneficiary for life insurance, durable property or health care power of attorney, joint ownership of a motor vehicle, co-parenting agreement)

This figure shows details of the DV program introduced by the Employer for open enrollment in the fall of 2009. Because of the Affordable Care Act of 2010, the dependency requirements for children have been eliminated (since 2011). Employees can now enroll children through the end of the year in which the child turns 26 regardless of marital, student, or residential status.

Figure 2: Re-enrollment Patterns: Employees versus Dependents



Each point shows the fraction of individuals who were reenrolled in the health plan in January of year t, conditional on having been enrolled in the plan in December of year $t-1$. The 2009-10 transition is associated with the introduction of the dependent verification program. 95% confidence intervals are generated from a linear probability model in which re-enrollment is regressed on year indicators interacted with indicators for employee and dependent status. Standard errors are clustered at the family level.

Table 1: Characteristics of Employees and Dependents: December Averages over 2007-2011

	(1) Employees	(2) Spouses	(3) Children
Number of Enrollees	5,104	1,975	3,274
Age	45.5	48.4	13.0
Female	0.42	0.68	0.48
Family size (including employee)	2.03		
Plan Type			
HMO	0.34	0.36	0.40
PPO	0.37	0.38	0.35
POS	0.21	0.23	0.23
Job Category			
Professional/Admin/Adv. Degree	0.70		
Maintenance/Service	0.15		
Support	0.16		
Salary			
\$0-37k	0.12		
\$37-46k	0.14		
\$46-54k	0.13		
\$54-63k	0.13		
\$63-76k	0.13		
\$76-97k	0.13		
\$97-145k	0.13		
\$145k+	0.08		
Re-enrolled next Jan (2 months)	0.98	0.96	0.95

The unit of observation over which means are taken is the person-year—if a person was enrolled in the health insurance plan three different years, then he or she counts as three observations. The first column shows figures for employees only; the second and third columns are only for spouses and children, respectively.

Table 2: Re-enrollment of Dependents Across the 2010 Policy Change: Main Specification

	(1)	(2)	(3)	(4)
dependent × *2009*	-0.003	-0.003	-0.006	-0.003
	(0.005)	(0.005)	(0.004)	(0.006)
dependent × *2010*	-0.027***	-0.027***	-0.027***	-0.023***
	(0.005)	(0.005)	(0.005)	(0.006)
dependent × *2011*	0.007	0.007	0.005	0.009
	(0.004)	(0.004)	(0.004)	(0.005)
dependent × *2012*	-0.006	-0.006	-0.009*	-0.003
	(0.004)	(0.004)	(0.004)	(0.005)
dependent	-0.020***	-0.021***	-0.029***	-0.026***
	(0.003)	(0.004)	(0.003)	(0.004)
2009	0.001	0.000	-0.011***	
	(0.003)	(0.003)	(0.002)	
2010	0.000	-0.000	-0.018***	
	(0.003)	(0.003)	(0.002)	
2011	0.000	-0.000	-0.019***	
	(0.002)	(0.002)	(0.002)	
2012	-0.001	0.001	-0.027***	
	(0.003)	(0.003)	(0.003)	
controls		✓	✓	✓
family FEs			✓	✓
family × year effects				✓
Observations	51772	51772	51772	51772

The coefficients are generated by linear probability models in which the left hand side variable is one if the individual was enrolled in the health care program in January of year t, conditional on having been in the program in December of year t-1. The sample includes up to 5 enrollment transitions for each person: December 2007 to January 2008, December 2008 to January 2009, December 2009 to January 2010, December 2010 to January 2011, and December 2011 to January 2012. Controls include the individual's own age and own gender, and the employed family member's age, gender, salary (8 quantiles), staff group (8 categories), and business unit (3 categories). Family fixed effects in column 3 are subsumed in column 4, which includes family-year interactions. Column 3 does not include the employed family member's age due to collinearity between age, employee fixed effects, and year effects. Standard errors in parentheses are clustered at the family level, allowing correlation of enrollment decisions across time and correlation among the members of each family. (*) indicates that the coefficient is significant at the 0.05 level; (**) at the 0.01 level; and (***) at the 0.001 level.

Table 3: Differences between Spouses and Children

	(1)	(2)
spouse × *2009*	-0.001	-0.001
	(0.007)	(0.007)
spouse × *2010*	-0.015*	-0.015*
	(0.007)	(0.007)
spouse × *2011*	0.001	0.001
	(0.006)	(0.006)
spouse × *2012*	-0.003	-0.003
	(0.006)	(0.006)
child × *2009*	-0.005	-0.005
	(0.008)	(0.006)
child × *2010*	-0.029***	-0.009
	(0.008)	(0.006)
child × *2011*	0.015*	0.008
	(0.006)	(0.005)
child × *2012*	-0.003	0.002
	(0.007)	(0.006)
child × *older* × *2009*		0.029
		(0.059)
child × *older* × *2010*		-0.186**
		(0.064)
child × *older* × *2011*		0.141**
		(0.052)
child × *older* × *2012*		0.057
		(0.051)
controls	✓	✓
family × year effects	✓	✓
Observations	51772	51772

In these models, the effects for dependent spouses and children are allowed to differ and are estimated via separate interaction terms. The coefficients are generated by linear probability models, with the sample and control variables defined as in Table 2. Year effects and main effects for spouse and child are included in both models. In addition, the specification in column (2) includes interactions between *older* and the *child* × *[year]* variables, where *older* is a a dichotomous variable equal to one if the child attained age 24 prior to December 31 of the plan year, and zero otherwise. Standard errors (in parentheses) are clustered at the family level, allowing correlation of enrollment decisions across time and correlation among the members of each family. (*) indicates that the coefficient is significant at the 0.05 level; (**) at the 0.01 level; and (***) at the 0.001 level.

Table 4: Differences by Employee Income and Job Type

	(1)	(2)	(3)	(4)
	Employee Income		Job Type	
	$< 25^{\text{th}}$ pctile	$> 25^{\text{th}}$ pctile	Maintenance/ Service	All Others
dependent × *2009*	-0.004	-0.003	-0.015	-0.001
	(0.015)	(0.006)	(0.014)	(0.006)
dependent × *2010*	-0.044*	-0.018**	-0.067***	-0.015*
	(0.019)	(0.006)	(0.019)	(0.007)
dependent × *2011*	0.013	0.008	0.012	0.009
	(0.013)	(0.005)	(0.011)	(0.006)
dependent × *2012*	0.000	-0.004	-0.014	-0.001
	(0.015)	(0.006)	(0.014)	(0.006)
controls	✓	✓	✓	✓
family × year effects	✓	✓	✓	✓
Observations	11332	40440	7637	44134

This table shows how the coefficient estimates vary by the employee's income and job type. Each column shows the results when the basic model is estimated using a different subsample. The coefficients are generated by linear probability models in which the left hand side variable is one if the individual was enrolled in the health care program in January of year t, conditional on having been in the program in December of year t-1. The sample includes up to 5 enrollment transitions for each person: December 2007 to January 2008, December 2008 to January 2009, December 2009 to January 2010, December 2010 to January 2011, and December 2011 to January 2012. Year effects and main effects for spouse and child are included in every model. Controls include the individual's own age and own gender, and the employed family member's age, gender, salary (8 quantiles), staff group (8 categories), and business unit (3 categories), except in specifications in which the sample is stratified by the respective covariates. The job categories presented in columns (3) and (4) are coarser than the the business unit and staff group controls that were made available to us in the interest of protecting the Employer's anonymity. Family fixed effects interacted with each year are included. Standard errors in parentheses are clustered at the family level, allowing correlation of enrollment decisions across time and correlation among the members of each family. (*) indicates that the coefficient is significant at the 0.05 level; (**) at the 0.01 level; and (***) at the 0.001 level.

Table 5: Same-Sex and Opposite-Sex Couples

	(1) Opposite-Sex	(2) Opposite-Sex	(3) Same-Sex	(4) Same-Sex
spouse × *2009*	-0.006	-0.002	-0.030	-0.040
	(0.005)	(0.006)	(0.040)	(0.051)
spouse × *2010*	-0.017**	-0.013*	-0.140*	-0.138
	(0.005)	(0.006)	(0.065)	(0.090)
spouse × *2011*	-0.005	-0.001	-0.007	-0.001
	(0.005)	(0.005)	(0.018)	(0.022)
spouse × *2012*	-0.009	-0.004	0.002	0.011
	(0.005)	(0.005)	(0.017)	(0.016)
controls	✓	✓	✓	✓
family fixed effects	✓	✓	✓	✓
family × year effects		✓		✓
Observations	33189	33189	451	451

This table shows how the coefficient estimates vary between same-sex and opposite sex couples. The sample is limited to families that include a spouse or domestic partner. Columns (1) and (2) are estimated using a subsample of families with opposite-sex spouses/partners. Columns (3) and (4) are estimated using a subsample of families with same-sex spouses/partners. The coefficients are generated by linear probability models in which the left hand side variable is one if the individual was enrolled in the health care program in January of year t, conditional on having been in the program in December of year t-1. The sample includes up to 5 enrollment transitions for each person: December 2007 to January 2008, December 2008 to January 2009, December 2009 to January 2010, December 2010 to January 2011, and December 2011 to January 2012. Year effects and a main effect for child and spouse are included in every model. Child-year interactions are included but not displayed. Controls include the individual's own age and own gender, and the employed family member's age, gender, salary (8 quantiles), staff group (8 categories), and business unit (3 categories). Standard errors in parentheses are clustered at the family level, allowing correlation of enrollment decisions across time and correlation among the members of each family. (*) indicates that the coefficient is significant at the 0.05 level; (**) at the 0.01 level; and (***) at the 0.001 level.

Table 6: Are Valid Dependents Discouraged by Compliance Costs?

	(1) Family drops all dependents	(2) Family drops some but not all dependents	(3) Family drops exactly one dependent
2009	0.001	-0.001	-0.004
	(0.005)	(0.008)	(0.008)
2010	0.005	0.040***	0.033***
	(0.005)	(0.009)	(0.009)
2011	-0.000	-0.025***	-0.023***
	(0.005)	(0.007)	(0.007)
2012	0.006	-0.003	-0.002
	(0.005)	(0.008)	(0.008)
controls	✓	✓	✓
Observations	7657	7657	7657

The linear probability models in this table are estimated with a sample that includes only families with two or more dependents. The unit of observation is a family-year, as opposed to previous tables, in which the unit of observation is a person-year. In column (1) the left-hand-side variable equals one only if the number of re-enrolled dependents falls to zero from a positive number. In column (2) the left-hand-side variable equals one only if the number of re-enrolled dependents falls, but not all the way to zero. In column (3) the left-hand-side variable equals one only if the number of re-enrolled dependents falls by exactly one. The sample includes up to 5 enrollment transitions for each person: December 2007 to January 2008, December 2008 to January 2009, December 2009 to January 2010, December 2010 to January 2011, and December 2011 to January 2012. Controls include the employed family member's age, gender, salary (8 quantiles), staff group (8 categories), and business unit (3 categories). Standard errors are clustered at the family level. (*) indicates that the coefficient is significant at the 0.05 level; (**) at the 0.01 level; and (***) at the 0.001 level.