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The Incentive Effects of Health Insurance on Labor Market Outcomes and SSDI/SSI Utilization

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ABSTRACT

Chapter 1

While there has been a tremendous amount of literature on how Unemployment Insurance (UI) affects unemployed workers' job search behavior, there has been no previous study on how the health insurance premium subsidy affects this behavior. To fill the gap in previous studies, this paper analyzes the impact of the substantial federal health insurance premium subsidy on unemployment duration, using data from the Survey of Income and Program Participation (SIPP). To begin this study, I detail the history of the Consolidated Omnibus Budget Reconciliation Act (COBRA) of 1985 that was established to help keep unemployed workers insured. Furthermore, in order to help unemployed individuals reduce the cost of getting insurance, the American Recovery and Reinvestment Act (ARRA) of 2009 provided a 65% health insurance premium subsidy for workers who lost their jobs involuntarily and elected to take up COBRA coverage. The extended unemployment duration resulting from this premium subsidy is equivalent to the effect of increasing Unemployment Insurance benefits by 56 percent which is equal to $168 per week. In order to identify the subsidy effect on the duration of unemployment, this paper compares the unemployment duration of two different unemployed cohorts who lost their jobs just before and after the end of subsidy eligibility. This study sheds light on the unemployed workers' job search behaviors during the Great Recession and finds that the COBRA premium subsidy causes unemployed workers to significantly increase their unemployment duration by 2.11 months. The empirical results are consistent with the predictions of job search theory. Therefore, these estimates imply that the COBRA premium subsidy is having an important impact on the unemployed workers' job transition behavior.

Chapter 2

This paper analyzes the impact of the Employer Sponsored Insurance (ESI) expansion, part of the 2006 Massachusetts health reform, on Social Security Disability Insurance (SSDI) participation decisions. I exploit the variation across SSDI beneficiaries among married couples to identify the causal effect of the reform. I find that for spouses without ESI, the positive effect of the ESI expansion on SSDI participation is 0.98 percentage points stronger than it is for spouses with ESI. These estimates imply that spouses without ESI increases SSDI beneficiaries by 0.0429% (i.e., the 10.73% of total SSDI beneficiaries increase in Massachusetts after the reform) and is associated with higher SSDI beneficiaries. Moreover, my estimates imply an elasticity of spouses without ESI with respect to SSDI beneficiaries of 0.0913. The calculations suggest that the health reform was more expensive than it might first appear because of an increase in SSDI expenditure.

Chapter 3

This paper analyzes the impact of the Medicaid expansion, part of the 2006 Massachusetts health reform, on Supplemental Security Income (SSI) participation decisions. I exploit the variation across SSI-disabled applicants to identify the causal effect of the reform on the SSI claim rate. My estimates imply that the reform reduces SSI-disabled claims by 0.098% (i.e., the 11.66% of total claims in 2008 in Massachusetts) and is associated with a lower initial SSI claim. These estimates also imply Medicaid-disabled expenditure can save around 1% by attending to small inefficiencies in the current program. However, the calculations suggest that the health reform was not as expensive as it might first appear because of reductions in SSI expenditure.

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Chapter 1.The Impact of the COBRA Premium Subsidy on the Duration of Unemployment: Evidence from the 2009 American Recovery and Reinvestment Act (ARRA)

1. Introduction

In the U.S., most group health insurance is closely tied to employment. As a result, those who are separated from employment generally go uninsured because insurance is both more expensive and less generous in individual insurance markets. This low rate of insurance coverage among the unemployed has brought considerable public policy debate over interventions in insurance markets to increase health insurance access for the non-employed (Gruber and Madrian, 1997).

To help keep unemployed workers insured, the Consolidated Omnibus Budget Reconciliation Act (COBRA) of 1985 requires most employers to offer coverage to former employees for up to 18 months after job termination. However, the COBRA insurance usage rate has been modest because the COBRA costs employees 102 percent of their pre-termination health insurance premium, i.e., the entire premium plus up to 2 percent more for administrative expenses (so employers don't need to share the premium costs). Consequently, many are unable to afford to pay the full premium after their job loss (Lambrew, 2001).

The extent of job loss during the Great Recession was severe and unforeseen; according to the Bureau of Labor Statistics, the unemployment rate climbed up to 9-10 percent between 2009 and 2011. In order to help unemployed individuals reduce the cost of getting insurance, the American Recovery and Reinvestment Act (ARRA) of February 2009 gave a 65 percent health insurance premium subsidy to workers who lost their jobs involuntarily between September 2008 and May 2010, to enable them to continue coverage for up to 15 months through their former employers' plans. The subsidy substantially reduced the cost of health insurance after job loss;

on average, the subsidy amount is about $816 per month for family coverage and $294 per month for single coverage.[1]

However, some studies have found that any intervention by the government to protect the unemployed may distort the employment behavior of unemployed workers (Gruber and Madrian, 1997). Employees do pay for the cost of health insurance, either explicitly through employee premiums or implicitly through lower wages when they are working. After individuals become unemployed they start to pay the full cost of health insurance premiums. The ARRA health insurance premium subsidy was designed to subsidize the 65 percent of the full cost of health insurance for the unemployed. This large subsidy could have a direct and immediate impact on unemployed workers by decreasing their job search efforts, and hence contributing to longer unemployment durations.

While there has been a considerable amount of literature on how Unemployment Insurance (UI) affects the unemployed workers' job search behavior, to the best of my knowledge, there has been no previous study on how the health insurance premium subsidy affects unemployed workers' job search behavior. This study is among the first to estimate the impact of the COBRA premium subsidy on the duration of unemployment. I estimate the relationship between the COBRA premium subsidy and unemployment duration by analyzing data from the Survey of Income and Program Participation (SIPP), the nationally representative panel study of households.

This paper focuses on one hypothesis; based on job search theory, the substantial health insurance premium subsidy will cause the COBRA premium subsidy-eligible unemployed individuals to have longer unemployed spells than the COBRA premium subsidy-ineligible

[1] These numbers are based on average monthly premiums for employer-sponsored insurance (ESI) in 2011 $1256 for family coverage and $452 for single coverage (Kaiser/HRET, 2012).

unemployed individuals. The job search theory states that reducing the cost of being unemployed will increase an individual's expected duration of unemployment (Ehrenberg and Oaxaca, 1976). I hypothesize that a premium subsidy would encourage unemployed workers to decrease their job search efforts, thereby contributing to an extension of the duration of their unemployment.

I identify the subsidy effect by the following procedures. First, I re-organize the data into one observation per individual and construct the unemployed cohort by identifying the workers who lost their jobs involuntarily during a particular month. Next, I trace each unemployed individual until he or she obtains his/her first paid job and calculate the duration of unemployment (i.e., how many months they spend on their job search).[2] Finally, I document the effect of the premium subsidy on the labor market outcome by comparing the unemployment duration of two different unemployed cohorts who lost their jobs involuntarily just before and after the end of subsidy eligibility.

After analyzing the data, I find that the COBRA premium subsidy-eligible unemployed workers are predicted to significantly increase their unemployment duration by 2.11 months, conditional on underlying factors influencing their individual preferences for insurance and unemployment insurance (UI) benefits. The extended unemployment duration resulting from the premium subsidy is equivalent to the effect of increasing UI benefits by 56 percent which is equal to $168 per week. This is contrary to Gruber and Madrian (1997), who found that the COBRA has little effect on the duration of unemployment. Two reasons may potentially explain why my study results in different findings. First, the period covered by Gruber and Madrian's study was almost twenty years ago (1983-1989), before the COBRA premium subsidy was

[2] If the unemployed only find a unpaid job through the whole panel, the duration will be recorded at the point when they find a job.

available. Second, their study limited the focus to prime-aged (25-54 years old) males, while this paper studies the whole sample population.[3]

The rest of this paper is laid out in six sections. After giving the background on the ARRA COBRA subsidy in section 2, section 3 discusses the theoretical prediction between the COBRA premium subsidy and the unemployment duration. The data and empirical strategy are summarized in section 4. Section 5 presents the results while section 6 discusses the implications in terms of UI. Section 7 concludes.

2. Background

The COBRA of 1985 offers former employees (and their qualified dependents), who terminate employment for reasons other than gross misconduct, the opportunity to purchase Employer-Sponsored Insurance from their former employers after their employment ends, at a premium not exceeding 102 percent (i.e., entire premium and 2 percent for administrative costs) of the group rate. The COBRA requires employers with 20 or more employees to make health insurance coverage available for up to 18 months after job termination. Many states have extended these provisions to small businesses of 2-19 employees (NCSL, 2009).[4] Employers are required to provide a notice of eligibility for a health plan to ex-workers within 30 days, and the insurance plans must send an election notice within 90 days after the job termination. A former employee has 60 days to elect this coverage from the date the notice was sent (Internal Revenue Service

[3] In SIPP, the labor force status recorded the people who are aged 15 or more.
[4] During the subsidy period, two states expanded COBRA coverage. First, Pennsylvania Act 2 of 2009 was signed into law to give employees of small businesses who receive health insurance from their employers the right to purchase continuation health insurance after they leave employment. Second, Connecticut allowed COBRA coverage up to 30 months on May 2010 (Connecticut Public Act 10-13). This study tests results with and without Pennsylvania and Connecticut and, in both cases, the results are robust.

(IRS), 2009). For employees, their employers usually cover a portion of the premium.[5] The premium under the COBRA is around four times higher for family coverage and six times higher for single individuals compared with when they were employed.

The ARRA, passed on February 17, 2009, subsidized 65 percent of the COBRA premium starting with the very next period of coverage, typically March 1st, for individuals who lost their jobs involuntarily between September 1, 2008 and May 31, 2010 and were ineligible for other group health coverage (e.g., a spouse's plan or new employer's plan) or Medicare (IRS, 2009). The premium subsidy can be drawn up to 15 months. Those who were laid off on or after September 1, 2008 but before February 17, 2009 became eligible for the subsidy retroactively and were given a second chance to elect the COBRA coverage. Employers covered by federal or state COBRA laws were mandated by the ARRA to send notices within thirty days of job termination to ex-workers who had previously enrolled in their employers' group health insurance plan. The employees (and their qualified dependents) were asked to decide whether to take up the subsidized coverage within sixty days. After eligible unemployed workers who take up the subsidy pay 35 percent of premiums, their former employers will pay 65 percent of premiums and receive reimbursement via federal tax credits. If eligible unemployed workers don't report that they or their families are qualified for other group health coverage or Medicare and receive the subsidy, they will need to pay penalties of up to 100 percent of the subsidy received.

3. Theoretical Prediction

[5] On average, 82 percent of the premium costs is paid by the employer for single coverage and 72 percent for family coverage (Kaiser/HRET, 2012).

Mortensen (1977) suggests that the following analysis is applicable when comparing any two groups, one receiving benefits (COBRA premium subsidy), the other not, provided that members of both groups qualify for benefits (COBRA) during an employer-initiated unemployment spell. Specifically, because the potential benefit period per unemployment spell is limited and because qualification is limited to workers who become unemployed due to their employers, unemployed workers receiving no extra benefit have an incentive to become employed more rapidly than would otherwise be the case.

The ARRA COBRA premium subsidy reduces substantial medical costs for the COBRA eligible individuals and hence, may contribute to longer unemployment duration compared to their counterparts. I also test this hypothesis on subsidy-eligible groups of people who are: older (35 or above); educated (some college or above); older and educated; married (spouse present or absent); and the full sample. Next, before I discuss these groups, it is useful to consider two types of costs related to insurance options: monetary cost (covered by 65 percent premium subsidy) and information cost (such as the time it takes to learn that the COBRA premium subsidy is available). In terms of monetary cost, this subsidy will help unemployed individuals with high medical costs (i.e., higher insurance premium) to reduce their unemployed costs substantially. Unemployed workers with higher education may have smaller information costs because they may be more aware of the ARRA subsidy, as well as be able to better comprehend the related policies. Apart from smaller information costs, this subsidy can further reduce unemployed costs for the educated. Consequently, the older educated may have much lower unemployed costs due to smaller information and monetary costs. Lastly, married unemployed individuals may have a longer unemployment spell because the higher the insurance premium costs for their family members, the higher amount of premium subsidy they will receive.

4. Data and empirical strategy

4.1 Data

The impact of the COBRA premium subsidy on the duration of unemployment is examined using data from the 2008 panel of the SIPP, (a longitudinal, stratified, random sample of families in the United States). The SIPP full panel that began in 2008 (the 2008–2012 SIPP) enrolled people from May 2008 through August 2008 and followed them for 52 months, which enables comparisons of the duration of unemployment during and beyond the implementation of the subsidy. The survey uses a 4-month recall period to conduct interviews every 4 months and asks respondents about their employment status and insurance coverage, among other things, in each of the previous 4 months. With monthly information on health insurance status and employment status, the impact of the premium subsidy could be more accurately measured compared to the Current Population Study, which only contains yearly information.

The longitudinal structure, along with the rich demographic and socioeconomic information in SIPP, allows for the tracking of each unemployed individual for a period of four to five years while controlling for underlying factors influencing individual preference for insurance. To identify the unemployed individuals that are eligible for the COBRA coverage, and the subsidy respectively, the person-month format is collapsed into person-record format and the unemployed cohorts are constructed by identifying when they began their unemployment spell. Therefore, the populations that are eligible for the COBRA coverage, and hence the subsidy, could be identified.

Collapsing the data into 126,275 individuals makes the analysis much more manageable than working with the underlying 3.91 million observations. Moreover, it allows researchers to easily understand and control for the individual characteristics involved in choosing the COBRA

subsidy. To identify those who potentially elected the COBRA coverage, the monthly health insurance status and employment status are used as indicators of whether individuals obtain health insurance through their former employers when they become unemployed. Further, those who lost their jobs involuntarily during the implementation of the subsidy could be identified and thus, they are potentially eligible for the COBRA premium subsidy.

Ryscavage (1988) uses two definitions of unemployment: a limited definition and a comprehensive definition.[6] The limited definition is confined to only two scenarios. The first identifies individuals as being unemployed (looking for work or on layoff) the entire month. The second identifies individuals as being unemployed for part of the month and not in the labor force for the rest of the month. The comprehensive definition of unemployment includes two more scenarios. The other two scenarios reflect individuals who, in addition to being unemployed at the same time during the month, also had a job, or had a job as well as some time outside the labor force.

This paper uses Ryscavage's comprehensive definition of unemployment to identify unemployed individuals, instead of the limited definition of unemployment, because it provides more stable results for a large sample. After I reconstruct the data and identify unemployment cohorts for each month separately, from March 2010 to August 2010, the monthly cohorts could be further accumulated into an aggregate sample to obtain better and more precise estimates. In order to check whether the results are robust to the time trend, this aggregate sample is divided into three different cohorts based on the end of subsidy eligibility, May 31st, 2010. The three different cohorts are: May/June = unemployment begins in May 2010-June 2010; April-

[6] In SIPP, the comprehensive definition of unemployment are defined as follows: (1) With job entire month, missed 1 or more weeks because of a layoff; (2) With job part of month, some time spent on layoff or looking for work; (3) No job in month, spent entire month on layoff or looking for work; (4) No job in month, spent part of month on layoff or looking for work. The limited definition of unemployment only consists of persons with (3) and (4).

May/June-July = unemployment begins in April 2010-July 2010; and Mar.-May/June-Aug. = unemployment begins in March 2010-August 2010. Lastly, each unemployed individual is traced until he or she obtained his or her first paid job so that I am able to calculate the duration of unemployment.

4.2 Empirical strategy

In order to distinguish between the subsidy-eligible and the subsidy-ineligible unemployed individuals, this study uses the end of subsidy eligibility, May 31st, 2010, as the cut-off date to estimate the effect of the subsidy. After the ARRA passed in February 2009, the unemployed individuals have an available health insurance premium subsidy if they are separated from their jobs before/to the end of subsidy eligibility. They can reduce their health insurance premiums substantially by taking this subsidy immediately after job termination. Therefore, I compare the unemployment duration of two different unemployed cohorts who lost their jobs just before and after the end of subsidy eligibility to identify the effect of the premium subsidy on the labor market outcome.

The subsidy effect can't be estimated by using the beginning of subsidy eligibility (i.e., September 2008) for two reasons. First, the health insurance premium subsidy is not available for workers who become unemployed during September 2008; they can't foresee the presence of the premium subsidy, which is available 6 months after job loss. Some unemployed workers may make up their mind on a job search plan within a few months after they lost their jobs. Others may also begin looking for jobs right away because of financial burdens (e.g., living expenses, health insurance premiums and so on). Second, unemployed individuals usually find a job within six months after job termination. The six-month gap between September 2008 and February

2009 prevents eligible unemployed individuals from utilizing the subsidy if they become re-employed with health insurance benefits during this period.

This study compares the duration of unemployment during, and beyond, the subsidy-eligible period for a population that is eligible for the COBRA premium subsidy, controlling for a number of characteristics of individuals that could affect preferences for insurance. If the relationship parameter β is estimated by regressing the observed *Duration* on *Subsidy*, the resulting ordinary least squares regression estimator is inconsistent. It will yield a downward biased estimate of the slope coefficient and an upward biased estimate of the intercept. Takeshi Amemiya (1973) has proven that the maximum likelihood estimator suggested by Tobin for this model is consistent.

The basic Tobit models are of the forms:

$$DURATION^{*} = \alpha + \beta * SUBSIDY + \delta * X + u,\ u \sim i.i.d.\ N(0, \sigma^{2}) \quad (1)$$

$$LOG\ (DURATION)^{*} = \alpha + \beta * SUBSIDY + \delta * X + u,\ u \sim i.i.d.\ N(0, \sigma^{2}) \quad (2)$$

$$Duration = \begin{cases} Duration^{*} \text{ if } Duration^{*} < Duration_{ul} \\ Duration_{ul} \text{ if } Duration^{*} \geq Duration_{ul} \end{cases}$$

where $Duration_{ul}$ is the upper limit of duration. The model supposes that there is a latent (i.e., unobservable) variable $Duration^{*}$. This variable linearly depends on *SUBSIDY* via a parameter (vector) β which determines the relationship between the independent variable (or vector) *SUBSIDY* and the latent variable $Duration^{*}$ (just as in a linear model). In addition, there is a normally distributed error term *u* to capture random influences on this relationship. The

observable variable *Duration* is defined to be equal to the latent variable whenever the latent variable is above or equal to the upper limit of duration, and less than the upper limit of duration otherwise. The β coefficient is interpreted as the combination of: one, the change in *Duration* of those above the limit, weighted by the probability of being above the limit; and two, the change in the probability of being above the limit, weighted by the expected value of *Duration* if above.

Using *LOG (DURATION*) as the dependent variable in a model, the coefficient on a dummy variable, when multiplied by 100, is interpreted as the percentage difference in *DURATION*, holding all other factors fixed. The vector of control variables, *X*, represents individuals' demographic and socioeconomic characteristics including age, race, marital status, education level, sex, and number of own children less than 18 years old in the family.

The source of variation in *SUBSIDY* is clear when we collapse the underlying micro-data from 3.91 million observations to 126,275 individuals: the preference for insurance arises from variations across age, race, marital status, education level, sex, and number of own children less than 18 years old in the family. Obviously each of these dimensions is correlated with preferences for insurance, so the regression for every category could be controlled. Therefore, in this specification, identification comes from the variation in the premium subsidy within these groups. That is, this approach uses the fact that the premium subsidy changes for the eligible versus the non-eligible unemployed workers over time.

This is a powerful empirical framework. It allows for differences in individual choice of insurance between the eligible and the non-eligible unemployed workers, and controls for any observable factors which might affect the relative demand for insurance by the eligible and the in-eligible unemployed workers over time. Subsidizing non-employment through the COBRA does raise the likelihood that prime-age males leave their jobs (Gruber and Madrian, 1997).

However, given this era of historically severe recession, very high layoffs, and long lags in finding new jobs, the probability of separating from the COBRA or the COBRA premium subsidy should be minimal.

Another concern is that the COBRA itself may have effects on the duration of unemployment. In theory, we could employ the hazard model to investigate the effect of the COBRA on the unemployment duration directly (Meyer, 1990). In practice, however, the Meyer's model would make it difficult to interpret the findings, since the sample of workers becoming unemployed is itself affected by the availability of the COBRA or even affected by the COBRA premium subsidy. Of course, there are potential concerns that other changes that are correlated with the change in the premium subsidy could bias these estimates. For instance, if those becoming unemployed have disproportionately short spells because of the COBRA or the COBRA premium subsidy, this would bias downward the estimated effect of the COBRA premium subsidy on the duration of unemployment.

Therefore, a more aggregate approach is taken to look at the effect of the COBRA premium subsidy on the duration of unemployment during a six-month period.[7] That is, a sample of unemployed workers is taken from March 2010 to August 2010. After accumulating more monthly cohorts, the estimates will be more reliable and precise and the results are consistent across different cohorts.

5. Results

5.1 Descriptive statistics and figures

Table 1.1 presents the summary statistics for two different unemployed cohorts. One is for the subsidy-eligible unemployed individuals, and the other is for the subsidy-ineligible unemployed

[7] This parallels the approach of Levine (1993) and Gruber and Madrian (1997)

workers. The relatively low fraction of married and relatively high fraction of non-white female workers is indicative of the subsidy-eligible cohorts' characteristics. Across all unemployed cohorts, the mean duration is higher for the subsidy-eligible unemployed workers compared to the subsidy-ineligible unemployed individuals. In addition to the higher average duration for the subsidy-eligible unemployed workers, the COBRA eligible has the largest average duration among all other groups. In terms of mean age and average level of education, there is not much difference between the subsidy-eligible and the subsidy-ineligible unemployed workers. The mean age is close to 40 years old while the average level of education is some college or above.

In order to see how these unemployed workers respond to the subsidy in terms of unemployed spells, figures 1.1 and 1.2 show the spell distributions for the two different unemployed cohorts, the subsidy-eligible and subsidy-ineligible unemployed individuals. From the figures, the shape of these two graphs are similar but the density of spell length for longer unemployed spells are larger for subsidy-eligible group compared to the subsidy-ineligible group.

5.2 Basic regression results

Estimates obtained from Tobit regression support the hypothesis that an intensive health insurance premium subsidy increased the spell duration for the subsidy-eligible unemployed, even controlling for the underlying individual choice of insurance. Furthermore, estimates based upon the other five groups of unemployed workers who are eligible for the subsidy indicates significant, but smaller impacts of the premium subsidy on the duration of unemployment. Table 1.2 presents the results for all three cohorts from estimating Eq. (1) by controlling the covariates. In this regression, the coefficient on *SUBSIDY, β (subsidy effect),* is positive and statistically significant almost across all three cohorts, and it measures the average difference in the duration

of unemployment between the subsidy-eligible and the subsidy-ineligible unemployed workers, given the same levels of age, race, marital status, education level, sex, and number of own children less than 18 years old in the family.

Table 1.2 has six panels; in all six cases comparisons are made relative to the subsidy-ineligible unemployed workers conditional on underlying factors influencing individual preference for insurance. The results in panel A show that the subsidy had a significant impact on the COBRA subsidy-eligible unemployed workers, which are predicted to increase by between 2.09 months and 2.62 months. The COBRA subsidy-eligible unemployed workers have a much larger behavioral response to the premium subsidy than the other groups because the COBRA eligible group is the main beneficiary of the ARRA COBRA subsidy. These results are consistent with the predictions of job search theory.

Evidence in panels B to E reflects that the subsidy had a significant predicted impact on respective targeted subsidy-eligible groups. In particular, a smaller impact is observed in the full sample of unemployed workers (increased by between 1.08 months and 1.75 months, panel B), the older (age 35 or above) unemployed workers (increased by between 1.02 months and 1.27 months, panel C), the educated (some college or above) unemployed workers (increased by between 1.52 months and 2.38 months, panel D), the older educated unemployed workers (increased by between 1.59 months and 2.15 months, panel E), and the married (spouse present or absent) unemployed workers (increased by between 0.96 month and 1.46 months, panel F).

I provide the following explanations for the observed impact. The reason for the increase in older and married participants could be attributed to the likeliness of having unemployed workers with high medical costs and, therefore, higher insurance premiums subsidized. Next, the educated unemployed workers will have a longer unemployment duration because they will have

much lower unemployed costs due to the subsidy and better comprehension of the complex policies. Lastly, the older educated unemployed individuals may have much lower overall unemployed cost because of both a higher premium subsidy and smaller information cost.

Table 1.3 presents the results for all three cohorts from estimating Eq. (2) by controlling the covariates. In this regression, the coefficient on *SUBSIDY, β (subsidy effect),* is again positive and statistically significant across all three cohorts and it measures the average percentage difference in duration of unemployment between the subsidy-eligible and the subsidy-ineligible unemployed workers, given the same levels of age, race, marital status, education level, sex, and number of own children less than 18 years old in the family.

Table 1.3 has six panels; in all six cases comparisons are made relative to subsidy-ineligible unemployed workers conditional on underlying preferences for insurance. In panel A, the COBRA subsidy-eligible unemployed workers are predicted to significantly increase unemployment duration by between 24.45 percent and 34.76 percent. Results in panels B to F show that the subsidy also had a significant predicted impact on the following subsidy-eligible groups: the full sample (increased by between 13.03 percent and 24.46 percent, panel B), the older (increased by between 12.37 percent and 18.18 percent, panel C), the educated (increased by between 21.72 percent and 35.19 percent, panel D), the older educated (increased by between 20.49 percent and 32.78 percent, panel E), and the married unemployed individuals (increased by between 17.64 percent and 20.11 percent, panel F). These results are also consistent with the predictions of job search theory.

5.3 Unemployment Insurance benefits

Some may argue that the subsidy effect on unemployment duration is partially or completely driven by these Unemployment Insurance (UI) extensions, concerned that UI provides a disincentive to search for work. However, such estimated disincentive effects have typically been small (Katz, 2010 and Card and Levine, 2000). Further, the literature suggests that the UI effect on job search behavior is likely even smaller in recessions.[8] As an example, workers who are eligible for unemployment insurance during the current recession are finding jobs at a nearly identical rate to those who are ineligible (Valletta and Kuang, 2010).

Typically, unemployed workers can receive up to 26 weeks of benefits, as long as they continue to search for work. In 2008, Congress created Emergency Unemployment Compensation (EUC) recognizing that unemployed workers were having a significantly more difficult time finding jobs than in a non-recession climate. Later, Congress extended and expanded the program by providing 100 percent federal funding of Extended Benefits (EB) after the labor market worsened. Individuals are eligible for EB once they exhaust their EUC benefits if their states meet certain unemployment-based triggers. Thus, an unemployed worker could receive up to 99 weeks of coverage in those states with the highest rates of unemployment (Council of Economic Advisers report, 2010).[9]

In order to control UI benefits, an extended version of Eq. (1) and Eq. (2) are estimated:

$$DURATION^{*} = \alpha + \beta * SUBSIDY + \delta * X + \lambda * UI + u,\ u \sim i.i.d.\ N(0, \sigma^2) \quad (1')$$

$$LOG\ (DURATION)^{*} = \alpha + \beta * SUBSIDY + \delta * X + \lambda * UI + u,\ u \sim i.i.d.\ N(0, \sigma^2), \quad (2')$$

[8] See Kroft and Notowidigdo (2010) and Schmieder, von Wachter, and Bender (2010).
[9] See Table A1 for more detail on these programs.

where *UI* is an indicator for how many number of weeks of UI benefits an unemployed individual can receive given the state he or she resides in. Therefore, by controlling UI benefits, this specification can also control the labor market conditions among states since many of the eligible weeks of benefits are determined at the state level by thresholds based on states' unemployment rate. That is, the maximum length of coverage provided by these federal programs is shorter in states with better economies.

Tables 1.4 and 1.5 present the results from estimating Eq. (1') and Eq. (2'), in all three cohorts conditional on underlying preferences for insurance and unemployment insurance benefits varying among states. These results show that the subsidy effect on unemployment duration is robust to unemployment insurance benefits, which further confirms that the subsidy effect on the duration of unemployment is mainly from the ARRA. The evidence in Tables 1.4 and 1.5 is consistent with Gruber and Madrian's study; the insignificant relationship between months of COBRA and the UI maximum benefit indicates that omitted variable bias from not simultaneously modeling the UI system should be minimal (Gruber and Madrian, 1997).

5.4 Robustness checks

5.4.1 Analysis of Subsidy selection by unemployed workers

Due to the availability of the health insurance premium subsidy, workers who are eligible for this benefit if they become unemployed may not financially need their jobs to the same degree as workers who are not eligible because the eligible unemployed will have the cost of unemployment reduced. Therefore, this subsidy may provide a disincentive for potentially eligible workers. For example, the potentially eligible workers may feel less financial pressure/obligation to maintain their employment if they are eligible to receive the subsidy.

Whereas workers who are ineligible for the subsidy may have a financial need to stay employed, and therefore workers who are eligible for the subsidy may be more secure with the threat of unemployment. For instance, an employee, whose primary motivation to work is for the availability of affordable health insurance, may have less incentive to work if a health care subsidy is available outside employment.

Table 1.6 presents the unemployment rate and incoming unemployed workers (UW) rate from February 2010 to August 2010.[10] In order to identify whether there is any selection into the premium subsidy between subsidy-eligible and subsidy-ineligible, I define *UW* as the number of unemployed workers who enter the unemployment pool, and *TU* as the total unemployed workers. Further, I define the UW rate=(UW/TU)·100. Since the unemployment rate (seasonally adjusted) stays roughly unchanged (Bureau of Labor Statistics, 2010), I can use the UW rate to represent the inflow of unemployment. If unemployed workers purposely lose their jobs to obtain the subsidy, the UW rate should be high at/before the end of subsidy eligibility.

During the subsidy-eligible period (February 2010 to May 2010), the coefficients are close to each other, which show that there is no "subsidy selection" behavior happening for potential eligible employed workers. Intuitively, given the severe labor market conditions during this time period, workers would not switch jobs easily for a health insurance premium subsidy. Besides, after the subsidy eligible period, the coefficients are also close to each other from July to August except for June. Two reasons may potentially explain why there is a spike for the UW rate in June. First, the difference of the unemployment rate (not seasonally adjusted) between May and June is 0.3 percent (9.6 percent -9.3 percent) representing 0.46 million unemployed workers. That is, the inflow of unemployment in June will be higher than the inflow of unemployment in May. Second, the difference in UW rate between May and June may depend

[10] These are author calculations using the 2008 SIPP panel.

particularly on seasonal influences, which is why it is important to use a statistical method to remove the seasonal component of a time series (e.g., unemployment rate) to analyze non-seasonal trends.

Therefore, I check whether there is a seasonal component for the UW rate between May and June using the data from different years, 2009 and 2011. Table 1.7 shows that the differences in the UW rate between May and June in 2009 and 2011 are 2.72 percent and 3.82 percent, respectively, which are close to the difference in the UW rate between May and June in 2010 (3.83 percent). Therefore, these results indicate that there are seasonal components happening with a similar magnitude during the May-June time period each year in SIPP. In sum, after taking seasonal fluctuation into account, these results imply that there is no subsidy selection between subsidy-eligible and subsidy-ineligible.

5.4.2 Seasonality check within COBRA eligible group

After checking seasonal components during the May-June time period each year in SIPP, I take one more step to further check whether this seasonality would affect the magnitude for the COBRA eligible group or not by looking at the data from the years 2009 and 2011, which were not subsidy eligible periods. Therefore, if the subsidy effect within the COBRA eligible group is insignificant for 2009 and 2011, that means the subsidy effect of the main results are driven by the ARRA COBRA subsidy. As Table 1.8 shows, the results are not significant and that means the seasonality is not affecting my main results.

5.4.3 Subsidy effect on unemployment duration of COBRA ineligible group

After confirming that there is no "subsidy selection" behavior happening between subsidy-eligible and subsidy-ineligible groups, I want to estimate the subsidy effect on the duration of unemployment for the COBRA in-eligible group using a placebo test. This test allows me to evaluate how likely it is to find a "false positive" when studying the ARRA COBRA premium subsidy effect. If I was to find a significant effect even in the group that had not been eligible for the subsidy, it would signal that the effects estimated in the COBRA eligible group may be spurious. I perform the placebo test using the COBRA ineligible group. Table 1.9 shows that there is no significant subsidy effect within the COBRA in-eligible group and the results further confirm that the subsidy effect on the duration of unemployment for the COBRA eligible group is mainly from ARRA.

5.4.4 Monthly variation and false end of subsidy eligibility

The false end of subsidy eligibility is set to see whether there is any subsidy effect on the duration of unemployment. During the subsidy-eligible period, January 2010 is set as the false end of subsidy eligibility; beyond the subsidy-eligible period, August 2010 is set as the false end of subsidy eligibility. I use two different unemployed cohorts who lost their jobs to investigate the effect of the premium subsidy on the duration of unemployment under the false end of subsidy eligibility scenario during, and beyond the subsidy-eligible period, respectively. The results in panel A in Table 1.10 show that the predicted subsidy effect is much smaller and insignificant under the false end of subsidy eligibility compared to the right end of subsidy eligibility, May 31st, 2010. Taking Jan'10 as the false end during the original subsidy-eligible period, the new subsidy-eligible/ineligible group can be established (Dec'09-Jan'10/Feb'10-

Mar'10). After controlling for underlying preference and UI, this group has negative and much smaller coefficients, which further show there is no subsidy effect on the false end of subsidy eligibility. Similarly, taking Aug'10 as the false end beyond the original subsidy-eligible period, the new subsidy-eligible/ineligible group can be established (Jun'10-Jul'10/Aug'10-Sep'10). After controlling the covariates and UI, this group also has negative and much smaller coefficients.

Some may argue that the effect of the premium subsidy on the duration of unemployment is driven by the variation between monthly cohorts. I employ two different unemployed neighboring cohorts to see the subsidy effect on the unemployment duration between monthly cohorts during and beyond the subsidy-eligible period, respectively. Panel B in Table 1.10 indicates that the subsidy effect from monthly variation is much smaller and insignificant compared to the results of the May/June cohort in Table 1.2. Similarly, the percentage differences of the subsidy effect are also much smaller and insignificant.

In sum, two things are indicated. First, there is no significant subsidy effect under the false end of subsidy eligibility, and the results further confirm that the subsidy effect on the duration of unemployment is mainly from the ARRA. Second, it shows that the main results are not driven by the variations between monthly cohorts because the variations between monthly cohorts are relatively small compared to the main results presented in Tables 1.2 and 1.3.

5.4.5 Unemployment exit rate at the end of COBRA/COBRA subsidy

Meyer (1990) finds the probability of leaving unemployment rises dramatically just prior to when benefits lapse. In order to check whether this scenario is happening here, results in Table 1.11 show the spell length for the subsidy-eligible and COBRA-eligible (i.e., subsidy-ineligible)

groups. In order to make the estimates be more reliable and precise, I add the number of individuals who become unemployed from Jan. 2010 to May 2010 and those are eligible for the 15-month premium subsidy. Likewise, I add the number of individuals who become unemployed from June 2010 to Dec. 2010 and those are eligible for 18-month COBRA premium. If the unemployed workers are having high probability of leaving unemployment at the end of the benefit, then we expect to see that there will be a spike when the benefits stop. The results in Table 1.11 show that there is no spike for the subsidy-eligible group when the spell length is equal to 15 months (i.e., the end of subsidy) and there is also no spike for the COBRA-eligible group when the spell length is equal to 18 months. These results confirm that there is no leaving unemployment behavior when benefits lapse.

6. Implication in terms of UI

This study suggests that the subsidy had a positive statistically significant impact on the unemployment duration for subsidy-eligible unemployed workers. A tremendous amount of literature exists about how UI affects the unemployment duration. In this section, I will discuss how the UI literature can help us interpret the results obtained from the ARRA COBRA premium subsidy study.

Katz and Meyer (1990) indicate that if the benefit level is reduced by 10 percent it decreases the predicted mean weeks of unemployment by 1.5 weeks. The average unemployment benefit is about $300 per week in 2010, 2011, and 2012 (Center on Budget and Policy Priorities, 2013). That is, a potential increase of $30 extra in UI benefit will lead to an increase by 1.5 weeks in unemployment duration. In this study, the COBRA premium subsidy-eligible unemployed workers significantly increased the predicted mean month of unemployment by 2.11

months. As a result, the predicted mean month of extended unemployment duration resulting from the premium subsidy is equivalent to the effect of increasing UI benefits by 56 percent.[11]

7. Conclusion

The American Recovery and Reinvestment Act (ARRA) of 2009 provided a 65 percent health insurance premium subsidy for workers who lost their jobs involuntarily and elected to take up COBRA coverage. This paper shows that the COBRA premium subsidy-eligible unemployed workers significantly increased unemployment duration conditional on underlying factors influencing their individual preferences for insurance and UI benefits. Moreover, the subsidy had a significant but smaller impact on the duration of unemployment for the following groups: full sample, married, older, educated, and older-educated unemployed individuals. These empirical results are consistent with the predictions of job search theory.

The behavioral responses to financial incentives are interesting from a scientific point of view because they allow me to investigate to what extent results are coherent with theoretical predictions. Theory provides predictions about the search intensity behavior pattern over the duration of unemployment. However, theoretical prediction only provides a description about the direction of the effects, but could not provide a rough estimate of their magnitude. With reference to the behavioral reaction of subsidy-eligible unemployed workers, these responses give some ideas about the potential degree of unemployment duration.

From a policy point of view this study is interesting as well. Extending the COBRA premium subsidy for individuals who experience such a loss in health insurance coverage when they become unemployed may correct market deficiencies (Gruber and Madrian, 1997). However, this study shows the government intervention has unintended side effects such as

[11] (2.11*4/1.5)*$30=$168; ($168/$300)*100%=56%

distorting employment decisions. This cost of lengthening the duration of unemployment must be weighed against the benefit which may be a more productive job search for higher paying positions from Ehrenberg and Oaxaca's study. However, in my study, I notice that there is no relation between a longer unemployment duration and higher post-employment wages. Therefore, there is much room here for future study.

While there has been a tremendous amount of literature on how Unemployment Insurance (UI) affects the unemployed workers' job search behavior, there has been no previous study on how the health insurance premium subsidy affects the unemployed workers' job search behavior. This study fills this gap and finds that the extended unemployment duration resulting from the premium subsidy is equivalent to the effect of increasing UI benefits by 56 percent which indicates that the COBRA premium subsidy is having an important impact on the unemployed workers' job transition behavior.

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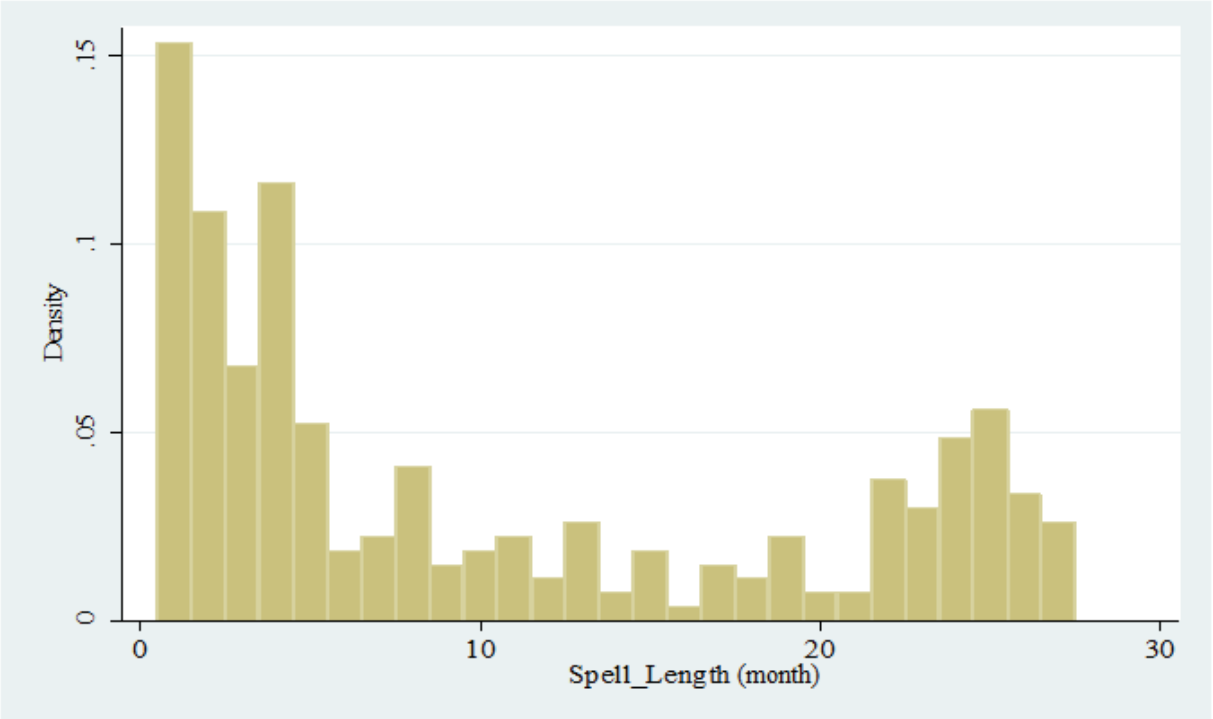
Table 1.1 Descriptive statistics

Variable	Unemployed cohorts (March 2010-August-2010)			
	Subsidy-eligible (before May 31st, 2010)		Subsidy-ineligible (after May 31st, 2010)	
	# of individuals	Mean	# of individuals	Mean
COBRA eligible				
Duration (May/June)	42	8.357	62	6.468
Duration (April-May/June-July)	80	9.000	101	7.029
Duration (Mar-May/June-Aug)	126	9.579	139	7.381
Full Sample				
Duration (May/June)	299	8.806	406	7.431
Duration (April-May/June-July)	555	8.863	733	7.873
Duration (Mar-May/June-Aug)	867	8.850	1038	7.835
Demographic characteristics Full sample (Mar-May/June-Aug)				
Age (years)		40.38		40.36
Female (%)		42.09		41.81
Non-white (%)		22.49		20.42
Education		40.12		40.18
Married (%)		39.56		41.32
# of own children		0.63		0.62
# of individuals		867		1038

Note: Bureau of Labor Statistics, 2010 shows that the average (mean) duration is 34.6 weeks (8.65 months) for 2010 and 40.2 weeks (10.05 months) for January 2011.
May 31st, 2010 is the end of ARRA COBRA subsidy eligibility.
Variable definitions: Duration = completed spell duration; Education= highest education level attained, 40 here indicates some college or above; Married = married with spouse present or absent; # of own children = number of own children less than 18 years old in family.

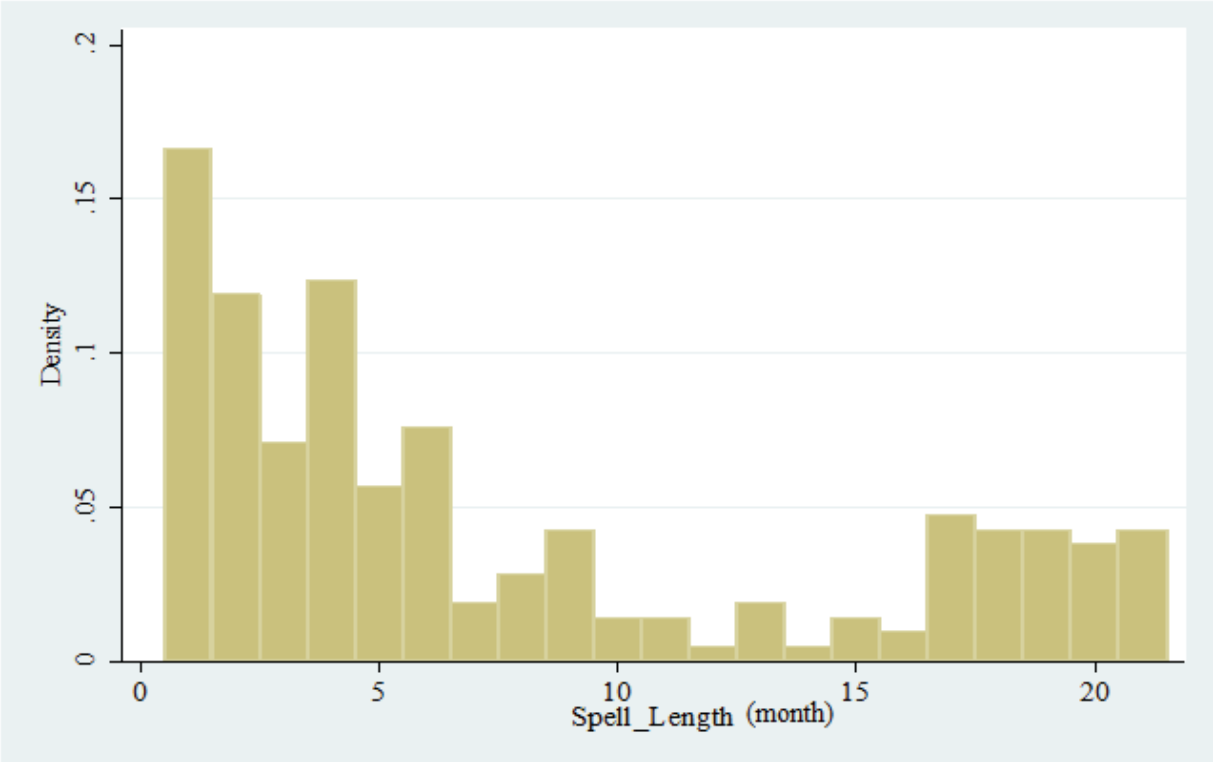
Variable	Unemployed cohorts (March 2010-August-2010)			
	Subsidy-eligible (before May 31st, 2010)		Subsidy-ineligible (after May 31st, 2010)	
	# of individuals	Mean	# of individuals	Mean
Older				
Duration (May/June)	161	8.373	231	7.229
Duration (April-May/June-July)	307	8.736	414	7.778
Duration (Mar-May/June-Aug)	498	9.122	581	7.840
Educated				
Duration (May/June)	172	8.994	224	6.763
Duration (April-May/June-July)	308	9.136	417	7.439
Duration (Mar-May/June-Aug)	484	8.888	572	7.460
Older educated				
Duration (May/June)	94	8.394	136	6.074
Duration (April-May/June-July)	176	8.898	254	7.063
Duration (Mar-May/June-Aug)	290	8.965	351	7.362
Married				
Duration (May/June)	119	7.445	170	6.559
Duration (April-May/June-July)	222	7.883	300	6.890
Duration (Mar-May/June-Aug)	343	8.184	429	6.737

Figure 1.1 Spell distribution for Subsidy-eligible group



Note: The sample is from the unemployed cohorts (March 2010-May 2010) within COBRA eligible group.

Figure 1.2 Spell distribution for Subsidy-ineligible group



Note: The sample is from the unemployed cohorts (June 2010-Aug. 2010) within COBRA eligible group.

Table 1.2 Subsidy effect on unemployment duration using Tobit

Subsidy-eligible / Subsidy-ineligible (control covariates)

	Spell Length (month)		
	May/June	April-May/June-July	Mar-May/June-Aug
Panel A: COBRA eligible			
Subsidy effect	2.1863	2.0996	2.6222
	[1.6972]	[1.307]*	[1.1363]**
# of individuals	104	181	265
Panel B: Full sample			
Subsidy effect	1.7587	1.2366	1.0836
	[0.8306]**	[0.6338]**	[0.5573]**
# of individuals	705	1288	1905
Panel C: Older			
Subsidy effect	1.1833	1.0282	1.2774
	[0.985]	[0.7541]	[0.6634]*
# of individuals	392	721	1079
Panel D: Educated			
Subsidy effect	2.3868	1.8974	1.5226
	[0.9246]***	[0.7325]***	[0.6213]**
# of individuals	396	725	1056
Panel E: Older educated			
Subsidy effect	2.1557	1.9494	1.596
	[1.0314]**	[0.8459]**	(0.7306)**
# of individuals	230	430	641
Panel F: Married			
Subsidy effect	0.9603	1.0675	1.4697
	[0.8923]	[0.7356]	[0.6214]**
# of individuals	289	522	772

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. Robust standard error is in square brackets.

*** Significant at the 1 percent level, two-tail test.

** Significant at the 5 percent level, two-tail test.

* Significant at the 10 percent level, two-tail test.

Table 1.3 Subsidy effect on log unemployment duration using Tobit

Subsidy-eligible / Subsidy-ineligible (control covariates)

	Log [Spell Length (month)]		
	May/June	April-May/June-July	Mar-May/June-Aug
Panel A: COBRA eligible			
Subsidy effect	0.2848	0.2445	0.3476
	[0.2375]	[0.1813]	[0.1542]**
# of individuals	104	181	265
Panel B: Full sample			
Subsidy effect	0.2446	0.162	0.1303
	[0.1131]**	[0.0864]*	[0.0723]*
# of individuals	705	1288	1905
Panel C: Older			
Subsidy effect	0.1818	0.1237	0.1488
	[0.1385]	[0.1051]	[0.0879]*
# of individuals	392	721	1079
Panel D: Educated			
Subsidy effect	0.3519	0.2832	0.2172
	[0.1248]***	[0.1004]***	[0.0836]***
# of individuals	396	725	1056
Panel E: Older educated			
Subsidy effect	0.3278	0.2572	0.2049
	[0.1442]**	[0.1185]**	[0.1012]**
# of individuals	230	430	641
Panel F: Married			
Subsidy effect	0.1935	0.1764	0.2011
	[0.1336]	[0.1071]*	[0.0881]**
# of individuals	289	522	772

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. Robust standard error is in square brackets.

*** Significant at the 1 percent level, two-tail test.

** Significant at the 5 percent level, two-tail test.

* Significant at the 10 percent level, two-tail test.

Table 1.4 Subsidy effect on unemployment duration using Tobit and controlling for UI

Subsidy-eligible / Subsidy-ineligible (control covariates)

	Spell Length (month)		
	May/June	April-May/June-July	Mar-May/June-Aug
Panel A: COBRA eligible			
Subsidy effect	2.1196	2.1137	2.6255
	[1.7165]	[1.3055]*	[1.1354]**
# of individuals	104	181	265
Panel B: Full sample			
Subsidy effect	1.7587	1.2366	1.1032
	[0.8306]**	[0.6338]**	[0.5567]**
# of individuals	705	1288	1905
Panel C: Older			
Subsidy effect	1.1063	1.0276	1.2729
	[0.987]	[0.7542]	[0.6631]*
# of individuals	392	721	1079
Panel D: Educated			
Subsidy effect	2.2623	1.8767	1.5328
	[0.9312]**	[0.7305]***	[0.6206]**
# of individuals	396	725	1056
Panel E: Older educated			
Subsidy effect	1.8147	1.9003	1.5824
	[1.0497]*	[0.8478]**	(0.7326)**
# of individuals	230	430	641
Panel F: Married			
Subsidy effect	0.9406	1.0572	1.4554
	[0.8921]	[0.7362]	[0.6204]**
# of individuals	289	522	772

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. Robust standard error is in square brackets.
UI=unemployment insurance

*** Significant at the 1 percent level, two-tail test.
** Significant at the 5 percent level, two-tail test.
* Significant at the 10 percent level, two-tail test.

Table 1.5 Subsidy effect on log unemployment duration using Tobit and controlling for UI

Subsidy-eligible / Subsidy-ineligible (control covariates)

	Log [Spell Length (month)]		
	May/June	April-May/June-July	Mar-May/June-Aug
Panel A: COBRA eligible			
Subsidy effect	0.2713	0.2473	0.3479
	[0.2379]	[0.1812]	[0.1542]**
# of individuals	104	181	265
Panel B: Full sample			
Subsidy effect	0.2446	0.162	0.1336
	[0.1131]**	[0.0864]*	[0.0721]*
# of individuals	705	1288	1905
Panel C: Older			
Subsidy effect	0.1681	0.1237	0.1483
	[0.1385]	[0.1051]	[0.0879]*
# of individuals	392	721	1079
Panel D: Educated			
Subsidy effect	0.329	0.2812	0.2191
	[0.1259]***	[0.1003]***	[0.0836]***
# of individuals	396	725	1056
Panel E: Older educated			
Subsidy effect	0.2663	0.2487	0.2026
	[0.1456]*	[0.119]**	[0.1014]**
# of individuals	230	430	641
Panel F: Married			
Subsidy effect	0.19	0.175	0.1991
	[0.1334]	[0.1072]*	[0.0886]**
# of individuals	289	522	772

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. Robust standard error is in square brackets.
UI=unemployment insurance

*** Significant at the 1 percent level, two-tail test.
** Significant at the 5 percent level, two-tail test.
* Significant at the 10 percent level, two-tail test.

Table 1.6 Subsidy selection check by unemployed worker groups

2010	Feb.	Mar.	April	May	June	July	Aug.
Bureau of Labor Statistics							
(Unadj) Unemployment rate	10.4	10.2	9.5	9.3	9.6	9.7	9.5
(Adj) Unemployment rate (%)	9.8	9.9	9.9	9.6	9.4	9.5	9.5
COBRA eligible							
Total Unemployed	344	333	324	306	310	281	282
New Unemployed workers	46	46	38	42	62	39	38
New Unemployed workers (%)	13.37	13.81	11.73	13.73	20.00	13.88	13.48
COBRA ineligible							
Total Unemployed	4678	4624	4244	4083	4425	4260	4161
New Unemployed workers	379	403	322	375	569	421	384
New Unemployed workers (%)	8.10	8.71	7.58	9.18	12.85	9.88	9.22
Full sample							
Total Unemployed	5022	4957	4568	4389	4735	4541	4443
New Unemployed workers	425	449	360	417	631	460	422
New Unemployed workers (%)	8.46	9.06	7.88	9.50	13.33	10.13	9.50
Older							
Total Unemployed	2848	2820	2589	2,439	2583	2517	2460
New Unemployed workers	235	272	223	225	385	263	229
New Unemployed workers (%)	8.25	9.65	8.61	9.23	14.91	10.45	9.31
Educated							
Total Unemployed	2682	2695	2480	2409	2612	2511	2497
New Unemployed workers	243	264	209	254	386	278	241
New Unemployed workers (%)	9.06	9.80	8.43	10.54	14.78	11.07	9.65
Older educated							
Total Unemployed	1656	1652	1519	1442	1550	1510	1501
New Unemployed workers	149	168	137	138	242	171	147
New Unemployed workers (%)	9.00	10.17	9.02	9.57	15.61	11.32	9.79
Married							
Total Unemployed	2059	2023	1828	1729	1853	1813	1778
New Unemployed workers	180	196	174	194	309	214	184
New Unemployed workers (%)	8.74	9.69	9.52	11.22	16.68	11.80	10.35

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. (Unadj) Unemployment rate= not seasonally adjusted unemployment rate. (Adj) Unemployment rate=seasonally adjusted unemployment rate.

Table 1.7 Subsidy selection check by unemployed worker between May/June in 2009 and 2011

	2009		2011	
	May	June	May	June
Bureau of Labor Statistics				
(Unadj) Unemployment rate	9.1	9.7	8.7	9.3
(Adj) Unemployment rate	9.4	9.5	9	9.1
Full sample				
Total Unemployed	4782	5089	3755	3923
New Unemployed workers	557	731	356	522
New Unemployed workers (%)	11.64	14.36	9.48	13.30

Note: (Unadj) Unemployment rate= not seasonally adjusted unemployment rate. (Adj) Unemployment rate=seasonally adjusted unemployment rate.

Table 1.8 Seasonality check within COBRA eligible group between May/June in 2009 and 2011 using Tobit

Subsidy-eligible vs. Subsidy-ineligible

	Spell Length (month)	
	2009	2011
	May/June	May/June
COBRA eligible		
Subsidy effect	0.1194	0.2408
	[1.6124]	[1.6487]
# of individuals	119	63

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility.

Table 1.9 Subsidy effect on unemployment duration using Tobit for COBRA in-eligible group

Subsidy-eligible vs. Subsidy-ineligible

	Spell Length (month)		
2010	May/June	April-May/June-July	Mar-May/June-Aug
COBRA in-eligible			
Subsidy effect	-0.5649	-0.0995	0.1199
	[0.3598]	[0.2719]	[0.2232]
# of individuals	601	1107	1640

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. Robust standard error is in square brackets.

Table 1.10 False end of subsidy eligibility and monthly variation checks using Tobit

Subsidy-eligible vs. Subsidy-ineligible

Panel A: False end of subsidy eligibility check				
	Jan'10		Aug'10	
	Dec'09-Jan'10/ Feb'10-Mar'10		Jul'10-Aug'10/ Sep'10-Oct'10	
	Spell	Log [Spell]	Spell	Log [Spell]
Subsidy Effect	-0.0252	-0.0107	-0.0457	-0.0494
	[0.7494]	[0.0921]	[0.7557]	[0.1125]
# of individuals	1456		1235	
Panel B: Monthly variation check				
	Jan'10/ Feb'10		Aug'10/ Sep'10	
	Spell	Log [Spell]	Spell	Log [Spell]
Subsidy effect	0.4727	0.0453	0.0261	-0.0438
	[1.3154]	[0.1581]	[0.9929]	[0.155]
# of individuals	638		621	

Note: May 31st, 2010 is the end of ARRA COBRA subsidy eligibility. Robust standard error is in square brackets.
UI=unemployment insurance

Table 1.11 Unemployment exit rate at the end of COBRA/COBRA subsidy check

Spell Length (month)	Subsidy-eligible (individuals)	Percent	COBRA-eligible (individuals)	Percent
1	29	17.68%	35	21.08%
2	23	14.02%	25	15.06%
3	15	9.15%	15	9.04%
4	26	15.85%	26	15.66%
5	11	6.71%	12	7.23%
6	4	2.44%	16	9.64%
7	5	3.05%	4	2.41%
8	8	4.88%	6	3.61%
9	4	2.44%	9	5.42%
10	2	1.22%	3	1.81%
11	6	3.66%	3	1.81%
12	3	1.83%	1	0.60%
13	5	3.05%	4	2.41%
14	1	0.61%	1	0.60%
15	4	2.44%	3	1.81%
16	1	0.61%	2	1.20%
17	4	2.44%	1	0.60%
18	3	1.83%	0	0.00%
19	5	3.05%	0	0.00%
20	1	0.61%	0	0.00%
21	2	1.22%	0	0.00%
23	1	0.61%	0	0.00%
25	1	0.61%	0	0.00%
Total individuals	164		166	

Table 1.A1 Details and schedule of Unemployment Insurance coverage

Panel A: Details coverage

Program		Length (weeks)	Eligibility
State Unemployment Insurance		26	all
Emergency Unemployment Compensation	Tier 1	20	all
	Tier 2	14	all
	Tier 3	13	state unemployment rate > 6%
	Tier 4	6	state unemployment rate > 8.5%
Extended Benefits	Option 1	13	state unemployment rate > 6.5%
	Option 2	20	state unemployment rate > 8%

Panel B: Benefits schedule

Unemployment rate (U)	Length (weeks)
U <= 6	60
6 < U <= 6.5	73
6.5 < U < 8.1	86
8 < U < 8.6	93
U > 8.5	99

Note: The schedule in Panel B is author's calculation

CHAPTER 2.The Impact of the Health Care Reform on the Applications for Disability Benefits: Evidence from Massachusetts

1. Introduction

Social Security Disability Insurance (SSDI), which provides benefits to those who are physically incapable of finding suitable work, is one of the largest income replacement programs in the United States. In 2009, there were 9.7 million program recipients, on which the government spent $124 billion in cash benefits and $70 billion in health care expenditures according to Congressional Budget Office (CBO), 2010. SSDI recipients receive a cash stipend that replaces, on average, 45 percent of their previous earnings. Moreover, they will receive Medicare coverage after a two-year waiting period.

Autor and Duggan (2006) provide several potential reasons why the SSDI rolls grow so rapidly. First, the liberalization of Disability Insurance screening makes people suffering from back pain and mental illness eligible. Next, the rise in the replacement rate (the ratio of disability cash benefits to previous labor earnings) provides incentives for workers to seek benefits. Lastly, the interactions of the above two factors with a rapid increase in the female labor force participation explain the enormous growth of SSDI beneficiaries.

However, the health insurance component of the SSDI program has not been the subject of much study except for Gruber and Kubik (2002). They find that potential SSDI beneficiaries tend to have high medical cost, and they also show that those who have an alternative source of insurance should they leave their job, are 26 to 74% more likely to apply for SSDI than those without such an alternative source of health insurance. On average health costs are particularly high for disabled individuals; in 2009, a SSDI recipient averaged $10,500 in Medicare health

costs, which is equivalent to more than 80 percent of the average yearly SSDI cash benefit (CBO, 2010). The health insurance component of SSDI is therefore particularly valuable to these disabled individuals. Thus, the 2006 Massachusetts health reform is likely to influence the application rates for SSDI benefits, but whether it will result in more or fewer beneficiaries is difficult to predict. Among other changes, the legislation will make it easier for people with health problems to buy their own insurance; it will also provide new subsidies for individually purchased coverage and expand eligibility for Medicaid.

This paper focuses on the hypothesis that the probability of applying for SSDI for spouses without ESI will be higher compared to spouses with ESI coverage after the reform. The differential is probable because potential beneficiaries for SSDI coverage who have stopgap forms of health coverage are substantially more likely to apply for disability than potential applicants who would lose other insurance coverage while awaiting eligibility for the disability-related Medicare benefit. Understanding and quantifying the effects of health reform on the disability insurance program is important because it will improve our ability to explain and manage the increasing costs of the SSDI programs in terms of new beneficiaries and the size of future caseloads.

I estimate the causal effect of the reform on SSDI beneficiaries using data from the Survey of Income and Program Participation (SIPP). This survey follows a sample of persons for four to five years, collecting data on their monthly SSDI beneficiary status, and spousal health insurance status across the time period before and after the implementation of the health reform.

I evaluate the effect of the ESI expansion on the change of SSDI beneficiaries' rate using the near-universal expansion of health insurance coverage happening in Massachusetts in 2006. Massachusetts simultaneously implemented premium credits to low- and middle-income

residents. I perform a behavioral analysis that utilizes the variation in the intensity of the impact of the reform across potential SSDI applicants. Potential SSDI applicants who are affected by the ESI expansion will have more incentive to move onto SSDI rolls than the individuals who are not affected by the ESI expansion. Additionally, I compare potential SSDI beneficiaries in Massachusetts to similar counterparts in six nearby states. Exploiting the variation in treatment intensity allows me to identify how expanding private insurance coverage affected SSDI beneficiaries in a way that is robust to Massachusetts-specific time trends.

I have several findings. I find that for spouses without ESI, the positive effect of the ESI expansion on SSDI participation is 0.98 percentage points stronger than it is for spouses with ESI. These estimates imply that spouses without ESI are associated with higher SSDI beneficiaries which accounts for a 10.7 percentage point increase in SSDI beneficiaries in Massachusetts after the reform. Moreover, my estimates imply an elasticity of spouses without ESI with respect to SSDI beneficiaries of 0.0913.

This rest of the paper is laid out in five sections. Section 2 gives an overview of the SSDI program in the US and the ESI expansion in Massachusetts. This is followed by Section 3, which discusses the data and empirical strategy. Section 4 presents the results for the impact of 2006 health reform on SSDI participation decisions, and Section 5 concludes.

2. Background

2.1 Social Security Disability Insurance

SSDI provides insurance for those persons who have an "inability to engage in any substantial gainful activity by reason of physical or mental impairment" and this impairment must be expected to last at least 12 months or result in death. Most applicants must have worked 20 of the

40 quarters prior to the onset of disability to be eligible to apply for SSDI. Moreover, an application for SSDI cannot be made unless the applicant has been out of work for at least five months. After a waiting period of two years, SSDI recipients are eligible for Medicare coverage, which is a very valuable benefit for the disabled with high medical costs.

2.2 Employer Sponsored Health Insurance expansions

Measuring the causal impact of insurance is notoriously difficult because it requires finding exogenous sources of variation in insurance status. The natural experiment in this paper is a particularly relevant source of credible exogenous variation to study the effect of private health insurance on SSDI participation. In 2006, Massachusetts restructured the way private insurance is purchased and sold to expand health insurance coverage to nearly all state residents. Massachusetts combined an individual mandate to purchase insurance with a major expansion of the Medicaid program and new subsidies for individuals earning up to 300 percent of the federal poverty line. Under the new law, all residents must meet minimum health insurance coverage (to avoid the loss of personal income tax exemption, $219 in 2007, with additional monthly penalties which are based on 50 percent of the cost for the lowest-priced available insurance plan beginning in 2008) when affordable coverage is available. For a detailed description of the reform, see Gruber (2008) or Raymond (2007).

In addition to the mandate, Massachusetts dramatically increased free and subsidized coverage to low-income households via the “MassHealth" Medicaid program, which expanded eligibility for low-income individuals and children. Massachusetts also introduced a new program, “Commonwealth Care," that subsidized private insurance to individuals earning up to 300 percent of the federal poverty line (with the level of subsidies based on income) who are not

eligible for ESI or MassHealth. Massachusetts facilitates enrollment of individuals into both subsidized and unsubsidized insurance plans (Lischko, 2009).

3. Data and empirical strategy

3.1 Data

The impact of the ESI expansion, part of 2006 Massachusetts health reform, on SSDI participation decisions is examined using data from both 2004 and 2008 panels of the SIPP, a nationally representative longitudinal survey in the United States.[12] The 2004 SIPP panel enrolls persons from October 2003 through January 2004 and follows them for 48 months; whereas the 2008 SIPP panel enrolls persons from May 2008 through August 2008 and follows them for 52 months. The survey uses a 4-month recall period to conduct interviews every 4 months and provides their monthly SSDI beneficiary status and spousal health insurance status that allowed me to see the change of these two groups before and after the implementation of the health reform. Moreover, this paper relies on administrative data sets from 2003 to 2009 for the SSDI beneficiaries and population data from Social Security Administration (SSA) to calculate the percentage of SSDI beneficiaries among states. Therefore, I can use the administrative data to confirm the change of SSDI beneficiaries in SIPP.

3.2 Empirical strategy

My identification strategy relies on the assumption that, if the reform had not taken place, SSDI beneficiaries in potential eligible—and ineligible—applicants in Massachusetts would have evolved similarly. Taking advantage of the "natural experiment" that occurred in Massachusetts

[12] The 2004 and 2008 SIPP panels can be used to produce state estimates. (SIPP USERS Guide, Chapter 10, Revisions 2009 Page 40)

to compare the change in the SSDI beneficiaries among married couples ages 18 to 64 before and after the state implemented its health reform initiative, using Difference-in-Difference (DD) and Difference-in-Difference-in-Difference (DDD) method.[13]The estimation approach exploits variation over time (comparing pre-and post-reform time periods), across population groups (comparing SSDI potential applicants who are affected by ESI expansion to SSDI potential applicants who are not affected by ESI expansion), and across states (comparing Massachusetts to comparison states in the Northeast that did not implement health reform).

Since Massachusetts implemented the health reform on April 2006 and the SSDI has a 5-month waiting period plus the 19-month Medicare waiting period, my post-reform period using the SSA and SIPP begins in 2008. After defining the pre-and post-reform periods, I then compared the SSDI beneficiaries in the post-reform period of 2008-2009 to those beneficiaries in the pre-reform period of 2003–2005 among married couples.

The comparison states provide an estimate of what would have happened in Massachusetts in the absence of health reform. Identifying an appropriate comparison state is difficult given the wide variation in state policies, programs and populations, and the frequency with which other states were also implementing program and policy changes that affected SSDI potential applicants over the study period. In this paper, I rely on four states in the New England division (1.New Hampshire, NH; 2.Vermont, VT; 3.Rhode Island, RI; and 4.Connecticut, CT) and two states in the Middle Atlantic division (5.New Jersey, NJ; 6.Pennsylvania, PA) in the Northeast region as comparison states. The comparison group provides an estimate of what would have happened in the absence of the ESI expansion within Massachusetts. The control group consists of people who have health insurance from their spouse's group/employer plan because they can have health insurance whether this reform existed or not.

[13] SIPP respondents are asked about the age to begin receiving Social Security Disability payments because of his/her disability.

To estimate the overall impact of health reform on SSDI participation decisions, I compared the change in the SSDI beneficiaries in Massachusetts to the change for a similar group in comparison states before and after the reform by using a DD framework. The comparison states control for underlying trends in the SSDI unrelated to health reform. Furthermore, I extend the analysis by comparing the DD estimate on the potential SSDI applicants who are affected by ESI expansion to an analogous DD estimate on the potential SSDI applicants who are not affected by ESI expansion using a DDD framework.

4. The Impact of 2006 Health Reform on SSDI Participation Decisions

Table 2.1 presents the summary statistics for two different periods (pre vs. post) and two different groups (USA vs. Massachusetts). One chart includes pre-reform SSDI beneficiaries, and the other consists of post-reform SSDI rolls. Two things are indicated. First, both the percentage of SSDI beneficiaries for US and Massachusetts are higher during the treatment period compared to the control period. In particular, the number of SSDI rolls in Massachusetts has increased more rapidly than the U.S. Second, the relatively low fraction of married female, nonwhite workers, and relatively young age is indicative of the difference between US and Massachusetts' SSDI beneficiaries' characteristics. In terms of average level of education, there is not much difference across all SSDI beneficiaries, and the average level of education is high school graduate.

I use two types of variation to identify the effect of employer sponsored insurance coverage on SSDI participation decisions. First, I analyze the relative change in the rate of SSDI beneficiaries and the rate of spouses without ESI in Massachusetts based on their exposure to the reform compared to the rate of these two groups during the pre-reform period. Because the

reform instituted near-universal coverage, spouses without ESI have more incentive to apply for SSDI than spouses with ESI. We should expect to see the rate of SSDI beneficiaries increase because more potential SSDI applicants will try to apply when they have a stopgap form of health insurance which is available after the health reform was implemented. Second, I compare the variation in the rate of SSDI beneficiaries in Massachusetts with the variation in the comparison states of New Hampshire, Vermont, Rhode Island, Connecticut, New Jersey, and Pennsylvania. These estimates are robust to Massachusetts when it comes to specific shocks and differential trends in SSDI beneficiaries' rate between Massachusetts and other states.

4.1 Within Massachusetts analysis

I first analyze the effect of the reform by comparing the number of SSDI beneficiaries before and after the health reform within Massachusetts, controlling for a number of characteristics of individuals that could affect preferences for applying for SSDI. Second, I exploit the variation across SSDI beneficiaries among married couples to identify the causal effect of the reform on SSDI beneficiaries. The basic models are of the forms:

$$SSDI = \alpha + \beta * T2 + \delta * X + u \quad (1)$$

$$Spouse\ w/o\ ESI = \alpha + \beta * T2 + \delta * X + u \quad (2)$$

In this specification, SSDI and Spouse w/o ESI are the outcomes of interest. *SSDI* is a dummy variable indicating the age to begin receiving Social Security Disability payments because of his/her disability while *Spouse w/o ESI* indicates an individual who does not have insurance through their spouses' group/employer plan. *T2* is an indicator to capture the average effect of

the health reform on the SSDI beneficiaries/ Spouse w/o ESI rolls. The vector of control variables, *X*, represents individuals' demographic and socioeconomic characteristics including ages, sex group, education levels, and race.

$$P\,(SSDI=1|x) = F\,(\beta_0 + \beta_1\,(Spouse\ w/o\ ESI) + \beta_2 T2 + \boldsymbol{\beta_3} T2\cdot\,(Spouse\ w/o\ ESI) + \delta * X) \qquad (3)$$

When subjects in a treatment group and a control group are observed in both the pre-treatment and post-treatment periods and the pre-treatment time trends in the outcome variable, SSDI rolls, are not significantly different in the two groups, DD models can be used to estimate the effect of the treatment on the treated. One way to specify the model is by defining a variable *T2* that is equal to one if the observation is from the post-treatment period, and zero if from the pre-treatment period, and a variable *Spouse w/o ESI* that is equal to one if the observation is from the treatment group and zero if from the control group.

The preference for SSDI arises from variations across age, sex group, education level, and race. Obviously each of these dimensions is correlated with preferences for SSDI, so the regression for every category could be controlled. Therefore, in this specification, identification comes from the variation in the treatment group within these categories. That is, this approach uses the fact that the health reform changes for the spouse with ESI versus the spouse without ESI over time.

Next, a more aggregate approach is taken to look at the effect of health reform on the change of SSDI beneficiaries during a seven-year period. That is, a sample of SSDI beneficiaries is taken from 2003 to 2009. After pooling all data across years, the estimates are reliable and precise, and the results are consistent across years.

This paper mainly focuses on the married sample. Before looking at the results from the married sample, we need to look at the whole sample to verify the generality of the estimation. Further, I generate SSDI beneficiaries' rates per-capita by dividing the number of SSDI beneficiaries in a given year by the SSA's estimated county population. Consequently, I compare the percentage change of SSDI beneficiaries between SIPP and SSA data during the pre- and post- reform period. Thus, the panel A of Table 2.2 presents the estimates of equation (1) for the whole sample to show that the SSDI beneficiaries from SIPP significantly increase by between 0.358 (un-weighted) and 0.561 (weighted) percentage points which are close to the percentage change of SSDI beneficiaries (0.55 percentage points) from SSA (see the appendix).

Next, for the married sample, panel B shows that the SSDI beneficiaries increase significantly by 0.4 percentage points while the group of spouses without ESI increases by 4.38 percentage points significantly after the health reform. The percentage change among SSDI beneficiaries, and spouses without ESI, helps calculate the elasticity between these two groups. A 0.4 percent increase in SSDI beneficiaries is associated with a 4.38 percent increase in spouses without ESI coverage, which translates into an elasticity estimate of 0.0913.

Lastly, in order to estimate the impact of the ESI expansion on SSDI participation decision in Massachusetts, I investigate the behavior of SSDI potential applicants in treatment group in a regression framework using equation (3). I expect to see the positive significant coefficients on interaction term because more intensive ESI expansion from the health reform may have led potential SSDI applicants who are affected by ESI to have much more incentive to move onto SSDI rolls. Panel C presents estimates of the effect on the treatment group after the health reform for the rate of SSDI beneficiaries ages 18 to 64. As expected, the coefficients of interest are positive and significantly different from zero. Moreover, the marginal effect is

0.0098. That is, a 1 percent increase in the group of spouses without ESI, and means that the SSDI beneficiaries will increase by 0.0098 percent. Recalled from panel A, the group of spouses without ESI increased by 4.38 percent which means the predicted SSDI beneficiaries will increase by 4.38*0.0098 percent=0.0429 percent which accounts for 10.73 percent (0.000429/0.004=0.1073) of total SSDI beneficiaries increase.

4.2 The impact of health reform on Massachusetts across states

In order to estimate the effect of health reform on Massachusetts compared to other neighboring states, extended version of Eq. (1) and Eq. (2) are estimated:

$$SSDI = \beta_0 + \beta_1 MA + \beta_2 T2 + \boldsymbol{\beta_3} MA \cdot T2 + \delta * X + u \qquad (4)$$

$$Spouse\ w/o\ ESI = \beta_0 + \beta_1 MA + \beta_2 T2 + \boldsymbol{\beta_3} MA \cdot T2 + \delta * X + u \qquad (5)$$

The dummy variable, MA representing Massachusetts, captures possible differences between the treatment and control state which is a non-policy state. The coefficient of interest is now $\boldsymbol{\beta_3}$, the coefficient on the interaction term. The variable, $T2 \cdot MA$, is the same as a dummy variable equal to one for those observations in the treatment state in the second period.

Table 2.3 demonstrates effects on the treatment state after health reform for the percentage of SSDI beneficiaries whose ages from 18 to 64 and the percentage of spouse without ESI. For the whole sample after health reform, panel A shows that the percentage change of SSDI beneficiaries increased by 0.48 percent to 0.59 percent while the percentage change of spouses without ESI increased by 0.84 percent to 3.16 percent. Additionally, these estimates are all significant at the 1 percent level. Next, for the married sample after health reform, panel B

shows that the percentage change of SSDI beneficiaries increased by 0.24 percent to 1.15 percent while the percentage change of spouses without ESI increased by 1.76 percent to 3.49 percent. As expected, the coefficients of interest are positive and significantly different from zero. I use three different numbers of control states to see if the results consistently show that these two groups increased significantly after health reform.

4.3 Omitted factors

The "differences-in-differences" identification strategy of Eq. (3) would be sufficient if there were no other Massachusetts-specific shocks and differential trends in SSDI beneficiaries' rate between Massachusetts and other states. But it is difficult to control for these trends and shocks that are changing at the same time. Therefore, I extend the analysis by comparing the DD estimate on the potential SSDI applicants who are affected by ESI expansion to an analogous DD estimate on the potential SSDI applicants who are not affected by ESI expansion using a DDD framework.

$$SSDI=\alpha_0+\alpha_1 MA+\alpha_2\,(Spouse\ w/o\ ESI)+\alpha_3 MA\cdot(Spouse\ w/o\ ESI)+\beta_0 T2+\beta_1 T2\cdot MA+\beta_2 T2\cdot(Spouse\ w/o\ ESI)+\boldsymbol{\beta_3} T2\cdot MA\cdot(Spouse\ w/o\ ESI)+\delta * X+u \quad (6)$$

In this "differences-in-differences-in-differences" model, the triple interaction term among *T2*, *MA*, and *Spouse w/o ESI* measures the specific effect of ESI expansion on those who are potential SSDI applicants in Massachusetts after health reform and β_3 measures the causal impact of the health reform on the change of SSDI rolls.

Table 2.4 presents the results from estimating Eq. (6) for the full sample and the married sample respectively. It demonstrates estimates of the treatment group in the treatment state during the treatment period (after health reform) for the percentage of SSDI beneficiaries whose ages are from 18 to 64; it shows the percentage change of SSDI beneficiaries increased by between 1.14 percent to 1.37 percent, and these estimates are all significant at the 1 percent level. As expected, the coefficients of interest are positive and significantly different from zero. This specification better controls for Massachusetts-specific shocks and differential trends in SSDI beneficiaries' rate between Massachusetts and other states because these estimates rely on this DDD model.

4.4 Robustness checks

I use a placebo test to estimate the effect of the reform as if it had occurred in other states or during another time. This test allows me to evaluate how likely it is to find a "false positive" when studying the Massachusetts health reform. If I were to find a significant effect even in states/time that had not enacted a major health care reform, it would signal that the effects estimated in Massachusetts may be spurious. I perform these placebo tests using the comparison states (New Jersey and Pennsylvania). In addition, I perform these placebo tests using false implement time of health reform, e.g. 2004 and 2005. Specifically, I use the following models to estimate:

$$y = \gamma_0 + \gamma_1 STATE + \gamma_2 T2 + \gamma_3 STATE \cdot T2 + u \qquad (7)$$

$$y = \gamma_0 + \gamma_1 MA + \gamma_2 TIME + \gamma_3 MA \cdot TIME + u \qquad (8)$$

The dummy variable, *STATE* representing the false treatment state, captures possible differences between the treatment and control states prior to the policy change. The coefficient of interest is now γ_3, the coefficient on the interaction term, *T2·STATE*. On the other hand, the dummy variable, TIME representing the false treatment time, captures possible differences between the treatment and control period prior to the policy change. The coefficient of interest is now γ_3, the coefficient on the interaction term, *MA·TIME*.

Panel A of Table 2.5 shows that the false treatment state, New Jersey, doesn't have a significant effect while Pennsylvania even has a negative significant effect which makes the main results even more credible. The absence of an effect in the placebo states provides some evidence that the results presented in the main text are due to the law in Massachusetts rather than a random fluctuation in SSDI beneficiaries' rate. In panel B, I use two false treatment times to further check whether there is any health reform effect happening at during a fictitious time. The results show that either there is no effect or there is negative significant effect, which confirms the change of SSDI beneficiaries is mainly from Massachusetts health reform.

5. Discussion and Conclusion

Congressional Budget Office, 2010 shows that a SSDI beneficiary had on average $10,500 in Medicare health costs, which is equivalent to more than 80 percent of the average yearly SSDI cash benefit in 2009. Therefore, the Medicare coverage is particularly valuable to these disabled individuals. Thus, the 2006 Massachusetts health reform is likely to influence the application rates for SSDI benefits, but whether it will result in more or fewer beneficiaries is unclear.

This paper finds that the positive effect of the ESI expansion on SSDI participation is 0.98 percentage points stronger for spouses without ESI than it is for spouses with ESI. These

estimates imply spouses without ESI are associated with higher SSDI beneficiaries which accounts for a 10.7 percentage point increase in SSDI beneficiaries in Massachusetts after the reform. Moreover, the estimates imply an elasticity of spouses without ESI with respect to SSDI beneficiaries of 0.0913. The calculations also suggest that the health reform was more expensive than it might first appear because of an increase in SSDI expenditure.

From a policy point of view this study is interesting. Providing the health insurance coverage to individuals creates stopgap forms of health coverage for potential SSDI beneficiaries. Those with an alternative source of coverage are more likely to apply to the program than those without such an alternative. These findings have several important welfare implications. First, they suggest that providing insurance coverage to potential SSDI applicants will not reduce uninsured people too much in the U.S. Indeed, it would only substitute private health insurance with public health insurance.

Second, providing insurance during the waiting period may encourage potential applicants to apply for SSDI. However, these increased applications are difficult to assess the welfare implications. Gruber and Kubik (2002) provide a detailed discussion about welfare implications regarding how health insurance affects different disabled applicants. For example, if the additional applications are from the least disabled applicants, then it might suggest little inefficiency in health reform expenditure; however, if the increased applications are from the most disabled applicants, then it could lead to significant welfare gains by helping disabled applicants who cannot bear the risk of going without coverage for any period of time. Future work could assess the underlying disability status of those SSDI applicants to improve our ability to explain and manage the increasing costs of the SSDI programs in terms of new beneficiaries and the size of future caseloads.

Appendix for Chapter 2.

SSA population data and disabled beneficiaries ages 18-64

Calendar year	Source
2003	Annual Statistical Report on the Social Security Disability Insurance Program, 2003 Table 8.
2004	Annual Statistical Report on the Social Security Disability Insurance Program, 2004 Table 8.
2005	Annual Statistical Report on the Social Security Disability Insurance Program, 2005 Table 8.
2006	Annual Statistical Report on the Social Security Disability Insurance Program, 2006 Table 8.
2007	Annual Statistical Report on the Social Security Disability Insurance Program, 2007 Table 8.
2008	Annual Statistical Report on the Social Security Disability Insurance Program, 2008 Table 8.
2009	Annual Statistical Report on the Social Security Disability Insurance Program, 2009 Table 8.

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Table 2.1 Summary statistics

	SSDI rolls (weighted)							
Variable	Treatment period (08-09)				Control period (03-05)			
	USA		*MA*		*USA*		*MA*	
Mean	1.42%		3.33%		1.37%		2.44%	
Age	54.84	(12.46)	52.59	(11.86)	54.65	(11.9)	52.48	(10.51)
Female (%)	47.67	(49.94)	48.38	(50.01)	51.4	(49.98)	59.87	(49.04)
Nonwhite (%)	25	(43.3)	16.12	(36.81)	26	(43.88)	11.02	(31.34)
Education	39.2	(3.1)	39.01	(2.99)	38.86	(3.13)	39.03	(3.62)
Married	40.23	(49.03)	18.27	(38.68)	41.11	(49.2)	23	(42.1)
n	15,557		558		34,944		952	

Notes: Standard errors are in parentheses; MA=Massachusetts.
Source: 2004 Full Panel and 2008 wave1-10 of Survey of Income and Program Participation

Table 2.2 Estimates within Massachusetts (MA)

Pre vs. Post within MA weighted (03-05 vs. 08-09)

Panel A: Full sample

	SSDI				
	SIPP				SSA
	weighted		un-weighted		
Health reform effect	0.0056***	0.0058***	0.0035***	0.004***	0.0055***
Robust S.E	[0.0008]	[0.0008]	[0.0008]	[0.0008]	[0.00099]
Control covariates	No	Yes	No	Yes	
Observations	101,182		101,182		5

Panel B: Married sample

	SSDI		Spouse w/o ESI		Elasticity
	weighted		weighted		
Health reform effect	0.0042***	0.004***	0.04793***	0.0438***	0.0913
Robust S.E	[0.0012]	[0.0012]	[0.0052]	[0.0049]	
Control covariates	No	Yes	No	Yes	
Observations	39,036		39,036		

Panel C: Married sample

	DD within MA			
	SSDI rolls (Logit)			
	ME			ME
Health reform effect	1.6001***	0.0087***	1.6213***	0.0098***
Robust S.E	[0.4477]	(0.0018)	[0.448]	(0.0022)
Control covariates	No		Yes	
Observations	39,036		39,036	

Notes: 1. robust standard errors in square brackets. ME: Marginal effect based on Ai and Norton, 2003.
*** Significant at the 1 percent level, two-tail test.
** Significant at the 5 percent level, two-tail test.
* Significant at the 10 percent level, two-tail test.

Table 2.3 Difference-in-Difference estimates between Massachusetts and neighboring states

	DD (weighted) (03-05 vs. 08-09)			
	SSDI rolls		Spouse w/o ESI	
Panel A: Full sample				
6 control states				
	n=432,532		n=432,532	
	0 .0051***	(0.0009)	0.0261***	(0.0027)
4 control states				
	n=188,162		n=188,162	
	0 .0059***	(0.0011)	0 .0084***	(0.0034)
2 control states				
	n=345,552		n=345,552	
	0.0048***	(0.001)	0.0316***	(0.0028)
Panel B: Married sample				
6 control states				
	n=169,630		n=169,630	
	0.0024*	(0.0015)	0.0303***	(0.0063)
4 control states				
	n=73,834		n=73,834	
	0 .0115***	(0.0015)	0 .0176**	(0.0077)
2 control states				
	n=134,832		n=134,832	
	0.00007	(0.0016)	0.0349***	(0.0065)

Notes: 1. Standard errors in parentheses.
6 control states=NH, VT, RI, CT, NJ, and PA
4 control states=NH, VT, RI, and CT
2 control states= NJ, and PA
*** Significant at the 1 percent level, two-tail test.
** Significant at the 5 percent level, two-tail test.
* Significant at the 10 percent level, two-tail test.

Table 2.4 Difference-in-Difference-in-Difference estimates

	DDD (weighted) (03-05 vs. 08-09)			
	SSDI rolls			
	Full sample		*Married sample*	
6 control states				
	n=432,532		n=169,630	
	0.0129***	[0.002]	0.0113***	[0.0025]
4 control states				
	n=188,162		n=73,834	
	0.0077***	[0.0035]	0.005	[0.0033]
2 control states				
	n=345,552		n=134,832	
	0.0147***	[0.0022]	0.0136***	[0.0028]

Notes: 1.Robust standard errors in square brackets.
6 control states=NH, VT, RI, CT, NJ, and PA
4 control states=NH, VT, RI, and CT
2 control states= NJ, and PA
*** Significant at the 1 percent level, two-tail test.
** Significant at the 5 percent level, two-tail test.
* Significant at the 10 percent level, two-tail test.

Table 2.5 Robustness checks

Panel A: False treatment state

	DD within			
	SSDI (Logit)			
	New Jersey	ME	Pennsylvania	ME
Health reform effect	0.1472	-0.0008	-0.4102**	-0.0057**
Robust S.E	[0.1782]	(0.0027)	[0.1807]	(0.0028)
Control covariates	Yes		Yes	
Observations	48,496		47,300	

Panel B: False treatment time

	Pre vs. Post within MA (weighted)			
	SSDI			
	(03-04 vs. 05-09)		(03 vs. 04-09)	
Health reform effect	-0.0005	-0.00067	-0.0088***	-0.0092***
Robust S.E	[0.0009]	[0.0009]	[0.0021]	[0.0029]
Control covariates	No	Yes	No	Yes
Observations	39,036		39,036	

Notes: 1. Robust standard errors in square brackets. ME: Marginal effect based on Ai and Norton, 2003.
*** Significant at the 1 percent level, two-tail test.
** Significant at the 5 percent level, two-tail test.

Table 2.A1 SSDI rolls aged 18-64 form SSA

Massachusetts	2003	2004	2005	2006	2007	2008	2009
Beneficiaries	157,173	163,210	169514	174,940	180,907	188,327	196,040
Resident population	4,089,322	4,097,973	4,087,881	4,132,347	4,157,960	4,199,836	4,266,071
	3.84%	3.98%	4.15%	4.23%	4.35%	4.48%	4.60%

Source: SSA administrative data

CHAPTER 3. How Did the Massachusetts Health Reform Affect the SSI-disabled Program?

1. Introduction

In the U.S., the public health insurance is particularly important to the people who have difficulties purchasing private health insurance coverage. Usually, the public health insurance is closely tied to the eligibility for the federal disability programs such as Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI). For example, SSDI beneficiaries receive Medicare while SSI beneficiaries receive Medicaid. These programs require a rigorous screening process for disability and the applicants usually need to show that they are physically incapable of finding suitable jobs. Therefore, the recipients typically choose to withdraw from the labor force in order to obtain the public health insurance coverage. These scenarios may lead to potential employment disincentives for working-age people with disabilities.

However, after the 2006 Massachusetts health reform, the public health insurance expansion creates significant employment incentives for potential SSI applicants. Several observations support the idea that expanding insurance coverage will reduce welfare program participation. First, the fully phased-in Medicaid reforms increased the probability of working in the labor force by 0.9 percentage points and Medicaid expansion reduced the probability of Aid to Families with Dependent Children (AFDC) program participation by 1.2 percentage points (Yelowitz, 1995). Second, the Qualified Medicare Beneficiary (QMB) program reduced SSI participation for the elderly (Yelowitz, 2000). Third, the Medicaid buy-in can break or weaken the link between health insurance and SSI eligibility for people with disabilities (Goodman et al., 2007) and, particularly in Massachusetts where the percentage of buy-in program enrollees working above substantial gainful activity is more than 60 percent (Hanes and Folkman, 2003).

Expanding insurance coverage could reduce SSI-disabled applicants' incentive by providing Medicaid and by maintaining their current jobs without withdrawing from the labor force.

There is a voluminous amount of literature to support the argument that expanding publicly subsidized health insurance leads to reduced welfare participation. However, there is little evidence about how insurance influences the SSI-disabled program participation and, more specifically, whether initial SSI-disability claims are affected. This is an important shortcoming in the literature because part of the support for expanding publicly subsidized health insurance comes from the belief that it will be de-linking health insurance and SSI eligibility.

I estimate the causal effect of the reform on SSI application decisions using three administrative data sources (Social Security Administration, Centers for Medicare & Medicaid Services and U.S. Cancer Statistics) and one national survey database, March Current Population Survey. Collecting data from these sources allow me to investigate the change before and after the implementation of health reform on the caseload of initial claims of SSI-disabled claimants, Medicaid recipients, cancer incidence counts and SSI-disabled beneficiaries.

I evaluate the effect of the Medicaid expansion on the change of initial SSI claim rate using the near-universal expansion of health insurance coverage in Massachusetts. In 2006, Massachusetts simultaneously mandated that all state residents must have insurance (or lose a personal income tax exemption with additional monthly penalties) and dramatically increased free and subsidized insurance for low- and middle-income residents. I perform a behavioral analysis that exploits the variation in the intensity of the impact of the reform across SSI-disabled applicants.

Potential SSI-disabled applicants who are affected by the Medicaid expansion will have much less incentive to move onto SSI rolls than their counterparts who are not affected by the

Medicaid expansion. Additionally, I compare potential SSI-disabled applicants in Massachusetts to similar counterparts in the neighboring states that do not implement health reform. Exploiting the variation in treatment intensity allows me to identify how expanding public insurance coverage affected the initial SSI claim rate in a way that is robust to Massachusetts-specific time trends.

I have several findings. I find that the health reform reduced the initial SSI claim rate by 0.098 percent (equivalent to 11.66 percent of total claims in 2008 Massachusetts). The result suggests that the initial SSI claim rate is quite sensitive to insurance status. These results also show that Medicaid-disabled expenditure can be saved around 1 percent and suggest inefficiencies in Medicaid-disabled expenditure in Massachusetts. However, spending $1 on the Medicaid-disabled could save $0.016 for individuals who get into SSI for the first time and $0.032 for individuals who receive SSI for multiple spells.

These findings have consequences for the cost of health care reform. Expanding eligibility for Medicaid could result in reduced expenditures for the current SSI program by giving potential disabled applicants less incentive to apply for SSI. Reducing caseloads could reduce current SSI expenditures and increase taxable revenue due to an increase in work hours. If Medicaid is an important determinant of the volume of SSI applications, then offering health insurance without participating in SSI may reduce total cost. This could happen when disabled adults are willing to forgo the cash benefits from SSI.

The rest of the paper is laid out in five sections. Section 2 gives an overview of the SSI program in the US and the Medicaid expansion in Massachusetts. This is followed by Section 3, which discusses the data and empirical strategy. Section 4 presents the results for the impact of

health reform on public insurance coverage, while Section 5 shows the results for the impact of health reform on SSI participation decisions. Section 6 concludes.

2. Supplemental security income, Medicaid expansion, and the 2006 Massachusetts reform

For the purposes of SSI eligibility, disabled individuals are those 'unable to engage in any substantial gainful activity (SGA) because of a medically determined physical or mental impairment expected to result in death or that has lasted, or can be expected to last, for a continuous period of at least 12 months.' Eligibility for benefits is determined on a monthly basis. SSI recipients are required to have their nonmedical eligibility factors reviewed periodically (e.g. every 1 to 6 years), depending on their situation. In addition to the nonmedical reviews, medical reviews are conducted on disabled recipients to determine whether or not they continue to be disabled, and are performed most frequently on disabled recipients whose medical conditions are considered likely to improve. Medical reviews are required for disabled recipients when earnings of recipients exceed the SGA level.

As for Medicaid eligibility, certain qualifications must be met regarding age, whether applicants are pregnant or disabled; applicants' income and resources; and whether applicants are U.S. citizens or lawfully admitted immigrants. The rules for calculating applicants' income and resources vary from state to state and from group to group. Assets and resources are also tested against established thresholds. Categorically needy persons who are eligible for Medicaid may or may not also receive cash assistance from the SSI program. Because of excessive medical expenses, medically needy persons who would be categorically eligible except for income or assets may become eligible for Medicaid.

Due to health reform in Massachusetts, individuals eligible under the Medicaid Demonstration program can have access to health care services through several pathways. The mandatory and optional Medicaid State plan populations determine their eligibility by reviewing the applicable Medicaid laws and regulations. State plan eligibilities are included in the Medicaid Demonstration program in order to generate savings to provide benefits to expansion populations. Table 3.1 lists all SSI potential qualifiers for applicants ages 19 to 64 who might get Medicaid via MassHealth without applying for SSI under the pre- and post-reform guidelines (the MassHealth Medicaid Demonstration defines 18-year-olds as children). These groups in Table 3.1 can increase their earning level without losing public insurance and avoid the wait involved in a SSI application if they value Medicaid more than cash assistance.

3. Data and empirical strategy

3.1 Data

First, Social Security Administration (SSA) refers to the first filing as an "initial claim" when a state agency first reviews a claim for disability benefits. For the initial claim rate of SSI from SSA, this dataset only includes disability claims sent to a state agency for determining disability criteria. Disability claims that do not meet the non-disability criteria are normally denied without being sent to a state agency. If SSA determines that non-disability criteria were not met while a claim is pending in a state agency, then claims pending in a state agency will be returned to SSA without a determination.

Second, SSA refers to simultaneous as a "concurrent claim" filing when the same person files a SSI claim and a Social Security Disability Insurance (SSDI) disability claim. When an applicant applies for both SSI and SSDI benefits, that claim is normally counted only once

because both types of claims are processed together. The division of claims between SSI and concurrent claims can provide the socioeconomic backgrounds of applicants. SSI applicants do not have a recent work history, and have little or no income and resources. Concurrent applicants have a recent work history, but also have scant income and resources. Claims filing analysis can be accomplished by comparing concurrent cases, SSI cases, and aggregate SSI cases.

Third, U.S. Cancer Statistics (USCS) is the official federal collection of statistics on cancer incidence from registries with high-quality data for the United States. Incidence data are provided by The Centers for Disease Control and Prevention National Program of Cancer Registries. Cancer incidence data are available for the United States and individual states by age group, race, gender, childhood cancer classifications, and cancer site for the years 1999 to 2008.

Fourth, Current Population Survey (CPS) is a nationally representative household survey of the US civilian, noninstitutionalized population, collecting monthly information on labor market characteristics. In addition to those data, the CPS includes an Annual Social and Economic Supplement (ASEC), conducted mostly in March, which collects detailed information on income and health insurance coverage. With an annual sample size of about 60,000 households, the CPS ASEC provides relatively large samples for many states, including Massachusetts. However, given the focus on a small group of the population, the sample size for this analysis is relatively small.

3.2 Empirical strategy

This analysis takes advantage of the “natural experiment” that occurred in Massachusetts to compare the change in the initial claim rate of SSI-disabled applicants ages 18 to 64 before and after the state implemented its health reform initiative, while using Difference-in-Difference (DD)

and Difference-in-Difference-in-Difference (DDD) methods. The estimation approach exploits variation over time (comparing pre-and post-reform time periods), across population groups (comparing SSI-disabled applicants who are affected by Medicaid expansion to SSI-disabled applicants who are not affected by Medicaid expansion), and across states (comparing Massachusetts to comparison states in the Northeast that did not implement health reform).

This paper relies on three administrative data sets from 2003 to 2008 for the caseload of initial claims of SSI-disabled claimants from SSA, Medicaid recipients from Centers for Medicare & Medicaid Services (CMS), and cancer incidence counts from USCS combined with population data from the Census Bureau (CB) to construct panel data and one national survey database, March CPS. I rely on data for 2004-2008 from 2005-2009 CPS.

Defining the SSI Beneficiaries-Disabled Status[14]—CPS respondents are asked in March to report the reason why they received SSI benefits over the prior calendar year. In the CPS, individuals are classified as SSI-disabled beneficiaries only if they report having SSI benefits at any point over the prior calendar year because they were disabled. However, the SSI-disabled beneficiary rate in the CPS aligns more closely to point-in-time estimates than full-year estimates.

Defining the Pre-and Post-Reform Periods—Since these datasets were calculated based on "Calendar year," I am limited in my ability to align the pre-and post-reform periods with the exact timing of reform implementation. Therefore, I define the pre-and post-reform periods based on the year, rather than the month, that Massachusetts implemented reform. Even though some of the initial reform efforts went into effect in October 2006, my post-reform period using the SSA, USCS, CMS, and CPS begins in 2007. After defining the pre-and post-reform periods, I

[14] The question reads, "What were the reasons (you/name) (Was/were) getting supplemental security income last year?" The five coded responses are: "Disabled (adult or child)," "Blind (adult or child)," "On behalf of a disabled child," "On behalf of a blind child," "Other (adult or child)," For disabled beneficiaries, I restrict the attention on Disabled (adult or child).

then compared the initial claim rate of SSI-disabled applicants, incidence rate of cancer patients, percentage of Medicaid recipients, and SSI-disabled beneficiaries in the post-reform period of 2007–2008 to those applicants, patients, recipients, and beneficiaries in the pre-reform period of 2003–2004.

Defining the Comparison States—The comparison states provide an estimate of what would have happened in Massachusetts in the absence of health reform. Identifying an appropriate comparison state is difficult given the wide variation in state policies, programs and populations, and the frequency with which other states were also implementing program and policy changes that affected SSI-disabled applicants over the study period. In this paper, I rely on four states in the New England division (1.New Hampshire, 2.Vermont, 3.Rhode Island, 4.Connecticut) and two states in the Middle Atlantic division (5.New Jersey, 6.Pennsylvania) in the Northeast region as the comparison states.

Defining the Comparison Group—The comparison group provides an estimate of what would have happened in the absence of the Medicaid expansion within Massachusetts. Potential control groups include SSI-Disabled Children (SSI-DC) and people who have been diagnosed with cancer. Due to comprehensive Medicaid expansion for children in Massachusetts, I will not use SSI-DC as the control group because SSI-DC might be affected significantly. To focus more on adults instead, I decide to use cancer patients ages 20 to 64 (USCS divides age categories into 5-year blocks, so 20-24 is the youngest data block which only covers adults). Since the age range between the control group and the treatment group is similar, these two groups should be more comparable. The control group includes all genders, all ethnicities, all races, and all types of cancer.

To estimate the overall impact of health reform on SSI-disabled applicants, using a DD framework I compared the change in the initial claim rate of SSI-disabled claimants in Massachusetts to the change in the initial claim rate for a similar group in a comparison state before and after the reform. The comparison states control for underlying trends in the initial claim rate of SSI-disabled unrelated to health reform. Furthermore, I extend the analysis by comparing the DD estimate on the potential SSI-disabled applicants who are affected by Medicaid expansion to an analogous DD estimate on the potential SSI-disabled applicants who are not affected by the Medicaid expansion using a DDD framework.

3.2.1 DDD estimate

I will label the two time periods as one and two, let *MA* represent the state implementing the policy, and let *Medicaid* denote the potential SSI-disabled applicants who are affected by Medicaid expansion. The coefficient of interest is now π_3, the coefficient on the triple interaction term, $T2 \cdot MA \cdot Medicaid$.

$$y = \alpha_0 + \alpha_1 MA + \alpha_2 Medicaid + \alpha_3 MA \cdot Medicaid + \pi_0 T2 + \pi_1 T2 \cdot MA + \pi_2 T2 \cdot Medicaid + \pi_3 T2 \cdot MA \cdot Medicaid + u \quad (1)$$

where y is the outcome of interest. The dummy variable, *Medicaid*, captures possible differences between the treatment and control group. The time period dummy, *T2*, captures aggregate factors that would cause changes in y even in the absence of a policy change. The dummy variable, *MA,* captures possible differences between the treatment and control state, which is non-policy state.

3.2.2 DD estimate across states

If I drop the *Medicaid* terms from Eq. (1), I will obtain the DD estimate described in the following:

$$y = \beta_0 + \beta_1 MA + \beta_2 T2 + \beta_3 MA \cdot T2 + u \quad (2)$$

The coefficient of interest is now β_3, the coefficient on the interaction term, $T2 \cdot MA$, which is the same as a dummy variable equal to one for those observations in the treatment state in the second period.

3.2.3 DD estimate within Massachusetts

On the other hand, if I drop the *MA* terms from Eq. (1), I will get another DD estimate displayed in the following:

$$y = \delta_0 + \delta_1 Medicaid + \delta_2 T2 + \delta_3 Medicaid \cdot T2 + u \quad (3)$$

The coefficient of interest is now δ_3, the coefficient on the interaction term, $Medicaid \cdot T2$, which is the same as a dummy variable equal to one for those observations in the treatment group in the second period.

4. The impact of the 2006 health reform on public insurance coverage

Table 3.2 presents the percentage of Medicaid recipients across states and years. The percentages of all states rise gradually by year, except Massachusetts, which increase by 7.82 percent from

2003 to 2008. By constructing panel data with this information and running regression analysis, I further investigate the percentage change of Medicaid recipients in the treatment state in a regression framework using Eq. (2), and expect to see positive significant coefficients because more intensive health reform can be anticipated to raise incentives to get Medicaid since state governments are expanding eligibility and providing premium subsidies to potential applicants.

Table 3.4 demonstrates estimates of the interaction term between the "treatment state" and "after health reform" for the percentage of Medicaid recipients for all adults. As expected, in column 2, the coefficients of interest are positive and significantly different from zero.

5. The impact of the 2006 health reform on initial SSI claim rate

5.1 The effect of Medicaid expansion on SSI-disabled claimants

Table 3.3 lists the caseload of initial claims for SSI-disabled and resident populations ages 18 to 64. Then I calculate the initial claim rate by dividing the caseloads by the corresponding population. In Table 3.3, only the percentage for Massachusetts decreases by 0.13 percent from 2003 to 2008. One potential explanation is that from 2005 to 2008, the economy was booming which might make the potential SSI-disabled applicants not apply due to higher opportunity cost. Therefore, I use the DD estimate across states to eliminate the business cycle factor between states and reveal the effect of health reform on SSI-disabled applicants.

Next, with the initial claim rate across states and years, I can construct panel data and proceed to assess the behavior of SSI-disabled applicants in the treatment state in a regression framework using Eq. (2). Table 3.4 shows estimates of the interaction term between the "treatment state" and "after health reform" for the initial claim rate of SSI-disabled applicants ages 18 to 64. In column 1, surprisingly, for initial claim rate of SSI-disabled applicants, the

coefficient of interest is negative and significantly different from zero. These findings suggest that potential SSI-disabled applicants ages 18 to 64 might have significantly less incentive to apply for SSI after health reform.

In the SSI program, reasons for applying for SSI might be to gain access to cash assistance and Medicaid. Therefore, scenarios explaining the significant caseload decline include the following possibilities. First, if potential disabled applicants have relatively higher incomes, they may not have enough incentive to apply because the cash assistance does not attract them. Second, these applicants may only need Medicaid without cash benefits because they value health insurance more. During the reform, Massachusetts expanded Medicaid income eligibility comprehensively and provided premium subsidies to both qualifying small employers and their low-income employees for the purchase of private health insurance. Thus, these applicants might have less incentive to apply for SSI because they can get Medicaid easily without participating in SSI.

5.2 March Current Population Survey results

After using DD and DDD estimators to confirm the hypotheses via information from the administrative database, I apply CPS-ASEC to assess the behavior of SSI-disabled beneficiaries ages 18 to 64 in the treatment state in a regression framework using Eq. (2). I expect to see that the coefficients should be negative because the caseload of initial claims of SSI-disabled applicants dropped significantly, which might make the number of SSI-disabled beneficiaries decrease. These results suggest that more intensive health reform might have led individuals to have less incentive to apply for SSI. Table 3.4 presents estimates of the interaction term between the “treatment state” and “after health reform” for the SSI-disabled beneficiaries. As expected, in

column 3, SSI-disabled beneficiaries, the coefficients of interest are negative and significantly different from zero.

5.3 Labor force participation for low skill workers

Since Massachusetts implemented near-universal health reform, especially the expansion of Medicaid income eligibility, I expected to see that the rate of labor force participation for low skill workers increase, and potential SSI-disabled applicants have less incentive to apply for SSI, which was confirmed by the results in Table 3.4. Next, Table 3.5 confirms that the labor force participation of low skill workers in Massachusetts increased.

For example, in the Northeast region, Massachusetts is the only state in which both the number of Temporary Assistance for Needy Families (TANF) participants with work requirements and the number of TANF participants who met work requirements increased significantly from 19 percent in FY2007 to 45.1 percent in FY2008, which is much higher than the national average (29.8 percent in FY 2008). Moreover, the results indicate that the Deficit Reduction Act (DRA)'s reauthorized TANF with changes in the work requirement in 2007 may not be the main reason for the increased labor force participation of low-income families. Both the number of TANF participants with work requirements and the number of TANF participants who met work requirements in other neighboring states decreased significantly except in Rhode Island, where the number of TANF participants with work requirements increased slightly.

5.4 Within Massachusetts analysis

Next, I want to further investigate the effect of Medicaid expansion on SSI-disabled applicants by using the control group to see if the Medicaid expansion made the caseload of SSI-disabled

applicants drop. This group could be the potential SSI-disabled applicants and this group is not affected by the Medicaid expansion, which means that the percentage of this group should not grow significantly.

In Table 3.6, I use the incidence counts divided by the approximate population ages 18 to 64 to get the incidence rate across states and years. Due to a lack of data in USCS, I was unable to get the data for Vermont for ages between 25-29 in 2005 and 2006, and 20-24 in 2004. Connecticut was not included in the national data.

Therefore, in order to estimate the impact of the Medicaid expansion in Massachusetts, I investigate the behavior of SSI-disabled applicants in the treatment group in a regression framework using Eq. (3). Thus, I compare changes over time in initial claim rates of SSI-disabled applicants ages 18 to 64 to changes over time in incidence rate of cancer in patients ages 20 to 64. I expect to see negative significant coefficients because a more intensive Medicaid expansion might have led potential SSI-disabled applicants who are affected by Medicaid to have much less incentive to move onto SSI rolls. Table 3.7 presents estimates of the interaction term between the "treatment group" and "after health reform" for the initial claim rate of SSI-disabled applicants ages 18 to 64. As expected, the coefficients of interest are negative and significantly different from zero. This coefficient shows that the total claims were reduced by 11.66 percent.[15] This table also shows that Medicaid-disabled expenditure can be saved up to $37.45 million, which is 0.82 percent and suggests inefficiencies in the Medicaid-disabled expenditure in Massachusetts.[16] However, spending $1 on the Medicaid-disabled could save $0.016 for individuals who get into SSI for the first time and $0.032 for individuals who receive

[15] (0.00098/0.0084)*100%=11.66%

[16] $9,100*0.098%*4.2M=37.45M; Medicaid Payments per Enrollee-Disabled in Massachusetts:9,100, FY 2008 available online at http://www.statehealthfacts.org/profileind.jsp?ind=183&cat=4&rgn=23&cmprgn=1; $37.45M/$4,571M=0.82%; Medicaid Payments of Disabled group in Massachusetts: $4,571 Million, FY 2008 available online at http://www.statehealthfacts.org/profileind.jsp?cmprgn=1&cat=4&rgn=23&ind=858&sub=47

SSI for multiple spells (See appendix A for description of calculation).[17] This indicates that it results in saving but that is not much in the grand scheme of government expenditures. The net cost of between Medicaid-disabled program and SSI-disabled program would be 4494.06 Million (4,571-76.94).

Furthermore, I want to confirm the results of Table 3.7 by looking at what happens to SSI-disabled applicants who are affected by the Medicaid expansion in the treatment state after health reform via a regression framework using Eq. (1). I expect to see that the coefficients should be negative. These results will confirm that following the health reform in the treatment state and in the treatment group, potential SSI-disabled applicants ages 18 to 64 might have less incentive to apply for SSI. Column 2 of Table 3.7 presents estimates of the triple interaction term among "treatment state", "treatment group", and "after health reform" for the initial claim rate of SSI-disabled applicants ages 18 to 64. As expected, the coefficient of interest is negative.

5.5 Robustness checks

As a placebo test, I estimate the effect of the reform as if it had occurred in other states. This test allows me to evaluate how likely it is to find a "false positive" when studying the Massachusetts reform. If I were to find a significant effect even in states that had not enacted a major health care reform, it would signal that the effects estimated in Massachusetts might be spurious. I perform these placebo tests using the SSA and CMS from the comparison states of New Hampshire, Vermont, Rhode Island, Connecticut, New Jersey, and Pennsylvania. Specifically, I estimate

[17] $7,528*0.098%*45.15%*5.5*4.2M=76.94M; 76.94/4,571=0.0168; $7,528*0.098%*45.15%*10.5*4.2M=146.89M; 146.89/4,571=0.0321

$$y = \gamma_0 + \gamma_1 STATE + \gamma_2 T2 + \gamma_3 STATE \cdot T2 + u \qquad (4)$$

The dummy variable, *STATE,* captures possible differences between the treatment and control states prior to the policy change. The coefficient of interest is now γ_3, the coefficient on the interaction term, *T2·STATE.*

Table 8 presents the results. I find a significant reduction in initial SSI claim rate as a result of the reform in Massachusetts. However, in all other states I do not find a negative statistically significant effect. The absence of an effect in the placebo states provides some evidence that the results presented in the main text are due to the law in Massachusetts rather than a random fluctuation in initial SSI claim rate.

6. Discussion and Conclusion

This paper is among the first to analyze how insurance induces people to substitute between Medicaid and SSI-disabled program. I study the 2006 Massachusetts health insurance reform to evaluate the impact of insurance on the initial SSI claim rate. In 2006, Massachusetts introduced legislation requiring that all state residents have health insurance coverage. I compare changes in the initial SSI claim rate both across potential SSI-disabled applicants in Massachusetts and between Massachusetts and other states to identify the causal effect of the law. The effect of the law on insurance coverage makes the initial SSI claim rate decreases significantly.

A one-percentage point increase in the public health insurance predicts a 0.028 percentage point reduction in initial SSI claim rate.[18] My estimate implies that the law reduced the initial SSI claim rate by 0.098 percent. The result suggests that initial SSI claim rate is quite sensitive to insurance status. Furthermore, I find that the reform could result in reduced

[18] From column 1 of Table 4, 0.157/5.62=0.028

expenditure for the current SSI program by encouraging potential disabled applicants not to move onto SSI rolls. The reduction in caseload could reduce current SSI expenditures and means that low skill workers are increasing hours of work. However, the Medicaid-only program might provide another incentive for some disabled adults who were not previously participating in SSI because of the stigma associated with the program. In this scenario, it could increase costs (Yelowitz, 1998). This might already be happening through the Medically Needy (MN) program, which in Massachusetts does not have an income limit for noninstitutionalized people with disabilities (Kaiser Family Foundation, 2003). Since the MN program has fewer covered services under Medicaid than for categorically needy recipients, it may not provide enough incentive for the disabled not to apply for SSI.

Finally, these results also show on one hand, that Medicaid-disabled expenditure can be saved up to $37.45 million, which is 0.82 percent and suggest inefficiencies in Medicaid-disabled expenditure in Massachusetts. However, spending $1 on the Medicaid-disabled could save $0.016 for individuals who get into SSI for the first time and $0.032 for individuals who receive SSI for multiple spells. Under current budget pressures, the Medicaid expansions and subsidies to purchase coverage mandated by the new Patient Protection and Affordable Care Act, the federal government may improve the ability to manage the costs of the Medicaid and SSI programs in the future.

This project also speaks to the larger issue of the impact of insurance on welfare program participation. While much literature has shown that insurance coverage decreases the participation of welfare program (e.g., AFDC; SSI-aged) generally, this study provides direct evidence that public insurance expansion may also lead potential SSI-disabled applicants to

increase their earning level without losing Medicaid and saving the waiting time involved in a SSI application process.

Measuring the causal impact of insurance is notoriously difficult because it requires finding exogenous sources of variation in insurance status. The natural experiment in this paper is a particularly relevant source of credible exogenous variation to study because it represents the same type of insurance expansion program that recently occurred at the federal level with the Patient Protection and Affordable Care Act. By analyzing the impact of the Massachusetts health reform on the initial SSI claim rate, this research contributes to the ongoing debate about the role of health insurance subsidies and individual mandates in public policy.

Appendix for chapter 3

A. Saving on SSI expenditure calculation

Saving on SSI expenditure for considering only first spell = (Reduced initial claim rate)*(Average allowance rate)*(Average SSI payment)*(Mean length of all first spell)*(Massachusetts resident population). Furthermore, Saving on SSI expenditure for considering all spells = (Reduced initial claim rate)*(Average Allowance rate)*(Average SSI payment)*(Mean length of all spells) *(Massachusetts resident population). Rupp and Scott (1995) show that mean length of all first SSI spells is 5.5 years; while multiple spells are accounted for, the projected mean total pre-retirement age SSI disability stay almost doubles to 10.5 years for all awardees. SSI Annual Statistical Report (2008) shows that total SSI payment for the disabled in 2008 is 37,245,543,000 and total recipients for ages 18-64 in 2008 are 4,947,475. Thus, I calculate average SSI payment per awardee is (37,245,543,000/4,947,475) =

7,528. As for the allowance rate of SSI initial claim, it is 45.15 percent after taking average on all seven states.

B. Data source

B.1. Population data ages 18-64

Calendar year	Source
2003	Annual Statistical Report on the Social Security Disability Insurance Program, 2003 pp. 144-145.
2004	Annual Statistical Report on the Social Security Disability Insurance Program, 2004 pp. 139-140.
2005	Annual Statistical Report on the Social Security Disability Insurance Program, 2005 pp. 149-150.
2006	Annual Statistical Report on the Social Security Disability Insurance Program, 2006 pp. 153-154.
2007	Annual Statistical Report on the Social Security Disability Insurance Program, 2007 pp. 155-156.
2008	Annual Statistical Report on the Social Security Disability Insurance Program, 2008 pp. 155-156.

B.2. Population data

U.S. Census Bureau, Population Division. 2011. Table 1. Preliminary Annual Estimates of the Resident Population for the United States, Regions, States, and Puerto Rico: April 1, 2000 to

July 1, 2010 (NST-PEST2010-01). Available at www.census.gov/popest/**states**/**tables**/**NST**-PE**ST2010**-01.xls

B.3. SSI-disabled caseload data

Social Security Administration (SSA), Office of Retirement and Disability Policy (ORDP), Office of Disability Programs (ODP), "SSA State Agency Monthly Workload Data." Baltimore, Maryland. Available at http://www.socialsecurity.gov/disability/data/SSA-SA-MOWL.xls

B.4. Medicaid data

Annual Statistical Supplement, various years. Available at http://www.ssa.gov/policy/docs/statcomps/supplement/

Massachusetts Medicaid Statistics, various years. Available at http://www.medicaid.gov/Medicaid-CHIP-Program-Information/By-State/Massachusetts.html

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U.S. Census Bureau, Population Division. 2011. Table 1. Preliminary annual estimates of the resident population for the United States, regions, states, and Puerto Rico: April 1, 2000 to July 1, 2010 (NST-PEST2010-01)

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Yelowitz, Aaron S., 1998. Why did the SSI-disabled program grow so much? disentangling the effect of Medicaid. Journal of Health Economics 17 (3), 321-349.

Yelowitz, Aaron S., 2000. Using the Medicare buy-in program to estimate the effect of Medicaid on SSI participation. Economic Inquiry 38 (3), 419-441.

Table 3.1 Medicaid covered population ages 19 to 64 under MassHealth

Before health reform		After health reform	
Base population	FPL	Expanded populations	FPL
Pregnant women ages 19 and older considered presumptively eligible	0-200%	Pregnant women ages 19 and older considered presumptively eligible	0-300%
Parents or adult caretaker relative living with their children under age 19	0-133%	Parents and caretaker relatives	0-300%
Disabled adults	0-133%	Disabled adults	0-133%
Parents and disabled nonworking adults	0-133%	Non-working disabled adults	Above 133%
		Higher income adults with disabilities working 40 hours a month or more	Above 133%
Long term unemployed individuals	0-100%	Long-term unemployed individuals or members of a couple and a client of Department of Mental Health (DMH) and/or receiving Emergency Aid to the Elderly, Disabled and Children (EAEDC)**	0-100%
		Long-term unemployed individuals or members of a couple, and **neither** a client of DMH or receiving EAEDC**	0-100%
		Families receiving unemployment benefits**	0-400%

Note: Presumptive eligibility is offered to certain children enrolled in MassHealth Standard and Family Assistance as well as pregnant women receiving services through the MassHealth Pre-Natal program. FPL=Federal Poverty Level.

Source: MassHealth Medicaid Section 1115 Demonstration, 2008. **Not otherwise eligible for medical assistance

Before health Reform		After health Reform	
Base population	FPL	Expanded populations	FPL
Individuals living with HIV positive	0-133%	Individuals living with HIV positive**	0-300%
Woman under age 65 with breast or cervical cancer	0-250%	Women eligible under the Breast and Cervical Cancer Treatment Program (BCCTP)	0-250%
		Individuals ages 19 and older with no access to ESI, Medicare, or other subsidized health insurance programs, including the following groups: (1) Low-income adults; (2) Adults working for an employer with 50 or fewer employees who offers no insurance or who contributes < 33% (or < 20% for family coverage) towards insurance costs	(1) 0-300%; (2) 0-300%
		Adults under the age of 65 who work for a qualified small employer and purchase ESI**	0-300%
		19 and 20 years olds	0-300%

Table 3.2 Summary of Medicaid recipients

	2003	2004	2005	2006	2007	2008
Treatment State						
Massachusetts	1,042,123	1,074,050	1,110,475	1,267,776	1,448,115	1,568,182
Residents	6,455,028	6,452,636	6,453,694	6,466,399	6,499,275	6,543,595
%	16.14%	16.65%	17.21%	19.6%	22.28%	23.97%
Control States						
Connecticut	496,680	500,952	520,660	517,529	518,675	524,210
Residents	3,468,319	3,474,379	3,477,185	3,484,531	3,488,084	3,502,664
%	14.32%	14.42%	14.97%	14.85%	14.87%	14.97%
Rhode Island	201,875	207,621	209,371	212,491	208,429	203,731
Residents	1,072,453	1,075,835	1,069,226	1,064,193	1,059,706	1,058,368
%	18.82%	19.30%	19.58%	19.97%	19.67%	19.25%
New Hampshire	112,044	119,207	120,760	126,458	126,074	131,056
Residents	1,282,146	1,292,566	1,301,050	1,311,184	1,316,496	1,320,981
%	8.74%	9.22%	9.28%	9.64%	9.58%	9.92%
Vermont	154,664	148,921	150,836	149,808	157,240	162,143
Residents	616,700	618,120	618,797	619,916	620,438	620,967
%	25.08%	24.09%	24.38%	24.17%	25.34%	26.11%
New Jersey	949,741	959,843	965,768	1,004,370	1,019,936	1,065,155
Residents	8,585,567	8,610,474	8,619,564	8,619,354	8,630,810	8,657,319
%	11.06%	11.15%	11.20%	11.65%	11.82%	12.30%
Pennsylvania	1,721,707	1,834,651	1,990,466	2,064,061	2,181,821	2,134,331
Residents	12,360,988	12,387,357	12,415,908	12,466,485	12,517,701	12,562,536
%	13.93%	14.81%	16.03%	16.56%	17.43%	16.99%

Sources: U.S. Census Bureau, Population Division. 2011. Table 1. Preliminary Annual Estimates of the Resident Population for the United States, Regions, States, and Puerto Rico: April 1, 2000 to July 1, 2010 (NST-PEST2010-01); Medicaid data are from Annual Statistical Supplement, 2003 to 2008, Medicaid: State Data; Massachusetts Medicaid Statistics, 2006 to 2008. The original sources are Medicaid Statistical Information System State Summary Data and Center for Medicare & Medicaid Services.

Table 3.3 Summary of initial claims of SSI ages 18 to 64

	2003	2004	2005	2006	2007	2008
Treatment State						
Massachusetts	39,645	35,032	35,169	35,630	34,935	35,461
Residents	4,089,322	4,097,973	4,087,881	4,132,347	4,157,960	4,199,836
%	0.97%	0.85%	0.86%	0.86%	0.84%	0.84%
Control States						
Connecticut	14,535	14,106	14,278	15,187	15,258	15,745
Residents	2,177,308	2,191,123	2,201,141	2,216,080	2,209,809	2,211,032
%	0.67%	0.64%	0.65%	0.69%	0.69%	0.71%
Rhode Island	4,285	4,251	4,921	6,126	5,992	6,552
Residents	681,318	686,232	681,060	682,193	677,870	674,602
%	0.63%	0.62%	0.72%	0.90%	0.88%	0.97%
New Hampshire	4,602	4,330	4,516	4,754	4,812	5,568
Residents	827,282	837,834	843,684	854,641	851,900	852,473
%	0.56%	0.52%	0.54%	0.56%	0.56%	0.65%
Vermont	3001	2765	2873	3073	3089	3125
Residents	401529	405,738	408,449	407,553	405,476	405,691
%	0.75%	0.68%	0.70%	0.75%	0.76%	0.77%
New Jersey	33,129	31,579	29,204	31,849	33,114	34,171
Residents	5,382,937	5,416,679	5,426,768	5,507,480	5,487,495	5,484,138
%	0.62%	0.58%	0.54%	0.58%	0.60%	0.62%
Pennsylvania	86,447	82,024	84,783	89,153	87,284	89,054
Residents	7,632,997	7,672,780	7,720,030	7,750,425	7,756,413	7,775,704
%	1.13%	1.07%	1.10%	1.15%	1.13%	1.15%

Notes: Author's calculations based on SSA and CB data.
Source: Caseload data are from SSA State Agency Monthly Workload Data; Population ages 18 to 64 data are from Annual Statistical Report on the Social Security Disability Insurance Program, 2003 to 2008; the original sources are Census Bureau, 2003 to 2008 resident population.

Table 3.4 DD estimates on initial claims rate of SSI, percentage of Medicaid recipients, and SSI-disabled beneficiaries

Before	After	DD					
		(1) SSA		(2) CMS		(3) CPS	
03-04	07-08	SSI		Medicaid		SSI-disabled	
6 control states		n=28		n=28		n=85,308	
		-0.00157***	(0.0005)	0.0562***	(0.0066)	-0.0075***	(0.002)
4 control states		n=20		n=20		n=52,436	
		-0.00188***	(0.0006)	0.06015***	(0.005)	-0.007***	(0.003)
2 control states		n=12		n=12		n=42,573	
		-0.00094**	(0.0004)	0.04832***	(0.009)	-0.0082***	(0.003)

Notes: 1. Standard errors are in parentheses. 2. Author's calculations are based on SSA, CB data and Annual Statistical Supplement, 2003 to 2008.

*** Significant at the 1 percent level, two-tail test.

** Significant at the 5 percent level, two-tail test.

* Significant at the 10 percent level, two-tail test.

Source: (1) SSA State Agency Monthly Workload Data; (2) Annual Statistical Supplement, 2003 to 2008, Medicaid: State Data. The original sources are Medicaid Statistical Information System State Summary Data and Center for Medicare & Medicaid Services; (3) 2005 to 2009 March Current Population Survey[19]

[19] Since the main matching variable, Household identification number (H_IDNUM) was renamed H_IDNUM1, and H_IDNUM2 beginning at 2004, I only use 2004 as pre-reform period for data matching consistency.

Table 3.5 Number of TANF families meeting work requirements in recent years

	TANF families			
	Before Deficit Reduction Act		After Deficit Reduction Act	
	FY 2005	FY 2006	FY 2007	FY 2008
USA				
Meet work requirements	295,294	269,679	263,092	243,026
With work requirements	874,798	807,710	870,140	815,877
%	33.80	33.4	30.2	29.8
Treatment State				
Massachusetts				
Meet work requirements	6,624	3,818	4,110	14,326
With work requirements	11,061	23,699	21,616	31,740
%	59.9	16.1	19	45.1
Control States				
Connecticut				
Meet work requirements	3,154	2,446	3,014	2,187
With work requirements	9,262	7,913	10,443	8,667
%	34.1	30.9	28.9	25.2
Rhode Island				
Meet work requirements	1,589	1,438	1,289	845
With work requirements	6,564	5,748	4,708	4,809
%	24.2	25	27.4	17.6

Note: TANF=Temporary Assistance for Needy Families. Numbers are average monthly numbers for families receiving TANF cash assistance. The percentages were calculated by Author.
Source: GAO-10-525 Report. The original source was from Department of Health and Human Services (HHS).

	TANF families			
	Before Deficit Reduction Act		After Deficit Reduction Act	
	FY 2005	FY 2006	FY 2007	FY 2008
Control States				
New Hampshire				
Meet work requirements	839	787	947	780
With work requirements	3,407	3,269	2,292	1,662
%	24.6	24.1	41.3	46.9
Vermont				
Meet work requirements	683	631	628	419
With work requirements	3,047	2,837	2,806	1,947
%	22.4	22.2	22.4	21.5
New Jersey				
Meet work requirements	7,460	7,150	6,766	3,702
With work requirements	25,427	24,440	20,486	19,625
%	29.3	29.3	33	18.9
Pennsylvania				
Meet work requirements	10,003	17,258	13,286	8,897
With work requirements	65,832	62,396	26,388	21,550
%	15.2	27.4	50.3	41.3

Table 3.6 Summary of cancer patients ages 20 to 64

	2003	2004	2005	2006	2007	2008
Treatment State						
Massachusetts	16,186	16,476	16,736	17,522	17,741	17,913
Residents	4,089,322	4,097,973	4,087,881	4,132,347	4,157,960	4,199,836
%	0.40%	0.40%	0.41%	0.42%	0.43%	0.43%
Control States						
Rhode Island	2,528	2,741	2,661	2,779	2,868	2,831
Residents	681,318	686,232	681,060	682,193	677,870	674,602
%	0.37%	0.40%	0.39%	0.41%	0.42%	0.42%
New Hampshire	3,305	3,329	3,434	3,634	3,519	3,639
Residents	827,282	837,834	843,684	854,641	851,900	852,473
%	0.40%	0.40%	0.41%	0.43%	0.41%	0.43%
New Jersey	20,689	20,966	21,214	22,361	22,832	22,438
Residents	5,382,937	5,416,679	5,426,768	5,507,480	5,487,495	5,484,138
%	0.38%	0.39%	0.39%	0.41%	0.42%	0.41%
Pennsylvania	29,865	30,613	31,001	32,294	33,233	33,567
Residents	7,632,997	7,672,780	7,720,030	7,750,425	7,756,413	7,775,704
%	0.39%	0.40%	0.40%	0.42%	0.43%	0.43%

Source: National Program of Cancer Registries: 1999 - 2008 Incidence, WONDER On-line Database. United States Department of Health and Human Services, Centers for Disease Control and Prevention and National Cancer Institute; 2011. Accessed at http://wonder.cdc.gov/cancernpcr-v2008.html on Aug 16, 2011 9:35:24 PM; Population ages 18 to 64 data are from Annual Statistical Report on the Social Security Disability Insurance Program, 2003 to 2008; the original sources are Census Bureau, 2003 to 2008 resident population.

Table 3.7 Medicaid expansion effect

Before	After	SSI			
03-04	07-08	(1) DD		(2) DDD	
Within Massachusetts		n=8		n=40	
		-0.00098***	(0.0003)	-0.00174	(0.001)

Notes: 1. Standard errors are in parentheses. 2. Author's calculations are based on SSA and USCS data.
*** Significant at the 0.1 percent level, two-tail test.
Source: National Program of Cancer Registries: 1999 - 2008 Incidence, WONDER On-line Database. United States Department of Health and Human Services, Centers for Disease Control and Prevention and National Cancer Institute; 2011. Accessed at http://wonder.cdc.gov/cancernpcr-v2008.html on Aug 16, 2011 9:35:24 PM

Table 3.8 Placebo tests

Dependent Vars.	(1) SSI	(2) Medicaid
Treatment State, (Six Control States)		
(1) Massachusetts, (CT,RI,NH,VT,NJ,PA)	-0.00157***	0.0562***
	(0.0005)	(0.0066)
(2) Connecticut, (MA,RI,NH,VT,NJ,PA)	-0.00023	-0.0158
	(0.0007)	(0.014)
(3) Rhode Island, (CT,MA,NH,VT,NJ,PA)	0.00278***	-0.0176
	(0.0004)	(0.0139)
(4) New Hampshire, (CT,RI,MA,VT,NJ,PA)	0.0000942	-0.0133
	(0.0007)	(0.014)
(5) Vermont, (CT,RI,NH,MA,NJ,PA)	-0.000145	-0.009
	(0.0007)	(0.0143)
(6) New Jersey, (CT,RI,NH,VT,MA,PA)	-0.00058	-0.0111
	(0.0007)	(0.014)
(7) Pennsylvania, (CT,RI,NH,VT,NJ,MA)	-0.00035	0.01082
	(0.0007)	(0.014)
Observations	28	28

Notes: 1. Standard errors are in parentheses. 2. Author's calculations are based on SSA data and Annual Statistical Supplement from 2003 to 2008.
*** Significant at the 0.1 percent level, two-tail test.
** Significant at the 1 percent level, two-tail test.
* Significant at the 5 percent level, two-tail test.
MA=Massachusetts; CT= Connecticut; NH= New Hampshire; NJ= New Jersey; PA= Pennsylvania; RI= Rhode Island; VT= Vermont.

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