ESSAYS IN LABOR ECONOMICS

by

Italo A. Gutierrez

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Doctoral Committee:

Professor Jeffrey Andrew Smith, Chair
Professor Charles C. Brown
Professor Brian P. McCall
Associate Professor Melvin Stephens Jr.

To God, my parents and Josianne.

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ABSTRACT

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Chair: Jeffrey Andrew Smith

My dissertation research extends the analysis of the effects of two major safety net programs, the unemployment benefits program and the Supplemental Nutrition Assistance Program (SNAP), beyond the traditional outcomes studied in the literature.

In the case of the unemployment benefits program, I analyze its effects on the behavior of employed workers rather than on unemployed individuals. I find evidence in Chapter I that, for older workers in the US, an increase in the potential replacement rate provided by unemployment benefits results in a decrease in the probability of searching on the job, which leads to a decrease in the probability of experiencing a job-to-job transition and an increase in the probability of transitioning into a jobless spell. The sizes of the estimated effects are larger for workers in downsizing firms. This finding is supported by the theoretical framework developed in my dissertation, which indicates that unemployment benefits would have stronger effects on the decisions of workers who are at higher risk of job loss.

Chapter II extends the analysis to the case of workers at imminent risk of layoff, using administrative data from establishment closures in West Germany. In this chapter, I study whether the potential duration of unemployment benefits, rather than their levels, has an

effect on workers' job search behavior when they arguably are aware of their impending job loss. I exploit changes in the rules for the duration of unemployment benefits to test the prediction (from the theoretical model developed in the chapter) that workers with longer benefits would be less likely to take a new job before their establishments close down. I find that the empirical evidence strongly supports this prediction.

In the case of SNAP, I study in Chapter III its impact on measures of material hardship beyond the standard focus on food security. I find that SNAP reduces not only food insecurity but also the risk of households falling behind on their non-food essential expenses including housing, utilities, and medical costs. These findings are important because SNAP has become the largest means-tested income transfer program in the US.

CHAPTER I

Do Unemployment Benefits Discourage On-The-Job Search? Evidence from Older American Workers[1]

1.1 Introduction

Previous work has shown that workers who are (or perceive themselves) at risk of layoff are more likely to search for a new job while still employed, which is called on-the-job search (OTJS). Workers who experience job insecurity may engage in OTJS in order to find a new job before their employment is terminated. In fact, job insecurity is the main reason for OTJS for a non-trivial fraction of workers. Fujita (2011) documents that the primary reason for OTJS for 12% of job seekers in the United Kingdom (2002-2009) is the fear of losing their jobs.[2] Similarly, Rosal (2003) documents that 27% of on-the-job seekers in Spain (2000) engaged in OTJS because of their job instability.[3]

Unemployment benefits (UB) provide temporary financial assistance for unemployed workers and, thus, reduce the economic burden of unemployment. Hence, they may also reduce the incentives to engage in actions to prevent falling into unemployment. One such

[1]This chapter has been funded in part with Federal funds from the U.S. Department of Labor under contract number DOLJ111A21738. The contents of this chapter do not necessarily reflect the views or policies of the Department of Labor or of any agency of the Federal Government, nor does mention of trade names, commercial products, or organizations imply endorsement of same by the U.S. Government.

[2]The source is the United Kingdom Labour Force Survey and the sample period is from the first quarter of 2002 to the first quarter of 2009.

[3]The source is the Spanish Survey of Economically Active Population (Encuesta de Poblacion Activa) from the second quarter of 2000.

action is OTJS. Only a few papers, Burgess and Low (1992, 1998) and Light and Omori (2004), have previously studied the relationship between UB and the job search behavior of employed workers. The results are as yet far from conclusive as will be described in section 1.2. In this chapter, information on older male American workers, from the Health and Retirement Study (HRS), is used to provide new evidence of whether UB has any effects on OTJS and of the magnitude of those effects.[4] This chapter tests the hypothesis that more generous UB reduce the probability of workers engaging in OTJS. The generosity of the UB is measured by the replacement rate, which is the fraction of earnings that UB would replace if the job is lost.

The main contributions of this chapter are threefold. First, the theoretical model predicts that the effect of UB on discouraging OTJS only exists if the worker is (or feels) at risk of job loss and, under some plausible conditions, the effect should be larger the higher the risk of job loss. Therefore, in this chapter, the effect of UB on OTJS is identified by focusing on a subgroup of workers who are likely to be at high risk of displacement. This group is composed of workers at downsizing firms, i.e. firms that have recently permanently reduced their labor force size. In fact, evidence presented in this chapter indicates that workers in downsizing firms have higher expected probabilities of job loss and actual higher predicted rates of transitioning into non-employment, even after taking into account observed worker and job characteristics. Previous papers have either not focused on "high risk" workers, finding very small effects (Light and Omori, 2004), or have examined only workers who had been ultimately displaced (Burgess and Low, 1992, 1998), which may be subject to sample composition bias. Second, this chapter studies the effect of UB not only on the probability of OTJS but also on employment status transitions, i.e. job-to-job (JTJ) transitions or job-to-non-employment (JTN) transitions. Previous papers have only studied the effect of UB on OTJS or on transitions, but not on both. Third, this chapter focuses on older workers, which is a subpopulation that has not been specifically studied before with

[4]The HRS consists of a nationally representative sample of adults over the age of 50 and is conducted every two years by the University of Michigan since 1992.

relation to the effect UB on OTJS.

The next section describes more in detail previous studies that have looked at how workers react towards the risk of displacement or job loss and unemployment and how this behavior can be changed by UB. Section 1.3 presents a simple model of OTJS and describes the main predictions that will be tested in the data. Section 1.4 describes the data sources and presents descriptive statistics on the main outcomes and covariates. Section 1.5 presents the econometric strategy to estimate the effect of the replacement rate on both the probability of OTJS and on monthly employment transitions probabilities. Section 1.6 presents the estimation results and section 1.7 concludes by comparing the findings of this chapter with previous results in the literature

1.2 Literature review

Previous work has shown that OTJS activities can change contingent upon expectations of job loss. Earlier papers on this issue analyzed the effects of layoff notifications on pre-displacement job search and on the probability of either avoiding or reducing the duration of unemployment spells. Burgess and Low (1992) found that 60.6% of male workers who received layoff notification performed OTJS, whereas only 38.9% of workers who did not receive such notification did so. Addison and Blackburn (1995) found that the main benefit of advanced layoff notifications was being able to locate a job without any intervening spell of unemployment (especially for white collar males) rather than reducing the length of jobless spells.

The more recent papers that relate job search to job loss expectations come from the study of worker flows in economically distressed firms. Lengermann and Vilhuber (2002) found, for the US, that highly qualified workers tend to voluntarily separate early from distressed firms before they close. Similar evidence was found by Schwerdt (2011) for the Austrian labor market. He found that a high fraction of all workers' separations from their employers, happening up to two quarters before a closure are directly related to "early leavers", or workers who decided to "abandon the sinking ship". These workers were more productive on average, as measured by higher earnings even after controlling for observed characteristics, than workers who stayed until the firm closure. Thus, as these studies show, plant closures that result in job loss rarely come as a surprise to workers. Moreover, workers also react in accord with the probability of these events by increasing their job search efforts and by leaving firms that are in economic distress.

The way workers react to the risk of job loss can be affected by different institutional arrangements. For example, severance pay requirements and UB reduce the financial cost of unemployment and can discourage workers from exerting effort to leave distressed firms and to avoid becoming unemployed. These effects, however, have been relatively understudied in the literature.

In the case of UB, most of the studies have focused on its effects on the duration of unemployment spells (see Meyer (1995) and Krueger and Meyer (2002) for surveys of the literature). Also, there is a relatively well-developed literature on the effect of the imperfect rated UB payroll tax on the probability of layoff (for example, see Feldstein (1976), Topel (1983), Topel (1984), Anderson and Meyer (1993), and Card and Levine (1994)). Conversely, only a few studies have directly studied the effect of UB on either OTJS or job transition outcomes.

The first set of studies on the effect of UB on OTJS was by Burgess and Low (1992, 1998). They found, using data from displaced workers in Arizona, that UB strongly discouraged pre-displacement job search (or OTJS) for workers who received advanced layoff notification and did not expect to be recalled by their employers. For example, a $10 increase in weekly unemployment benefits above the sample mean reduced the likelihood of OTJS by eight percentage points from 59.7% to 51.7%. Conversely, they found no statistically significant effect of UB on OTJS behavior for non-notified workers or for notified workers who expected to be recalled. Thus, UB only changed the behavior of workers who felt (relatively more) at risk of job loss. Although the results were compelling, two limitations threatened their validity, as acknowledged by the authors. First, there was a selection problem because the study sample was comprised of workers with at least 5 weeks of unemployment. Therefore, workers who performed more OTJS were less likely to be part of the sample. Second, since all respondents were unemployed individuals in Arizona in the years 1975-1976, there is no variation in UB other than that due to the workers' earnings histories that could be used to identify the effects. This could lead to potential biases in the estimated effects of UB. For example, earnings, and thus unemployment benefits, are plausibly positively correlated with productivity and job match quality, factors that standard job search theory predict would have a negative effect on OTJS. The resulting estimation bias can potentially inflate the negative effects of UB on OTJS behavior.

In a second study of the effect of UB on OTJS, Light and Omori (2004) formulated the theoretical arguments more formally and looked for evidence by analyzing JTJ transitions (originated by individual resigns or quits) and job-to-unemployment transitions using the 1979 National Longitudinal Survey of Youth (NLSY79).[5] If UB actually reduce incentives to perform OTJS, higher UB would be associated with a decline in JTJ transitions and an increase in job-to-unemployment transitions. Light and Omori found evidence of this effect, although its size was very small. Specifically, a one standard deviation increase above the mean in weekly UB reduced the probability of a JTJ transition in the next 15 weeks (for a worker with 30 weeks of tenure) from 0.047 to 0.045 (an elasticity of -0.09). An important drawback of this paper is that Light and Omori lacked a good measure of the risk of job loss. As will be discussed later, UB only reduces the value of OTJS if the worker feels vulnerable to job loss and, under certain plausible conditions, the effect is greater for workers who feel at a greater risk of job loss. Thus, without being able to focus on a subpopulation that has a relatively high level of job insecurity, Light and Omori's estimated coefficient captured the effect of UB for the average worker. Further, as

[5]The NLSY79 is a nationally representative sample of 12,686 young men and women who were 14-22 years old when they were first surveyed in 1979. These individuals were interviewed annually through 1994 and are currently interviewed on a biennial basis. The study is conducted by the US Bureau of Labor Statistics (BLS). Further information is available at http://www.bls.gov/nls/nlsy79.htm.

suggested by the survey responses in the Survey of Economic Expectations (SEE) and in the HRS, the average worker feels relatively secure in his job. Hence, it is not surprising that Light and Omori observed only relatively small effects of UB on reducing JTJ transitions.[6]

Finally, McCall (1995) studied the effect of UB on the probability of the unemployed workers' benefits take-up using the 1984, 1986, 1988, 1990 and 1992 Current Population Survey (CPS) Displaced Worker Supplement (DWS).[7] He found that increasing the replacement rate of wages increases the probability of UB take-up among the eligible. Although he does not look at the effect of UB on OTJS specifically, he included in his sample individuals reporting zero weeks of joblessness following a job displacement or job loss in order to account for the possibility that "an increase in UB will reduce the amount of OTJS and hence increase the possibility of incurring a positive spell of joblessness. This in turn, may lead to an increase in the probability of take-up [of UB]" (McCall, 1995, p. 190).

In this chapter the HRS is used to add new evidence on whether OTJS is discouraged by the generosity of UB. There are two main advantages of using the HRS. First, it contains information on whether the worker's employer has recently experienced a permanent reduction in employment. This variable is an important predictor of both workers' perceived expectations of job loss and of actual job loss or displacement. Thus, the effect of UB on a group of workers at high risk of displacement can be isolated. These workers are more likely to react to the incentives provided by UB. Second, HRS contains information on OTJS and, given its panel structure, it also contains information on workers' transitions. Therefore, the effect of UB on both the behavior of interest (OTJS) and on the policy

[6]In the SEE, the average value of the respondent's expected probability of job loss in the next twelve months is 11% for male workers aged 50 years or older (years 1994 through 1998). In the HRS, the average value is 15% (years 1996 through 2006). The SEE was a nationwide survey that examined how Americans in the labor force perceived their near-term economic future. The SEE questions were asked as a periodic module of the WISCON Survey, a project of the University of Wisconsin Survey Center. Further information is available at http://www.disc.wisc.edu/archive/econexpect/index.html. Also see Dominitz and Manski (2000).

[7]Data on displaced workers are collected by BLS every 2 years from a supplementary survey to the CPS . According to BLS website, "displaced workers are defined as persons 20 years of age and older who lost or left jobs because their plant or company closed or moved, there was insufficient work for them to do, or their position or shift was abolished". Further information is available at http://www.bls.gov/cps/lfcharacteristics.htm#displaced.

relevant outcomes (transition into another job or into unemployment) can be estimated.

1.3 Theoretical model

Mortensen (1977) showed that in a fully dynamic setting the effect of UB in relation to job search for unemployed workers is ambiguous. Increases in benefits have two opposing effects: 1) an increase of the value of being unemployed, and 2) an increase of the value of future employment since better paying jobs come with better UB – known as the “entitlement effect”. He also showed that the first effect reduces the incentives to search for a job and, thus, increases the length of the unemployment spell. This effect is more dominant at the beginning of the spell. The entitlement effect creates incentives for workers to search more because there is higher reward (in terms of better UB) for finding a good-paying job. Thus, this effect decreases the length of the unemployment spell and dominates when the worker is near the end of his benefit period.

UB has the same ambiguous effect on OTJS. On one hand, UB reduce the cost of falling into unemployment, but on the other hand, it increases the payoff from getting a higher-paying job since it comes with better unemployment benefits. Therefore, in theory, the effect of more generous UB on OTJS would be ambiguous, as well. However, for the demographic sample of this chapter, the second effect is less important for two reasons: First, since the entitlement effect is a forward-looking effect, it becomes less important for a worker the fewer remaining working years. The HRS samples individuals who are 50 years or older. Therefore, in the analysis sample for this chapter, the average (male) worker is 59 years old and, thus, the length of the remaining working years until retirement is relatively short.[8] Second, each state sets a maximum level of weekly unemployment benefits. The entitlement effect would not exist for workers whose potential UB (if they lost their jobs) are above that maximum level. Given that earnings usually increase with

[8]The sample for this chapter covers the years 1996-2006. Therefore the analysis period does not include the Great Recession, which has been associated with an increase in the length of time people continue to work.

experience (and age), 57% of workers in the sample have earnings that would put their UB above their state's maximum level. Thus, for these two reasons, a model that abstracts from the entitlement effect has been selected. A simple model was proposed by Light and Omori (2004). For this study, an adaptation of Light's and Omori's model is used and the analysis is expanded in order to derive predictions that will be tested in the empirical section. See section 1.8 for further discussion of the theoretical model.

The model consists of two periods.[9] In the first period, the worker is employed and earns w_1. The maximization problem of the worker is to decide if he wants to search on-the-job (or OTJS) or not. If the worker decides to search, then he has to pay a cost of k, which is distributed among workers with probability function $G()$. The flow utility is given by the logarithm of the available earnings and the cost of job search (if he searches on the job).

If a worker searches, the probability of receiving an offer in the second period equals λ. Offers come from a known distribution $F(w)$ and at most one offer can be received. The worker faces a probability of layoff in period two equal to p. If he gets laid off, he can collect UB equal to $r * w_1$, where w_1 is the current wage of the worker in period 1 and r is the effective replacement rate.[10]

If the worker decides not to search for a job, there are 2 possible scenarios for the second period: 1) with probability $(1-p)$ he does not lose his job and continuous receiving earnings equal to w_1; and with probability p he loses his job and takes unemployment benefits equal to $r * w_1$. If the worker decides to search for a job, four different scenarios can occur in the second period: 1) with probability equal to $(1-\lambda)(1-p)$, the worker does not lose his job and does not receive an offer and, thus, continues receiving earnings equal to w_1; 2) with probability equal to $(1-\lambda)p$, the worker loses his job and does not receive an offer, and takes unemployment benefits equal to $r * w_1$; 3) with probability $\lambda(1-p)$, the

[9] In a two-period model, there is no entitlement effect by construction since only the immediate future (period 2) matters for the period 1 decision.

[10] The effective replacement rate takes into account the cap on UB set by the state. Thus, it is calculated using the following formula: $r = \frac{Min(\bar{r} * w_1, \text{max benefits})}{w_1}$, where $\bar{r}$ is the nominal replacement rate.

worker does not lose his job and receives an offer; in this scenario the worker takes the new job as long as the offered wage is greater than w_1; and 4) with probability λp, the worker loses his job and receives an offer; he takes the offer as long as the offered wage is greater than $r * w_1$. Thus, the maximization problem for the worker in period 1 consists of deciding whether he should search on the job or not. More formally, the maximization problem of the worker can be formulated as described below (where δ is the worker's discount factor):

$$\begin{aligned} Max\{\text{No Search, Search}\} =& Max\Big\{ log(w_1) + \delta\left[(1-p)log(w_1) + p \times log(r * w_1)\right], \\ & log(w_1) - k + \delta\Big[(1-\lambda)(1-p)log(w_1) + (1-\lambda)p \times log(r * w_1) \\ & + \lambda(1-p)\int Max\{log(w_1), log(z)\} f(z)dz \\ & + \lambda p \int Max\{log(r * w_1), log(z)\} f(z)dz\Big]\Big\} \end{aligned} \tag{1.1}$$

The optimal decision for the worker is to search in period 1 if his cost of search is below the reservation level k^R, which is a function of the worker's job loss expectations, his current wage, the replacement rate, the probability of receiving an offer if he searches, the distribution of offers, and of the discount factor. An expression for k^R, is given by equation (1.2) below, where $\Theta = (p, w_1, r, \lambda, F(\cdot), \delta)$:

$$\begin{aligned} k^R(\Theta) =& \delta\Big[p\lambda \int_{r*w_1}^{\infty} \left[log(z) - log(r * w_1)\right] f(z)dz \\ & + (1-p)\lambda \int_{w_1}^{\infty} \left[log(z) - log(w_1)\right] f(z)dz\Big] \end{aligned} \tag{1.2}$$

Hence, the probability that a worker engages in OTJS is given by $G\left(k^R(\Theta)\right)$. The following propositions can be shown: [11]

[11] Note that in this setting it is assumed that all workers who fall into unemployment are eligible to receive UB. Moreover, I also assume a take-up rate of 100%. In reality, although most (not self-employed) workers would be eligible for UB, the take-up rate is considerably less than 100%. However, as long as the probability of take-up of UB also increases with the replacement rate, as documented by McCall (1995), all the propositions still hold.

Proposition I.1. *Other things equal, workers with greater probability of job loss are more likely to perform OTJS.*

Proof. See section 1.8.2 □

Proposition I.2. *An increase in the (effective) replacement rate leads to a decrease in the probability of OTJS, but only for workers whose probability of layoff is non-zero (i.e.* $p > 0$*). The replacement rate plays no role in affecting the decision of engaging in OTJS for workers who feel safe (*$p = 0$*) at their jobs.*

Proof. See section 1.8.2 □

Proposition I.3. *Under plausible conditions on the probability function* $G()$*, an increase in the replacement rate will have a larger negative effect on the probability of OTJS the more the worker is at risk of job loss.*

Proof. See section 1.8.2 □

The explanation for the first result is that a worker who does not lose his job will only take another job if the wage offered is greater than the wage at his current job, or w_1, whereas a worker who loses his job will take a job offer as long as the wage offered is larger than the amount of unemployment benefits he can collect , or $r * w_1$. Thus, the expected value of a job offer is larger if the worker losses his job. As a consequence, workers with a higher expectation of layoff are willing to accept a higher job search cost k^R to engage in OTJS (i.e. $\frac{\partial k^R}{\partial p} > 0$) and, therefore, they are more likely to search on the job.

The explanation behind the second result is that UB reduce the expected value of an offer because they increase the reservation wage in case of unemployment, or $r * w_1$. However, this effect would not matter if there is no probability that a worker loses his job. For workers with a positive probability of job loss, higher UB reduce the maximum job search cost that workers are willing to accept to engage in OTJS (i.e. $\frac{\partial k^R}{\partial r} < 0$), and therefore they are less likely to search on the job.

The reason behind the third result follows naturally. An increase in UB generosity will have a higher impact on search decisions the more likely it is that job offers will be measured against the potential UB that a worker can collect (in the second period). See section 1.8.2 for more details on the conditions on $G(\cdot)$ for Proposition I.3 to be always true.

The model also provides interesting comparative statics results regarding the effect of UB on transitions, either from the current job to another job or from the current job into unemployment:

Proposition I.4. *More generous UB do not affect transition probabilities for workers who are not at risk of displacement (i.e. $p = 0$). For workers who perceive a positive risk of job loss, more generous UB increases the likelihood of falling into unemployment and decreases the likelihood of a JTJ transition.*

Proof. See section 1.8.2 □

Proposition I.5. *Given that the worker has a positive probability of job loss ($p > 0$), the effect of an increase in UB generosity is larger (in absolute value) on JTJ transitions than on job-to-unemployment transitions.*

Proof. See section 1.8.2 □

Proposition I.4 indicates that UB can decrease the JTJ transitions and increase the inflow into unemployment. Similar to OTJS, these effects are at work only if the worker is at risk of displacement. Proposition I.5 indicates that the effect of changes in UB should be larger in magnitude on JTJ transitions than on job-to-unemployment transitions. The underlying intuition is that changes in UB directly affects job mobility through their effects on OTJS and on reservation wages, but they only affect entering unemployment if the worker is actually laid off. Therefore, the effect on job transitions should be easier to detect in statistical analyses like the one done in this chapter.

1.4 Data description

The main data source is the HRS, which consists of a nationally representative sample of adults over the age of 50.[12] More than 22,000 Americans have been interviewed every two years since the study was launched in 1992. The study collects information about work status, earnings, assets, and several job characteristics, among other variables. Information from both the raw HRS files and from the RAND HRS Data file was combined.[13]

This chapter focuses on the analysis of male workers. There are a total of 11,298 respondent-year observations corresponding to not self-employed male workers, ages 50 years or older, in the waves between 1996 and 2006, which are the ones that were used for OTJS analysis.[14] After dropping observations with missing information on OTJS activity, firms' downsizing status, weekly wages, and other covariates used in estimations, the sample size was reduced to 8,796. In order to select workers who would potentially qualify for UB, workers whose earnings would be below the eligibility threshold were dropped. Finally, to further minimize the number of ineligibles in the sample, those cases whose estimated weekly benefits would be below the state weekly minimum UB amount were also dropped. These restrictions resulted in dropping 49 additional observations.

As suggested by the theoretical model in section 1.3, UB should affect only the OTJS behavior of workers who feel at risk of displacement. The HRS elicits the subjective probability of job loss through the following question: *"Sometimes people are permanently laid off from jobs that they want to keep. On the (same) scale from 0 to 100 where 0 equals absolutely no chance and 100 equals absolutely certain, what are the chances that you will lose your job during the next year?"* The median of the responses is zero, which indicates that

[12] The HRS is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and the Social Security Administration. It is conducted by the University of Michigan, Ann Arbor, Michigan. Further information is available at http://hrsonline.isr.umich.edu/.

[13] The RAND HRS Data file is an easy to use longitudinal data set based on the HRS data. It was developed at RAND with funding from the National Institute on Aging and the Social Security Administration. Further information is available at http://www.rand.org/labor/aging/dataprod/hrs-data.html.

[14] Firm downsizing status was not available before 1996 and workers' subjective expectations of job loss and job finding are not available after 2006.

this group of workers feels relatively safe in their jobs, and the mean value is 15%. Besides zero, there are also important bunching of responses at 10% and 50%, and to a lesser extent around 90%, which is indicative that responses may be rounded around some focal points (see figures 1.1 and 1.2). In fact, Manski and Molinari (2010) and Kleinjans and van Soest (2010) found strong evidence of rounding responses for subjective probability questions in the HRS, with the extent of rounding differing across respondents. It is not clear what type of bias this non-classical measurement error can introduce. Thus, one has to be cautious when working with these questions. Moreover, there is also evidence that respondents do not necessarily know how to work with probability questions. Since 2004, workers in the HRS have been asked many job satisfaction questions, including questions about job security. Respondents can choose answers from *"strongly disagree"*, *"disagree"*, *"agree"*, or *"strongly agree"* to the statement, *"My job security is poor"*. About 51% of the respondents who said that their expected probability of job loss was 0% did not "strongly disagree" with this statement and 11% of the respondents either agree or strongly agree with it. This inconsistency between responses in both scales suggests that the measurement error in the expected probability of job loss is due not only to rounding but also to inability to work with a probability scale. This measurement error may explain why the estimation results using the subjective probability of job loss have the expected signs but are imprecisely estimated and thus not statistically significant.

Instead of working directly with the subjective probability of job loss, this study uses information on whether the firm where the respondent works has experienced a permanent reduction (downsizing) in employment in the last two years. Table 1.1 shows that labor force downsizing is an important shifter of the subjective probability of job loss. The average subjective job loss probability for workers in downsizing firms is 20%, whereas among workers in non-downsizing firms it is 14%. Labor force downsizing is also a strong predictor of actual displacement even after controlling for observed characteristics. As will be discussed in section 1.6, the monthly relative probability of displacement is 56% larger for workers in downsizing firms than for workers not in downsizing firms. Therefore, the econometric analysis estimates the effects of UB on OTJS and on job transitions separately for workers in downsizing firms (workers at high risk of displacement) and for workers in non-downsizing firms (workers at low risk of displacement).

In total, about 25% of the observations in the sample correspond to workers whose employers have experienced a downsizing. Table 1.1 shows that workers in downsizing firms not only have stronger beliefs that they will lose their jobs in the near future but are also more likely to engage in OTJS than workers in non-downsizing firms (12% versus 9%). Thus the empirical evidence conforms to Proposition I.1 presented in section 1.8.2, i.e., that workers with a greater expected probability of job loss will perform more OTJS .

Table 1.1 also shows that workers in downsizing firms are (on average) more educated, have longer tenure at their current employers, earn higher wages, are more likely to have employer-provided health insurance and pension benefits, work for larger employers, are more likely to be unionized, are more stressed, are more likely to be in the manufacturing sector, have lower expectations of being able to find an equally good job, and have higher average unemployment rates than their counterparts in non-downsizing firms.

The last two rows in Table 1.1 show the variation in two variables related to UB generosity.[15] The first variable is the potential replacement rate for HRS respondents. The value of this variable depends on the worker's weekly wage, the nominal replacement fraction, and the maximum level of weekly benefits set by each state.[16] This method has been

[15] I thank Professor Brian McCall (University of Michigan) for providing a database with the unemployment insurance rules by year and state for the years 1996-2004. This data set was corroborated and updated with the "Comparison of State Unemployment Insurance Laws" reports, which are published annually by the United States Department of Labor (see for example http://www.ows.doleta.gov/unemploy/uilawcompar/2006/comparison2006.asp for a description of 2006 rules).

[16] Given that this study excludes workers whose calculated benefits are below the state minimum weekly benefits, the potential replacement rate was calculated as given below (where r is the nominal replacement fraction):

$$RR = \frac{Min\,(r * \text{weekly earnings, maximum weekly benefits})}{\text{weekly earnings}}$$

used before by Dominitz and Manski (2000) and McCall (1995).[17] The calculation does not include adjustments for dependents.

The second variable is the average of the potential replacement rate for currently employed male workers (over the age of 50) in each state for each year. This variable was constructed from the CPS March Supplement, following the same procedure as before for HRS respondents.[18] The average replacement rate is a measure of the generosity of the UB for the average older worker in that state and year. It depends not only on the nominal replacement fraction, but most importantly on how binding the weekly maximum benefits level is in comparison to the average weekly earnings of an older worker.

1.5 Econometric approach

1.5.1 Unemployment insurance and OTJS

Following the theoretical framework, the probability of OTJS is modeled as a function of the potential replacement rate that the worker would be entitled to if he is displaced and of other covariates. The regressions were estimated using ordinary least squares (OLS) for two separate subsamples: 1) workers from downsizing firms (i.e., workers at high risk of job loss), and 2) workers from non-downsizing firms (i.e., workers at low risk of job loss). The reduced-form models are given by equation (1.1), where i indexes individuals, t indexes year, and d indexes downsizing status ($d = 0$ if a firm is not downsizing and $d = 1$ if a firm is downsizing).

$$y_{it} = \alpha^d + \beta^d_{OLS} RR_{it} + f(w_{it}) + X_{it}\delta^d + \varepsilon_{it} \tag{1.1}$$

[17] As pointed out in McCall (1995), in many states UB depends on the highest quarterly earnings in the base period (the year prior to unemployment). Therefore, measurement error will occur if the usual weekly earnings differ from the weekly equivalent of the high-quarter earnings.

[18] The CPS March Supplement, also known as the March Annual Demographic File and Income Supplement, provides detailed information on respondent's earning over the past year. Further information is available at http://cps.ipums.org/cps/intro.shtml.

The dependent variable y_{it} takes the value of one if the worker is looking for another job while employed and zero otherwise. The variable RR_{it} measures the worker's potential replacement rate if he loses his job; $f(w_{it})$ is a cubic function of the worker's weekly wages.[19] The vector X contains the additional covariates described in Table 1.1, such as age (and a dummy for being older than 62 years), educational attainment (dummies), and several characteristics of the worker's current job and labor market conditions.[20]

According to the predictions from the theoretical model in section (1.8.2), the working hypothesis is that a higher replacement rate decreases the probability of OTJS. In other words, $\beta^d \leq 0$ for $d = \{0, 1\}$. Moreover, the model also predicted that the effects of the replacement rate should be greater for workers who are at higher risk of job loss, i.e. for workers in downsizing firms. In other word, it should be expected that $\beta^1 < \beta^0$.

The replacement rate is a non-linear function of the worker's wage. The objective of this chapter is to disentangle the effect of earnings from the effect of the replacement rate itself on the probability of OTJS. A way to do this is by controlling for a sufficiently flexible function of wages in the regression specification so that the estimated coefficient for the replacement rate relies on cross-state and cross-year variation in the UB rules and not on respondents' wages. The drawback of this approach is that wages are endogenously determined and may be correlated with workers' and jobs' unobserved characteristics that also affect the probability of OTJS. Since the replacement rate is correlated with wages, this unobserved correlation can contaminate both the coefficients on wages and on the replacement rate.

An alternative to disentangle the effect of earnings from the effect of the replacement rate on the probability of OTJS and also control for the endogeneity of earnings (and thus of the replacement rate) is to use an instrument that is not a function of each worker's earnings. The instrument used for the worker's replacement rate is the average replacement

[19] I found that introducing higher order polynomials did not affected the estimation results and also were not statistically significant.

[20] Workers who are 62 years or older are eligible to start collecting social security pensions; this institutional factor may affect their OTJS behavior.

rate (for workers above the age of 50) in the same state and year, calculated from the CPS March Supplement as described previously in section 1.4. To facilitate the interpretation of the instrumental variables (IV) estimation results, this chapter follows Angrist and Pischke (2009) and recodes the average replacement rate (continuous variable) into four dummy categories defined by the quartiles of the distribution of the CPS average replacement rate in the data (all years). In other words, the IV estimation is described by equations (1.2) and (1.3) below. The instruments are given by the variables z_{it}^{j}, which equal 1 if the worker's state's average replacement rate in year t falls in the jth quartile of the distribution of states' average replacement rates over the whole sample.

$$RR_{it} = \gamma^{d} + \phi_2^{d} z_{it}^{2} + \phi_3^{d} z_{it}^{3} + \phi_4^{d} z_{it}^{4} + X_{it}\theta^{d} + u_{it} \tag{1.2}$$

$$y_{it} = \alpha^{d} + \beta_{IV}^{d}\widehat{RR_{it}} + f(w_{it}) + X_{it}\delta^{d} + \varepsilon_{it} \tag{1.3}$$

The values of the quartile boundaries are 0.3463, 0.375, and 0.401, respectively. Despite these values being strikingly close, the instruments are very strong, as shown by the first stage F-statistics described later in the findings section.

For the IV estimation results to be valid, the instruments should be uncorrelated with the error term ε_{it} (the exclusion restriction). An important threat to this condition is that UB rules (and thus the average replacement rate) may be endogenous to labor market characteristics that also affect the probability of the worker of engaging in OTJS . The importance of this threat is assessed by analyzing the sensitivity of the results to controlling for the average annual unemployment rate for the worker's state of residence and for the worker's subjective probability of finding an equally good job if the worker loses his current job. Regarding this second variable, currently employed workers in the HRS are asked the following question, *"Suppose you were to lose your job this month. What do you think are the chances that you could find an equally good job in the same line of work within the next few months?"* The response to this question and the state average unemployment rate act as proxies for labor market characteristics that may encourage or discourage OTJS and that may be correlated with UB policies.

1.5.2 Unemployment insurance and employment transitions

The theoretical framework not only gives predictions about the effect of the replacement rate on the probability of OTJS, but also on how it affects employment transitions. In order to test these predictions, a discrete-time competing risk hazard model is estimated to analyze the effect of UB on monthly employment transitions.

The competing hazards are defined as the reason for separating from the employer, as reported by the respondent. The reported reasons for separation include voluntary resignations or quits, quits for better jobs, business closure, layoffs, poor health, family care, retirement, and other (where the respondent provides his own response). A code for separation due to a JTJ transition is assigned if the worker said he left his previous employer because of a better job or if he said he left for another reason and started a new job without an intervening jobless spell. The employment calendar data from HRS is used to distinguish between separations that were followed by a new job or by a jobless spell.[21] A code for a JTN transition is assigned if the worker separated due to business closures, layoffs, poor health conditions, family care or other reasons and experienced a jobless spell immediately after separation. Finally, a code for separation due to retirement (RET) is assigned if a worker reported that he separated from his previous employer due to retirement (and did not start a new job after separation).

For every employed male worker at a given wave it is determined whether that employment survived until the next wave or whether it ended in between.[22] It is also determined

[21] Jobless spells of one month or less were not considered. In other words, if a worker moved to another job with an intervening jobless spell of one month or less, it was recorded as a JTJ transition without an intervening jobless spell.

[22] In administering the HRS, respondents are asked about whether they are at the same employer as in the previous wave. If they are not with the same employer (of if they are not working), it asks about the month and year of ending previous employment, the month and year of starting current employment, and any months worked in between.

when it ended and the (reported) reason why it ended. Thus, using the panel structure of HRS, the monthly employment history is reconstructed up to the job separation date or up to two years following each interview if the worker continued with his employer (HRS interviews respondents every two years). These monthly observations were used for estimation of the model, with covariates fixed at the value observed at each wave. Only workers whose information was used for the OTJS analysis were included in the transition analysis. Using information from waves 1996 through 2008, there are a total of 164,368 (person-month) observations for all workers, divided into 121,839 observations for workers in non-downsizing firms and 42,529 observations for workers in downsizing firms.

The discrete-time competing risk model was estimated using a multinomial logit following the approach suggested initially by Allison (1982). The monthly probability for individual i of experiencing event j in a given month is given by equation (1.4). There are a total of four different events $\{SAME, JTJ, JTN, RET\}$. Event $k = SAME$ refers to staying at the same employer; event $k = JTJ$ refers to transitions to a new job (without an intervening non-employment spell); event $k = JTN$ refers to transitions to non-employment spells; and event $k = RET$ refers to separation due to retirement.[23] For identification of the parameters of the model, staying at the same employer ($SAME$) is defined as the baseline event.[24]

$$P_{ij} = \frac{e^{\left\{\alpha_j+\lambda_{1j}D_i+f_j(w)+\lambda_{2j}RR_i+\lambda_{3j}t+\beta_j X_i\right\}}}{\sum_K e^{\{\alpha_k+\lambda_{1k}D_i+f_k(w)+\lambda_{2k}RR_k+\lambda_{3k}t+\beta_k X_i\}}} \tag{1.4}$$

As before, the variable RR_i is the individual's potential replacement rate at the time of

[23]An assumption of using multinomial logit for modeling competing hazards is that the risk of event m should not be related to the risk of event n after conditioning on the covariates. This is equivalent to the *Independence of Irrelevant Alternatives* (IIA) assumption that underlies any multinomial logit model. This assumption is plausible given the rich set of covariates in the model, including worker, job and labor market characteristics. Moreover, it is reassuring that after controlling for the cubic function in earnings, the estimated coefficient for the replacement rate is very robust to the selection of observed characteristics included in the vector X. This fact reduces the concerns that the estimated effects could be biased because of failing to control for factors which would result in a violation of the IIA assumption.

[24]For identification of the parameters of the model, all the coefficients for the baseline are set equal to zero.

the survey and $f(w)$ is a cubic function of his weekly wages. Variable D_i takes the value of one if the worker is at a downsizing firm and zero otherwise. Variable t measures the time (number of months) elapsed since the wave interview date, and X_i are other covariates measured at the time of the wave interview, similar to those in equation (1.1). Estimations are done clustering standard errors at the individual level.

In order to interpret the coefficients in equation (1.4), the relative probability of separation must be defined. For any individual i, his relative probability of separation due to event j (or $\tilde{P}_{ij}$), for $j = \{JTJ, JTN, RET\}$, is the ratio of his probability of separation due to event j to the probability of continuing at the same employer, as shown by equation (1.5):

$$\tilde{P}_{ij} = \frac{P_{ij}}{P_{i,SAME}} = e^{\left\{\alpha_j + \lambda_{1j} D_i + f_j(w) + \lambda_{2j} RR_i + \lambda_{3j} t + \beta_j X_i\right\}} \tag{1.5}$$

Hence, $\left[\left(e^{\lambda_{1j}} - 1\right) * 100\right]$ is the percentage change in the relative probability of separation due to reason j for workers in downsizing firms in comparison to workers in non-downsizing firms.[25] Similarly, $\left[\left(e^{\lambda_{21j}} - 1\right) * 100\right]$ is the percentage change in the relative probability of separation because of reason j when the replacement rate changes by one percentage point.

Also computed are the marginal effects of downsizing status and the replacement rate on the average monthly probabilities of employment transitions, which are measured in percentage points rather than in percentage changes. Equation (1.6) provides the expression of the marginal effect of a change in downsizing status on the monthly transition probability due to event j for individual i, while equation (1.7) provides the marginal effect of a change in the replacement rate.

[25] The relative probability of separation due to event j when $D_i = 1$ is equal to $e^{\left\{\alpha_j + \lambda_{1j} + f_j(w) + \lambda_{2j} RR_i + \lambda_{3j} t + \beta_j X_i\right\}}$, and when $D_i = 0$ it is equal to $e^{\left\{\alpha_j + f_j(w) + \lambda_{2j} RR_i + \lambda_{3j} t + \beta_j X_i\right\}}$. Therefore, the relative probability when $D_i = 1$ is equal to $e^{\lambda_{1j}}$ times the relative probability when $D_i = 0$, or a percentage change of $\left[\left(e^{\lambda_{1j}} - 1\right) * 100\right]$.

$$P_{ij|D=1} - P_{ij|D=0} = P_{ij|D=0}\left[\frac{e^{\lambda_{1j}}}{\sum_K\left(P_{ij|D=0} * e^{\lambda_{1k}}\right)} - 1\right] \tag{1.6}$$

$$\frac{\partial P_{ij}}{\partial RR_i} = P_{ij}\left[\lambda_{2j} - \sum_K \left(P_{ik} * \lambda_{2k}\right)\right] \tag{1.7}$$

It is worth noticing that the sign of the response in equation (1.6) is not necessarily given by the sign of λ_{1j}. Similarly, the sign of the response in equation (1.7) is not necessarily given by the sign of λ_{2j}.[26] This is so because λ_{1j} and λ_{2j} can be used to measure the percentage change in the probability of separation due to event j relative to the probability of continuing at the same employer when ignoring the impact that changes in downsizing status and in the replacement rate can have on the probability of other events (different from j) occurring. In contrast, the marginal effects calculations take into account the effect of an increase in the replacement rate on the probability of all events occurring, and makes sure that the sum of those effects equals zero.

Finally, I also investigated the causal effects of the replacement rates on the (monthly) transition probabilities by using the instrumental variables discussed above. Given that IV analysis is not commonly done with multinomial outcomes, I alternatively defined the first-stage and second-stage regressions given by equations (1.2) and (1.3) below. The dependent variable in the first stage is the worker's potential replacement rate (RR_i). As before, it is instrumented by the dummies z_{it}^q indicating if the worker's state's average replacement rate falls in the qth quartile of the distribution of states' average replacement rates. The dependent variable in the second stage is the probability of separation due to event j, for $j = \{JTJ, JTN, RET\}$, relative to continuing at the same employer, and it is denoted by $\tilde{P}_{ij}$. The regressions were also estimated using OLS. In all cases, separations due to an alternative reason k (i.e. $k \neq j$) are treated as censored events. Notice that τ_{1j} and τ_{2j} measure the effects of downsizing status and of the replacement rate in percentage

[26]See Cameron and Trivedi (2005, page 502) for further details on computing marginal effects in a multinomial logit model.

points, so cannot be directly compared to the coefficients in (1.4).

$$RR_i = \phi_j + \psi_2 z_{it}^2 + \psi_3 z_{it}^3 + \psi_4 z_{it}^4 + \zeta_{3j} t + \Psi_j X_i + v_i \tag{1.8}$$

$$\tilde{P}_{ij} = \varphi_j + \tau_{1j} D_i + \tau_{2j} \widehat{RR_i} + \tau_{3j} t + \Gamma_j X_i + \varepsilon_i \tag{1.9}$$

All of the employment transition analyses were done for all workers and also separately for workers at downsizing firms and for workers at non-downsizing firms.

1.6 Findings

1.6.1 Unemployment insurance and OTJS

Column 1 in Table 1.2 shows the estimation results of β_{OLS}^d from equation (1.1) when controlling linearly for weekly wages (and no other covariates). Column 2 presents the ordinary least squares (OLS) results when controlling for a cubic function of weekly wages (and no other covariates). Moving from column 1 to column 2, the estimated coefficient changes substantially and becomes more negative, especially in the case of workers in downsizing firms. This was expected since, all else equal, workers at lower paying jobs are more likely to be engaged in OTJS and have higher potential replacement rates. So, if weekly earnings are not controlled for in a flexible way, their correlation with the potential replacement rate will cause the estimated coefficient of β_{OLS}^d to be biased upwards. The results are very robust when controls for other individual and job characteristics are included in the regression (column 3).

After controlling for a cubic function in earnings, the variation in the replacement rate comes mostly from differences in UB rules across states. As mentioned in section 1.5, one threat to the validity of the results is that UB state rules may be endogenous to labor market characteristics. To test the importance of this threat, in column (4) controls are added for the average annual unemployment rate for the worker's state of residence and for the worker's subjective probability of finding an equally good job if he loses his current job. The coefficients for β^d_{OLS} are robust to the inclusion of these variables which suggests that the potential correlation of UB policies and labor market conditions is not introducing a substantial bias in the estimates. Finally, the estimates of β^d_{OLS} are also robust to the introduction of year dummies (column 5 in Table 1.2). Note that state dummies are not included in the model because almost all of the variation in the states' rules is across states rather than within states over time.[27] Moreover, the introduction of state dummies implies that the source of identification comes from changes in rules within state, which are more likely to be correlated with unobserved changes in state-level economic conditions.

The estimate of the effect of the replacement rate for workers in non-downsizing firms (β^0_{OLS}) is positive across all specifications in Table 1.2, against the prediction of the theoretical model (second comparative statics result). One explanation is that weekly earnings may be correlated with unobserved characteristics that can affect search behavior, making the replacement rate endogenous. For example, workers with high earnings may have a better job match quality and/or work at a firm that also offers unobserved desirable amenities. Standard search theory indicates that these factors should correlate negatively with the probability of OTJS. Thus, workers with low replacement rates (high earnings) would appear to be searching less frequently rather than more, as predicted by the theoretical model of section 1.3. Also, this type of bias is more likely to exist for workers in non-downsizing firms than workers in downsizing firms. Evidence (see Lengermann and Vilhuber 2002) indicates that both individuals with low and high earnings (after controlling for observed characteristics) are at a relatively high risk of separation from downsizing firms (the first group due to layoffs and the second one due to quits or resignations).

The IV estimation described in equations (1.2) and (1.3) should control for the endogeneity bias of earnings (and thus, of the replacement rate). The results using IV estimation

[27]For example, the between-states variance in the nominal replacement rate is 65 times the within-state variance; in the case of the maximum weekly benefits, the between-state variance is 40 times greater than the within-state variance.

are shown in Table 1.3. The estimations were done separately for workers in downsizing firms and workers in non-downsizing firms. Column 1 shows the IV estimation results of the effects of the replacement rate on the probability of OTJS (β^d_{IV}) with no covariates. Now the estimated coefficients are negative for both types of workers with $\beta^1_{IV} < \beta^0_{IV}$, as predicted by the theory, although the coefficient is not statistically significant for workers at non-downsizing firms. Also, the IV estimation results for workers in downsizing firms (β^1_{IV}) are similar in magnitude to the OLS estimates from the preferred specification (column 5 of Table 1.2).

The IV estimation results are robust to the inclusion of workers and job covariates (column 2 of Table 1.3). Also, similar to the OLS case, a threat to the validity of the results is that the average replacement rate in a state (and year) would respond to market conditions that also affect OTJS behavior. That would violate the exclusion restriction needed for the IV estimation results to be valid. However, the estimated coefficients are again robust to controlling for the average unemployment rate and for the worker's expectation of job finding possibilities (column 3). Thus, there is no strong evidence of violations of the exclusion restriction. Furthermore, the results are also robust to controlling for year dummies (column 4).

The IV estimation results can be interpreted as the weighted average of the causal response along the potentially nonlinear relationship between the replacement rate and OTJS (see Angrist (1990) and Angrist and Pischke (2009)). The weighting functions are in proportion to the size of the complier population at each point, which can be calculated by the instrument-induced change in the cumulative density function (CDF) of the replacement rate at that point. These changes are depicted in Figure 1.3 for non-downsizing firms and in Figure 1.4 downsizing firms. The figures plot the differences in the probability that the worker‘s replacement rate is at or exceeds the replacement rate on the x-axis (e.g. one minus the CDF). The differences are between observations for which the state's average replacement rate is either in the second, third or fourth quartile of the sample and observations for which the state's average replacement rate is in the first quartile.

From figures 1.3 and 1.4, it can be observed that the CDF differences drop dramatically at replacement rates above 0.5. This is because in most states, UB around half of the lost earnings but only up to a maximum level of weekly benefits. Thus, a state's UB generosity can be thought of as the level of the maximum weekly unemployment benefits it allows, in comparison to the average earnings of an older (50+) male worker. Moving from a "low UB generosity" state to a "high UB generosity" state does not affect the replacement rate of workers with low earnings whose UB are not capped by their state's maximum benefit level. In other words, the IV estimation results should be interpreted as the average response of relatively high earning workers, for whom their potential UB are affected (capped) by their states' maximum weekly UB.

Regarding the size of the estimated effects, column 4 from Table 1.3 shows that, in the case of workers from downsizing firms, a one percentage point increase in the replacement rate leads to a decrease of 0.47 percentage points in the probability of OTJS, or a reduction of 3.9% when evaluated at the sample mean of 0.12 for the probability of OTJS. This is a substantively large effect, with an estimated elasticity (also at sample means) of -1.4.

For workers in non-downsizing firms, a one percentage point increase in the replacement rate leads to a smaller decrease of 0.13 percentage points in the probability of OTJS, or a decrease of just 1.5%. Moreover, this effect is not statistically significant. The estimated elasticity evaluated at the sample means is also smaller, about -0.57.

The effects of other covariates (not shown here) conform to standard predictions from search theory; specifically, higher wages decrease the probability of OTJS; workers whose employers provide health insurance and pension benefits are less likely to engage in OTJS; workers who are unionized are also less likely to be looking for another job (potentially because they have less job insecurity); and finally, the probability of OTJS decreases with age and with tenure at current employment.

1.6.2 Unemployment insurance and employment transitions

Table 1.4 presents the estimation results for the multinomial logit model in equation (1.4) using all workers and also separate estimations for workers in downsizing firms and in non-downsizing firms. The estimations control for a cubic specification in weekly wages and for worker, job and labor market characteristics as in column 5 of Table 1.2. Only the estimation results for the effects of downsizing status (λ_{1j}) and the replacement rate (λ_{2j}) are presented.

Table 1.1 showed that workers in downsizing firms have, on average, six percentage points (or 20%) higher expectations of job loss than workers in non-downsizing firms. By estimating equation (1.5) on the full sample of workers, it can be tested whether workers in downsizing firms are more likely to separate from their employers, after controlling for other observed characteristics. The first row in Table 1.4 shows that the relative probability of separation due to a JTN transition is indeed 56% higher for workers in downsizing firms than for workers in non-downsizing firms. In contrast, there are not statistically significant associations between downsizing and the relative probability of separation for other reasons.

In Table 1.5, the marginal effects are calculated using the formula from equation (1.6), and averaged across individuals. The results indicate that being in a downsizing firm is associated with an increase of 0.2 percentage points in the monthly probability of transitioning into non-employment. This implies a 50% increase in comparison to the average predicted monthly transition probability into non-employment (see Table 1.6). Moreover, being in a downsizing firm is associated with a decrease of 0.28 percentage points in the monthly probability of staying at the same employer. These effects are statistically significant, meaning that workers in downsizing firms are indeed more likely to separate from their employers and to enter into a jobless spell, even after controlling for all observed characteristics.

However, the estimated effect of downsizing status on job separations are smaller than the average difference in the subjective probability of job loss between workers in downsizing firms and workers in non-downsizing firms, suggesting that workers in the first group may be overly pessimistic about their job security. Figure 1.5 shows the effects when the impacts of downsizing status on the monthly transition probabilities are compounded over a period of six months, 12 months, 18 months and 24 months. For example, over a 12-month period, being in a downsizing firm is associated with a decrease of 2.9 percentage points (from 0.86 to 0.83) in the probability of staying at the same employer and an increase of 2.1 percentage points (from 0.04 to 0.06) in the probability of transitioning into a non-employment spell.

Regarding the effects of the replacement rate on the monthly transitions probabilities, Table 1.4 shows that a one percentage point increase in the replacement rate decreases the relative probability of separating because of a JTJ transition by 1.1% (significant at the 10% confidence level) when using the sample of all workers. The effect gets larger, a decrease of 2.5% (also significant at the 10% level), when using only a sample of workers at downsizing firms. Similarly, a one percentage point increase in the replacement rate increases the relative probability of transitioning into a jobless spell by 0.4% when using all the workers, and by 2.0% when using only workers at downsizing firms. However, in this case, the effects are more imprecisely estimated and not significant even at the 10% level.

Table 1.5 confirms using the marginal effects estimation that an increase of one percentage point in the replacement rate is associated with a decrease of 0.005 percentage points (or 1.1%) in the monthly probability of observing a JTJ transition when using all workers (significant at the 10% level), and with a decrease of 0.009 percentage points (or 2.5%) when using only workers in downsizing firms (significant also at the 10% level). The corresponding estimated elasticities (at the mean values) are -0.42 and -0.88. Two conclusions can summarize the results in tables 1.4 and 1.5. First, being in a downsizing firm is associated with an increased likelihood of separation, even after controlling for all observed characteristics. Second, changes in the replacement rate have a larger effect on monthly transitions for workers in downsizing firms, especially in JTJ transitions. This was expected given the previous findings that changes in the replacement rate have a stronger effect on OTJS behavior for workers in downsizing firms.

Table 1.7 shows the estimation results of the IV estimation for the monthly relative transition probabilities, described in equations (1.8) and (1.9). The advantage of the IV estimation is that the estimates would not be contaminated by potentially unobserved worker and job characteristics that are correlated with worker's earnings. However, the disadvantage is that this analysis can only be done for the probability of experiencing any transition relative to staying at the same employer, which ignores the probability of experiencing other types of transitions. In other words, joint marginal effects for each type of transition (which should sum to zero) cannot be computed, as was the case with the multinomial logit. The estimation results from Table 1.7 confirm that an increase in the replacement rate leads to a decrease in the monthly relative probability of experiencing a JTJ transition, although in this case the IV estimates are more similar between workers in downsizing firms and workers in non-downsizing firms.

Alternative specifications of the competing hazards were tried and, although the results were somewhat sensitive to the model specification, the effect of the replacement rate on the relative probability of JTJ transitions was consistently larger and more statistically significant than on the relative probability of entering a non-employment-spell. This finding is supported by the Proposition I.5 presented in section 1.8.2, i.e. that changes in the generosity of the replacement rate should have a greater effect on JTJ transitions than on JTN transitions and thus should be easier to detect statistically in the data.

1.6.3 Limitations

Some limitations of the validity and generalizability of the empirical results are worth discussing. First, as mentioned in section1.6, the identification strategy relies on comparing

UB rules and average replacement rate across states rather than using changes within states over time. This is because UB rules and, therefore, average replacement rates have been relatively stable within states over the sample period (1996-2006). In other words, there is not sufficient within-state variation that can be used for identification; hence, the identification of effects is done by cross-states comparisons. The threat to this identification strategy is that UB rules (and average replacement rates) may be correlated with other states characteristics that can also affect OTJS. If this is the case, then the estimated effects would be biased. In this chapter, additional controls for labor market conditions are introduced in an attempt to minimize that risk. It is reassuring that the estimated effects are robust to the introduction of those controls.

An improvement of this chapter with respect with previous work is that the statistical analysis is done on a sample of workers at high risk of job loss (workers from downsizing firms) and on a sample of workers at low risk of job loss (workers from non-downsizing firms). The evidence in this chapter suggests that the effects of UB are stronger when workers have higher levels of job insecurity. However, although the results are robust to the introduction of different controls, the estimated effects of UB on OTJS and on employment transitions may not apply to other situations of high job insecurity that are not directly linked to employer downsizing. For example, workers who have high expectations of being laid off because of their low productivity, unpleasant relationships with their supervisors or health problems may react differently to higher replacement rates.

Finally, as discussed in section 1.6, low-wage workers, whose potential UB are not affected by their states' caps, usually are entitled to a replacement rate of roughly half of their working earnings. Thus, the actual variation in the replacement rate for the empirical analysis comes from relatively high earners and the estimated effects should be interpreted as an average effect for this subpopulation. In other words, the findings in this chapter may not be extrapolated to low-wage workers for whom UB provisions, specifically maximum UB levels, are not binding.

1.7 Conclusions

This chapter presents the results of a study of the effect that UB (measured as the fraction of lost earnings that they replace) have on OTJS and on employment transitions. The theoretical model discussed here predicts that more generous UB will reduce the benefits of OTJS because generous UB reduce the costs or financial burden of unemployment. The model also predicts that the effects of UB on OTJS are only present for workers who believe they are at risk of job loss and are larger (under plausible conditions) the higher is the risk of job loss. To test these hypotheses, the sample was divided into two groups: workers from downsizing firms (i.e. firms that have experienced a permanent reduction in their labor force) and workers from non-downsizing firms. Workers from downsizing firms have both higher expectations of job loss and higher actual rates of job displacement.

Results from this study indicate that an increase in the replacement rate discourages OTJS for workers in downsizing firms, which in turn reduces their probability of leaving for another job. A one percentage point increase in the replacement rate leads to a 0.47 percentage point decrease in the probability of OTJS. Evaluated at the sample means, this effect corresponds to a change of -3.9% and an elasticity of -1.4. The same increase in the replacement rate would lead to a decrease of 2.5% in the monthly probability that the worker experiences a JTJ transition (relative to staying at the same employer) and to an increase of 2.0% in the relative probability of separation into a jobless spell. For workers in non-downsizing firms, the estimated effects of an increase in the replacement rate on the probability of OTJS and on transition probabilities were smaller and not statistically significant.

In order to compare the results with those found in previous work, the implied elasticities are used. Burgess and Low (1992, 1998) found that UB discouraged OTJS for workers who received advanced notification of layoff and did not expect a recall from their employers. In particular, they found that a $10 increase in weekly benefits above their mean reduced the likelihood of OTJS by eight percentage points from 59.7% to 51.7%, or an elasticity close to -1. This effect is slightly smaller than the elasticity of -1.4 that the current study found for the effect of the replacement rate on OTJS probability for workers at downsizing firms. There are a few potential explanations for this difference. First, Burgess and Low used the level of UB as a measure of UB generosity whereas this current study used the replacement rate. In fact, redoing the estimation using the level of UB provides much closer elasticity estimate (-1.1). Second, another potential explanation is that the sample used by Burgess and Low was comprised of ultimately displaced workers, who probably were less likely to engage in OTJS, other things equal. For example, workers with high search costs would be less responsive to incentives from UB. Third, the identification strategy in Burgess and Low (1998) relies in the nonlinear interaction of UB rules and lost wages, after controlling for a linear term in wages. In this chapter, the identification relies on cross-state variation in UB generosity alone, and not in the worker's wage. Therefore, the results are not directly comparable. Finally, it may also be the case that the effect of UB on OTJS may be larger for older workers. For example, Haan and Prowse (2010) argue that UB are a stepping stone into retirement in Germany. However, this is less likely to be the case in the US since the duration of UB for older workers in the US is not as long as in Germany.

Light and Omori (2004) found that job resignations or quits decline as UB increase, although they found a very small effect. They estimated that one standard deviation increase above the mean in weekly UB reduces the job quit probability (or a JTJ transition) in the next 15 weeks from 0.047 to 0.045, which implies an elasticity of -0.09.[28] In contrast, the present study found an elasticity of -0.88 for workers in downsizing firms, and no evidence that UB generosity (measured by the replacement rate) affects JTJ transitions for workers in non-downsizing firms. The fact that Light and Omori (2004) lacked a good measure of workers who could be at high risk of displacement may explain why their estimates are much lower than the estimates in this study and those of Burgess and Low (1992, 1998).[29]

[28]This estimate is for a worker with 30 weeks of tenure.

[29]Light and Omori (2004) use, as proxy for risk of displacement, the evolution in employment (at the

As mentioned in section 1.3, the OTJS behavior of workers would respond to UB only if they feel at risk of job loss. Moreover, the average worker feels very secure at his job and, thus, estimated effects that do not take into account workers' job insecurity should be on average very small.

However, even for the sample of workers in downsizing firms, the estimated effects of changes in the replacement rate on employment transition probabilities are small when measured in percentage points (rather than in percentage changes or in elasticities). This is so because older workers tend to have high attachment to their employers, as evidenced by their low (monthly) probability of separation.

To summarize, the results of this chapter indicate that UB reduce the probability OTJS for workers at high risk of job loss. Future work should evaluate the potential costs and benefits of such effects. At least for older male workers, the findings in this chapter suggest that UB does not change substantially the entry rate into unemployment, which may be one of the main costs associated with a reduction in OTJS, especially for worker at high risk of job loss. On the benefits side, higher UB may reduce the stress level and poor health outcomes associated with job insecurity. Higher unemployment benefits can also prevent workers who feel at risk of lay off from rushing into poor jobs. In conclusion, future research should expand the analysis of potential costs and benefits of UB beyond the unemployed to include employed workers who are (or perceive themselves to be) at risk of layoff.

county level) in the employer's industry with respect to overall employment. This measure is a very crude measure of risk of displacement which also confounds with the worker's prospects of job finding. Since the probability of job loss and of job finding have opposite effects on OTJS, and given the low precision of the approximation for the risk of job loss, it is not surprising that Light and Omori did not find a statistically significant interaction between UB and this proxy of layoff probabilities.

Table 1.1: Estimation sample means

	All workers	Workers not in downsizing firms	Workers in downsizing firms
Sample size	8,747	6,533	2,214
Downsizing firm	0.25	—	—
On-the-job Search	0.09	0.09	0.12
Subjective probability of job loss	0.15	0.14	0.20
Weekly wage in 2006 dollars (hundreds)	10.94	10.64	11.84
Full-time worker	0.94	0.94	0.97
Part-time worker	0.06	0.06	0.03
Age	58.69	58.95	57.92
Tenure on current job	14.21	12.86	18.19
Education			
Less than High School	0.15	0.16	0.11
GED or HS diploma	0.33	0.33	0.33
Some college	0.22	0.22	0.23
College and above	0.30	0.28	0.33
Fringe Benefits			
Employer-provided HI	0.80	0.77	0.87
Worker has a defined-benefit pension plan	0.41	0.35	0.57
Worker has a defined-contribution pension plan	0.45	0.43	0.50
Physical Effort			
Worker has to exert physical effort all/most of the time	0.20	0.20	0.18
Worker has to exert physical effort most of the time	0.15	0.15	0.15
Worker has to exert physical effort some of the time	0.31	0.32	0.28
Worker has to exert physical none of the time	0.34	0.33	0.38
Stress levels			
Worker strongly agrees that job is stressful	0.17	0.15	0.21
Worker agrees that job is stressful	0.41	0.40	0.45
Worker disagrees that job is stressful	0.38	0.40	0.32
Worker strongly disagrees that job is stressful	0.04	0.04	0.03
Unionized	0.12	0.11	0.16
Employer's size			
Less than 5 workers in location	0.04	0.05	0.02
5-14 workers in location	0.07	0.08	0.04
15-24 workers in location	0.04	0.04	0.02
25-99 workers in location	0.10	0.11	0.07
100-499 workers in location	0.10	0.11	0.10
500+ workers in location	0.08	0.07	0.12

Employer's Industry			
Agriculture/Forest/Fishing	0.02	0.02	0.02
Mining and Construction	0.07	0.08	0.06
Non-durable goods manufacturing	0.09	0.08	0.11
Durable goods Manufacturing	0.15	0.12	0.23
Transportation	0.10	0.10	0.11
Wholesale	0.06	0.07	0.04
Retail	0.08	0.09	0.06
Finance/Insurance/Real State	0.05	0.05	0.04
Business/Repair Services	0.06	0.06	0.05
Personal Services	0.01	0.01	0.01
Entertaining and Recreation	0.02	0.02	0.01
Professional and related services	0.18	0.19	0.15
Public Administration	0.07	0.07	0.08
Other/missing industry	0.04	0.05	0.02
Urban/Rural area of residency			
Metro areas of 1 million population or more	0.49	0.48	0.51
Metro area of 250k-1 million population	0.23	0.23	0.23
Metro area fewer of 250K populations	0.07	0.06	0.08
Urban population of 20k or more	0.09	0.10	0.07
Urban population of 2.5k to 19k	0.11	0.11	0.09
Completely rural less than 2.5k	0.02	0.02	0.02
Local labor market conditions			
State average unemployment rate	4.86	4.81	5.02
Expected probability of finding an equally good job	0.48	0.51	0.39
Unemployment benefits			
Potential replacement rate	37.87	38.73	35.33
State's average replacement rate	37.21	37.15	37.36

Source: Health and Retirement Study (1996-2006) and Current Population Survey March Supplement (1997-2007).

Table 1.2: Effect of the replacement rate on the probability of OTJS (OLS estimation)

	Model Specification (Outcome =1 if looking for another job; =0 if not)				
	(1)	(2)	(3)	(4)	(5)
A. Workers in non-downsizing firms					
Replacement rate 1/	0.00153***	0.00111***	0.00086**	0.00094**	0.00093**
	[0.00315]	[0.00039]	[0.00042]	[0.00042]	[0.00042]
B. Workers in downsizing firms					
Replacement rate 1/	-0.00116	-0.00424***	-0.00394***	-0.00406***	-0.00429***
	[0.00111]	[0.00138]	[0.00138]	[0.00132]	[0.00141]
Controls:					
Weekly wages (linear)	X				
Weekly wages (cubic)		X	X	X	X
Worker and job characteristics			X	X	X
Labor market conditions				X	X
Year dummies					X
State dummies					

Notes:

*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance at 10% level.

1/ In percentage points (i.e. from 0-100).

Standard errors clustered at the state level. Sample sizes were 6,533 and 2,214 for workers in non-downsizing firms and workers in downsizing firms, respectively.

Table 1.3: Effect of the replacement rate on the probability of OTJS (IV estimation)

	Model Specification			
	(1)	(2)	(3)	(4)
A. Workers in non-downsizing firms				
First stage coefficients				
2nd quartile	5.91313***	4.80251***	4.81722***	4.53901***
	[0.85950]	[0.70460]	[0.73368]	[0.62867]
3rd quartile	8.02886***	7.03880***	7.02309***	6.69200***
	[0.90534]	[0.99494]	1.05732]	[0.84644]
4th quartile	11.26093***	10.87604***	10.85603***	10.77817***
	[1.18813]	[1.14033]	[1.15168]	[1.01395]
F-statistic	32.8502	29.9991	29.1413	38.0299
Second stage coefficients				
Replacement rate	-0.00123	-0.00124	-0.00122	-0.00132
1/	[0.00118]	[0.00125]	[0.00130]	[0.00118]
B. Workers in downsizing firms				
First stage coefficients				
2nd quartile	4.54574***	3.75264***	3.88207***	3.61159***
	[1.00700]	[0.70432]	[0.67942]	[0.69286]
3rd quartile	7.53548***	6.69115***	6.83804***	6.61887***
	[1.11062]	[0.88085]	[0.86208]	[0.75466]
4th quartile	11.06116***	11.03958***	11.17638***	11.14061***
	[1.24798]	[1.21998]	[1.18079]	[1.14735]
F-statistic	26.8756	28.1674	31.0105	35.539
Second stage coefficients				
Replacement rate	-0.00429*	-0.00455**	-0.00468**	-0.00469**
1/	[0.00229]	[0.00197]	[0.001892]	[0.00193]
Controls (included in both stages):				
Workers and job characteristics		X	X	X
Labor market conditions			X	X
Year dummies				X
State dummies				

Notes:
*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level;
* denotes statistically significance at 10% level.
1/ In percentage points (i.e. from 0-100).
Standard errors clustered at the state level. Sample sizes were 6,533 and 2,214 for workers in non-downsizing firms and workers in downsizing firms, respectively.

Table 1.4: Multinomial Logit (MNL) estimation results

Variable	Estimated coefficient in the MNL linear index for:		
	Job-to-Job transition (JTJ)	Job-to-Non-Employment transition (JTN)	Retirement (RET)
A. ALL WORKERS			
Being in a downsizing firm	0.13288	0.44767***	0.04695
	[0.10074]	[0.09479]	[0.08024]
Replacement rate 1/	-0.01068*	0.00421	-0.011882**
	[0.00555]	[0.00698]	[0.00474]
B. WORKERS IN NON-DOWNSIZING FIRMS			
Replacement rate 1/	-0.00794	0.00107	-0.01141**
	[0.00615]	[0.00823]	[0.00561]
C. WORKERS IN DOWNSIZING FIRMS			
Replacement rate 1/	-0.02572*	0.02003	-0.00846
	[0.01488]	[0.01498]	[0.01277]

Notes:
*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance at 10% level.
1/ In percentage points (i.e. from 0-100).
Standard errors clustered at the individual level. Sample sizes were 164,368 (person-month) observations for all workers, 121,839 observations for workers from non-downsizing firms and 42,529 observations for workers from downsizing firms. Estimation includes controls for a cubic in weekly wages, other worker characteristics, job characteristics, local labor market characteristics, and year dummies.

Table 1.5: Average marginal effects on monthly transition probabilities

	Type of Transition			
	Same employer (SAME)	Job-to-Job transition (JTJ)	Job-to-Non-Employment transition (JTN)	Retirement (RET)
A. ALL WORKERS				
Being in a downsizing firm	-0.00280***	0.00060	0.00198***	0.00022
	[0.00077]	[0.00048]	[0.00048]	[0.00040]
Replacement rate 1/	0.00009**	-0.00005*	0.00002	-0.00006*
	[0.00004]	[0.00002]	[0.00003]	[0.00002]
B. WORKERS IN NON-DOWNSIZING FIRMS				
Replacement rate 1/	0.00009*	-0.00004	0.00000	-0.00005**
	[0.00005]	[0.00003]	[0.000032]	[0.000026]
C. WORKERS IN DOWNSIZING FIRMS				
Replacement rate 1/	0.00006	-0.00009*	0.00008	-0.00005
	[0.000107]	[0.000054]	[0.000063]	[0.000074]

Notes:
*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance at 10% level.
1/ In percentage points (i.e. from 0-100).
Marginal effects calculated using results from Table 1.4.

Table 1.6: Predicted average monthly transition probabilities

	Same employer (SAME)	Job-to-Job transition (JTJ)	Job-to-Non-Employment transition (JTN)	Retirement (RET)	Total
All workers	0.98662	0.00446	0.00395	0.00496	1.00000
Workers in non-downsizing firms	0.98673	0.00476	0.00387	0.00465	1.00000
Workers in downsizing firms	0.98632	0.00360	0.00421	0.00588	1.00000

Note: Average monthly transition probabilities calculated using results from Table 1.4.

Table 1.7: OLS models for monthly relative transitions

	OLS estimation		IV estimation	
	Being in a downsizing firm	Replacement rate 1/	Being in a downsizing firm	Replacement rate 1/
A. All workers				
JTJ transitions	0.00038	-0.00003	0.00040	-0.00009*
	[0.00039]	[0.00002]	[0.00040]	[0.00005]
JTN transitions	0.00164***	0.00003	0.00165***	0.00000
	[0.00039]	[0.00002]	[0.00039]	[0.00004]
RET	0.00008	-0.00005*	0.00008	-0.00004
	[0.00043]	[0.00002]	[0.00043]	[0.00004]
B. Workers in non-downsizing firms				
JTJ transitions		-0.00002		-0.00010*
		[0.00003]		[0.00006]
JTN transitions		0.00003		-0.00002
		[0.22100]		[0.00005]
RET		-0.00004*		-0.00008
		[0.00002]		[0.00005]
C. Workers in downsizing firms				
JTJ transitions		-0.00010*		-0.00008
		[0.00005]		[0.00008]
JTN transitions		0.00006		0.00007
		[0.00006]		[0.00008]
RET		-0.00007		0.00010
		[0.00006]		[0.00009]

Notes:
*** denotes statistically significance at 1% level. ** denotes statistically significance at 5% level. * denotes statistically significance ant 10% level.
1/ In percentage points (i.e. from 0-100).
Standard errors clustered at the individual level. Estimation includes controls for worker's characteristics, job's characteristics, local labor market characteristics, and year dummies.

Table 1.8: Predicted average monthly relative transition probabilities

	Job-to-Job transition (JTJ)	Job-to-Non-Employment transition (JTN)	Retirement (RET)
All workers	0.00450	0.00399	0.00501
Workers in non-downsizing firms	0.00480	0.00390	0.00469
Workers in downsizing firms	0.00363	0.00425	0.00592

Notes: Average monthly transition probabilities calculated using results from Table 1.7

Figure 1.1: Probability of job loss: Non-downsizing firms

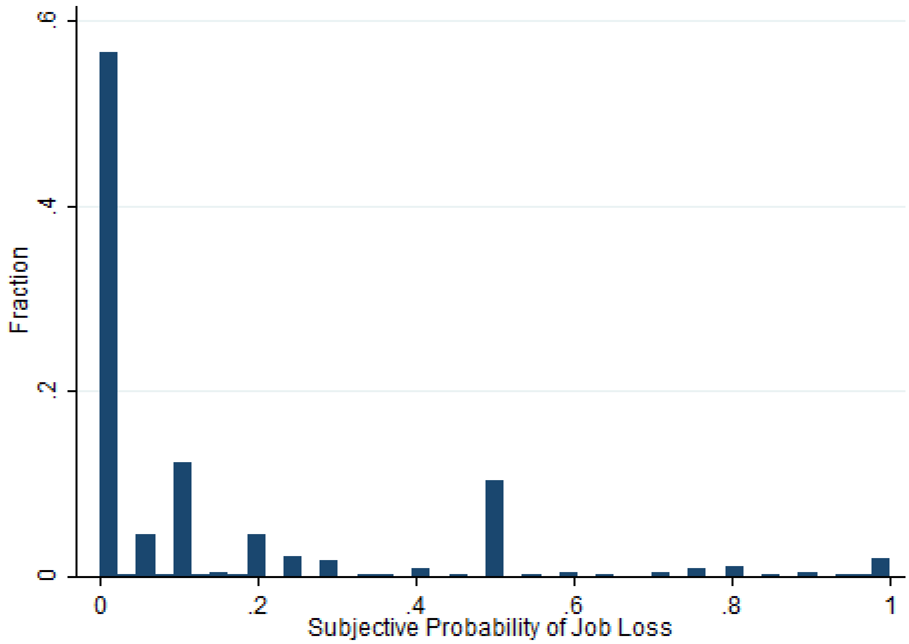


Figure 1.2: Probability of job loss: Downsizing firms

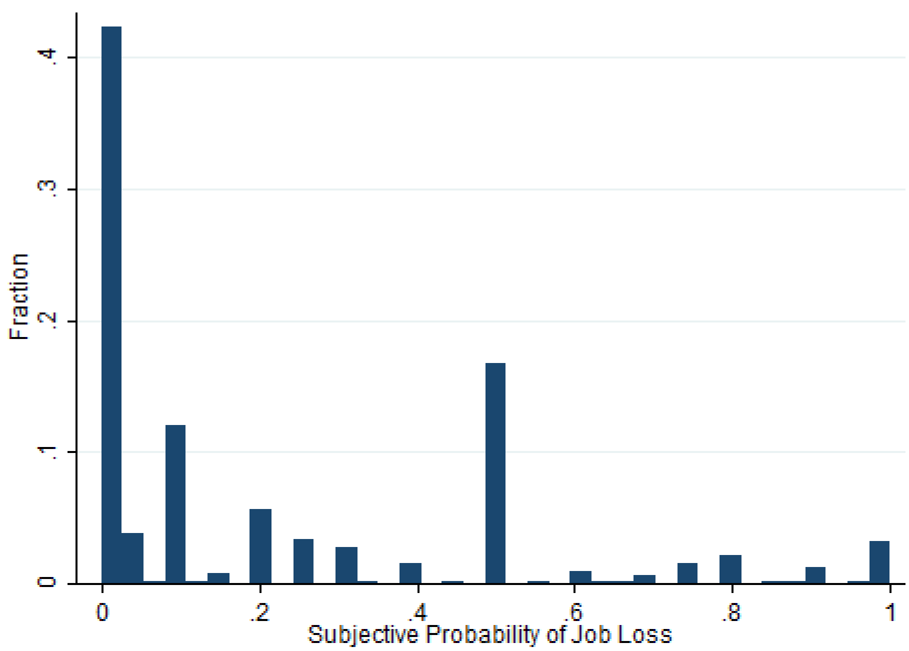
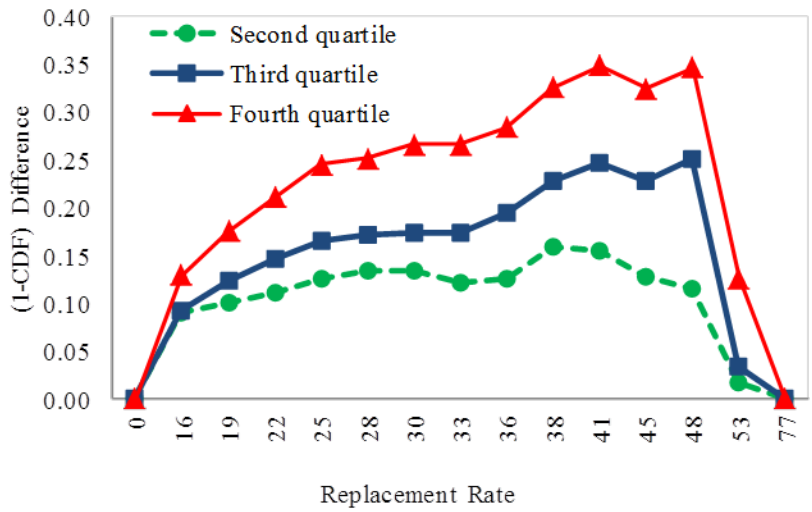
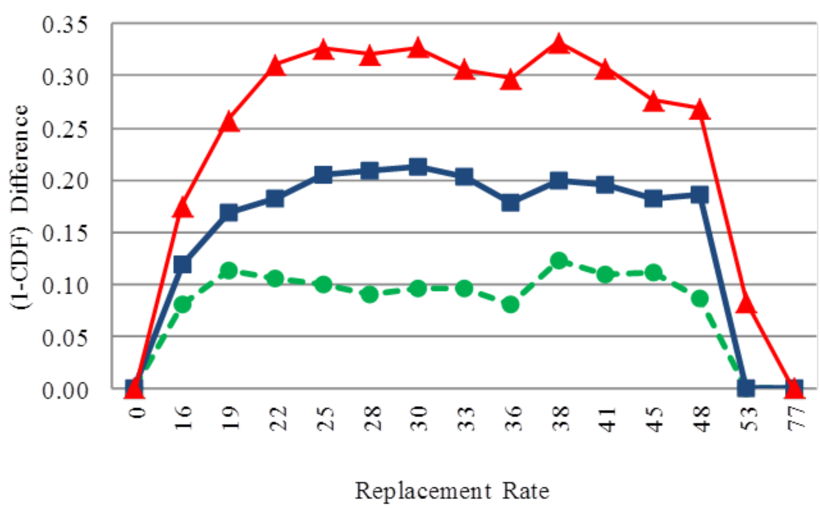


Figure 1.3: Effects of instruments on the replacement rates' CDF: Non-downsizing firms



Notes: Each point measures the effect of each quartile dummy on the probability that the replacement rate is above the level on the x-axis. They were estimated by running OLS regressions with the dependent variable being an indicator equal to one if the replacement rate is above the level on the x-axis. Dummies for the second, third and fourth quartile and controls for workers, job and labor market observable characteristics were on the right-hand side. This graphs reports the coefficients for the quartile dummies.

Figure 1.4: Effects of instruments on the replacement rates' CDF: Downsizing firms



Notes: Each point measures the effect of each quartile dummy on the probability that the replacement rate is above the level on the x-axis. They were estimated by running OLS regressions with the dependent variable being an indicator equal to one if the replacement rate is above the level on the x-axis. Dummies for the second, third and fourth quartile and controls for workers, job and labor market observable characteristics were on the right-hand side. This graphs reports the coefficients for the quartile dummies.

Figure 1.5: Effect of downsizing on transition probabilities (in percentage points)

1.8 Appendix: Theoretical model in detail

1.8.1 Model set up

The following model is an adaptation of the model initially proposed by Light and Omori (2004). The model consists of two periods. In the first period, the worker is employed and earns w_1. The maximization problem of the worker is to decide if he wants to search on-the-job (OTJS) or not. If the worker decides to search, then he has to pay a cost of k, which is distributed among workers with probability function $G()$. The flow utility is given by logarithm of the available earnings and the cost of job search (if he searches on the job).

If a worker searches, the probability of receiving an offer in the second period equals λ. Offers come from a known distribution $F(w)$ and at most one offer can be received. The worker faces a probability of layoff in period two equal to p. If he gets laid off, he can collect UB equal to $r*w_1$, where w_1 is the current wage of the worker in period 1 and r is the effective replacement rate.

If the worker decides not to search for a job, there are two possible scenarios for period 2: 1) with probability $(1-p)$ he does not lose his job and continuous receiving earnings equal to w_1; and with probability p he loses his job and takes unemployment benefits equal to $r*w_1$. If the worker decides to search for a job, four different scenarios can occur in period two: 1) with probability equal to $(1-\lambda)(1-p)$, the worker does not lose his job and does not receive an offer and, thus, continues receiving earnings equal to w_1; 2) with probability equal to$(1-\lambda)p$, the worker loses his job and does not receive an offer, and takes unemployment benefits equal to $r*w_1$; 3) with probability $\lambda(1-p)$, the worker does not lose his job and receives an offer; in this scenario the worker takes the new job as long as the offered wage is greater than w_1 ; and 4) with probability λp, the worker loses his job and receives an offer; he takes the offer as long as the offered wage is greater than $r*w_1$. Thus, the maximization problem for the worker in period 1 consists of deciding whether he should search on the job or not. More formally, the maximization problem of the worker can be formulated as described below (where δ is the worker's discount factor) :

$$\begin{aligned} Max\,\{\text{No Search, Search}\} =& Max\Big\{ log(w_1)+\delta\left[(1-p)log(w_1)+p\times log(r*w_1)\right], \\ & log(w_1)-k+\delta\Big[(1-\lambda)(1-p)log(w_1)+(1-\lambda)p\times log(r*w_1) \\ & +\lambda(1-p)\int Max\,\{log(w_1),log(z)\}\,f(z)dz \\ & +\lambda p\int Max\,\{log(r*w_1),log(z)\}\,f(z)dz\Big]\Big\} \end{aligned} \tag{1.1}$$

The optimal decision for the worker is to search in period 1 if his cost of search is below the level a maximum level k^R, which is a function of the worker's wage, job loss expectations and the replacement rate. An expression for k^R, is given by equation (1.2) below, where $\Theta = (p, w_1, r, \lambda, F(\cdot), \delta)$:

$$\begin{aligned} k^R(\Theta) =& \delta\Big\{p\lambda\int_{r*w_1}^{\infty}\left[log(z)-log(r*w_1)\right]f(z)dz \\ & +(1-p)\lambda\int_{w_1}^{\infty}\left[log(z)-log(w_1)f(z)dz\right]\Big\} \end{aligned} \tag{1.2}$$

Hence, the probability that a worker engages in OTJS is given by $G(k^R(\Theta))$, or the probability that his idiosyncratic cost of search is below the maximum cost he is willing to accept to search on the job.

1.8.2 Comparative statics results

The following propositions hold in the model.

Proposition I.1. *Other things equal, workers with greater probability of job loss are more likely to perform OTJS.*

Proof. Equation (1.3) below shows the derivative of k^R with respect to the expected probability of job loss (p), which is unambiguously positive.

$$\frac{\partial k^R(\Theta)}{\partial p} = \delta\lambda\left[\int_{r*w_1}^{w_1}[log(z) - log(r*w_1)f(z)dz] - \int_{w_1}^{\infty}[log(z) - log(w_1)]f(z)dz\right] > 0 \qquad (1.3)$$

Given that the probability of performing OTJS is given by $G(k^R(\Theta))$, and that the probability density function $g(k^R(\Theta))$ is always positive, then equation (1.4) below completes the proof of Proposition I.1:

$$\frac{\partial G\left(k^R(\Theta)\right)}{\partial p} = g\left(k^R(\Theta)\right) * \frac{\partial k^R(\Theta)}{\partial p} > 0 \qquad (1.4)$$

Proposition I.2. *An increase in the (effective) replacement rate leads to a decrease in the probability of OTJS , but only for workers whose probability of layoff is non-zero (i.e.* $p > 0$*). The replacement rate plays no role in the decision to engage in OTJS for workers who feel safe (*$p = 0$*) at their jobs.*

Proof. Equations (1.5) and (1.6) below show the derivative of k^R and of the probability of OTJS with respect to r:

$$\frac{\partial k^R(\Theta)}{\partial r} = -\delta\lambda p\left[\frac{w_1}{log(r*w_1)}\right][1 - F(r*w_1)] \leq 0 \qquad (1.5)$$

$$\frac{\partial G\left(k^R(\Theta)\right)}{\partial r} = g\left(k^R(\Theta)\right) * \frac{\partial k^R(\Theta)}{\partial r} \leq 0 \qquad (1.6)$$

The derivatives are zero if $p = 0$. Thus, changes in the replacement rate have no effect on the probability of OTJS if the worker is not at risk of job. If p is positive, then an increase in the replacement rate will decrease the maximum search cost an employed worker is willing to pay, everything else equal, and thus will reduce the probability that he engages in OTJS.

Proposition I.3. *Under plausible conditions on the probability function* $G()$*, an in-*

crease in the replacement rate will have a larger negative effect on the probability of OTJS the more the worker is at risk of job loss.

Proof. Equation (1.7) below shows the cross-derivative of k^R with respect of p and r, whereas equation (1.8) shows the cross-derivative of the probability of OTJS with respect to p and r:

$$\frac{\partial k^R(\Theta)}{\partial p \partial r} = -\delta\lambda \left[\frac{w_1}{log(r*w_1)}\right][1-F(r*w_1)] < 0 \tag{1.7}$$

$$\frac{\partial G\left(k^R(\Theta)\right)}{\partial p \partial r} = \left[g\left(k^R(\Theta)\right)\frac{\partial k^R(\Theta)}{\partial p \partial r}\right] + \left[\frac{\partial g\left(k^R(\Theta)\right)}{\partial k^R(\Theta)}\frac{\partial k^R(\Theta)}{\partial p}\frac{\partial k^R(\Theta)}{\partial r}\right] \tag{1.8}$$

Equation (1.7) shows that the effect of an increase in the replacement rate (r) on reducing the maximum search cost an individual is willing to accept gets larger (more negative) at higher levels of job loss expectations (p). However, a priori, the effect of r on the probability of OTJS can be larger or smaller with higher levels of p. In other words, the sign of equation (1.8) is a priori ambiguous. However, under condition (1.9) below, the sign of $\frac{\partial G\left(k^R(\Theta)\right)}{\partial p \partial r}$ would be unambiguously negative.

$$\frac{\frac{\partial g\left(k^R(\Theta)\right)}{\partial k^R(\Theta)}}{g\left(k^R(\Theta)\right)} > \frac{-\frac{\partial k^R(\Theta)}{\partial p \partial r}}{\frac{\partial k^R(\Theta)}{\partial p}\frac{\partial k^R(\Theta)}{\partial r}} \tag{1.9}$$

Condition (1.9) is a smoothness condition requiring that if the rate of change in the probability density function $g()$ is negative, then it should be bounded by $\frac{-\frac{\partial k^R(\Theta)}{\partial p \partial r}}{\frac{\partial k^R(\Theta)}{\partial p}\frac{\partial k^R(\Theta)}{\partial r}}$. In other words, condition (1.9) requires that $g()$ varies smoothly around $k^R(\Theta)$.

Equations (1.10) and (1.11) state the probability that workers experience a quit (Q) or fall into unemployment (U) in period two, respectively.

$$P(Q|\Theta) = G\left(k^R(\Theta)\right)\lambda\left\{p\left[1-F(r*w_1)\right]+(1-p)\left[1-F(w_1)\right]\right\} \tag{1.10}$$

$$P(U|\Theta) = p\left\{1-G\left(k^R(\Theta)\right)\lambda\left[1-F(r*w_1)\right]\right\} \tag{1.11}$$

The following propositions can be shown:

Proposition I.4. *More generous UB do not affect transition probabilities for workers who are not at risk of displacement (i.e. $p=0$). For workers who perceive a positive risk of job loss, more generous UB increase the likelihood of falling into unemployment and decrease the likelihood of a JTJ transition.*

Proof. Taking the derivatives of (1.10) and (1.11) with respect to the replacement rate r, the following equations are obtained, where $f()$ is the probability density function of the wage offer distribution $F()$:

$$\begin{aligned}\frac{\partial P(Q|\Theta)}{\partial r} =& \frac{\partial G\left(k^R(\Theta)\right)}{\partial r}\lambda\left\{p\left[1-F(r*w_1)\right]+(1-p)\left[1-F(w_1)\right]\right\}\\ &+G\left(k^R(\Theta)\right)\lambda p\left[-f(r*w_1)w_1\right]\end{aligned} \tag{1.12}$$

$$\begin{aligned}\frac{\partial P(U|\Theta)}{\partial r} =& -\lambda p\left\{\frac{\partial G\left(k^R(\Theta)\right)}{\partial r}\left[1-F(r*w_1)\right]\right.\\ &\left.+G\left(k^R(\Theta)\right)\right\}\left[-f(r*w_1)w_1\right]\end{aligned} \tag{1.13}$$

Thus, $\frac{\partial P(Q|\Theta)}{\partial r}$ is zero if $p=0$, and negative if $p>0$. Similarly, $\frac{\partial (U|\Theta)}{\partial r}$ is zero if $p=0$, and positive if $p>0$.

Proposition I.5. *Given that the worker has a positive probability of job loss ($p>0$), the effect of an increase in UB generosity is larger (in absolute value) on JTJ transitions than on job-to-unemployment transitions.*

Proof. We can re-write the absolute value of (1.12) as

$$
\begin{aligned}
\mid \frac{\partial P(Q|\Theta)}{\partial r} \mid = & -\frac{\partial G\left(k^R(\Theta)\right)}{\partial r}\lambda\left\{p\left[1-F(r*w_1)\right]+(1-p)\left[1-F(w_1)\right]\right\} \\
& -G\left(k^R(\Theta)\right)\lambda p\left[-f(r*w_1)w_1\right]
\end{aligned} \tag{1.14}
$$

Subtracting (1.13) from (1.14) we have:

$$
\mid \frac{\partial P(Q|\Theta)}{\partial r} \mid - \mid \frac{\partial P(U|\Theta)}{\partial r} \mid = -\frac{\partial G\left(k^R(\Theta)\right)}{\partial r}\lambda(1-p)\left[1-F(w_1)\right] \geq 0 \tag{1.15}
$$

Again, expression (1.15) equals zero when p is zero and is positive when p is positive.

CHAPTER II

Do Unemployment Benefits Affect Workers' Job Search? Evidence from Establishment Closures in West Germany[1]

2.1 Introduction

This chapter complements the analyses in Chapter I in four main aspects. First, I focus on establishment closures (in Germany), as a way to study the effect of unemployment benefits (UB) on workers who are at (imminent) risk of layoff, which is arguably known to them and exogenous to their own characteristics.[2] Second, I use a rich and high-quality administrative dataset that allows me to distinguish whether the worker separated from the closing establishment to start a new job without an intervening nonemployment spell, i.e. a job-to-job (JTJ) transition; or whether he separated by entering nonemployment, i.e. a job-to-non-employment (JTN) transition. The empirical analyses in Chapter I relied on self-reports on employment histories and on reasons for employment termination to classify a worker separation from his employer as either a JTJ or a JTN transition. Third, I use a identification strategy based on difference-in-difference methods that exploits changes in the rules for the potential duration of unemployment benefits (PDB) in Germany in the

[1] I am grateful to the Sweetland/Rackham Dissertation Institute and to the Rackham Graduate School for providing funding for this research.

[2] The institutional settings in Germany, regarding mandatory advance dismissal notifications (in written form) and mandatory consultations with work councils and local employment agencies prior to a closure, ensures that establishment closures are not a surprise to workers. Thus, workers at an establishment that is near its demise are likely to be aware of their impending layoff, providing an ideal setting to test the effects of UB on workers' search behavior.

mid-1980s and mid-1990s. Fourth, and related to the previous point, this chapter explores the effects of changes in the duration of UB rather than changes on their levels (or in replacement rates).

The theoretical prediction I test in this chapter, as suggested by the model, is that workers entitled to longer PDB have incentives to exert less effort in searching for a new job and have higher reservation wages, as well. As a result, they have a lower probability of experiencing a JTJ transition before the establishment closes. I find empirical evidence in support of this prediction. In particular, I find that the large expansion in the PDB in the 1980s reduced the probability that workers left their establishments before their closure to take on new jobs. I also find that the reduction in the PDB in the mid-1990s increased the probability that workers moved to a new job before their establishments closed, although the size of the effect per month of change in the PDB is smaller than that found for the 1980s (and non-statistically significant). This can be explained because the moderate reductions in the PDB in the 1990s are likely to have affected the search behavior of a smaller fraction of workers, as opposed to the large increase in the 1980s.

Thus, the findings in this chapter support the theoretical claim that UB affect the job search behavior of employed workers who are at risk of layoff. Furthermore, these findings have two important implications. First, studies regarding the optimal design of UB should include their potential effect on employed workers, particularly those at risk of job loss, in their analysis. Second, studies that use establishment closures to study the effect of job loss should be aware of the potential sample composition bias arising from the fact that workers present at the moment of closure are likely to be entitled to more generous UB.

The rest of the chapter is organized as follows: Section 2.2 describes the related literature; Section 2.3 describes the institutional background in Germany, specifically the unemployment insurance system and the procedures for dismissing workers; Section 2.4 describes the data set; Section 2.5 presents the theoretical framework that guides the empirical analysis; Section 2.6 discusses the econometric strategy; Section 2.7 presents the empirical results; and Section 2.8 concludes by summarizing the findings and suggesting avenues for future research.

2.2 Related literature

As reviewed in Chapter I, only a few papers have directly studied the effect of UB on workers' on-the-job search (OTJS) behavior or JTJ mobility (see Section 1.2). This chapter provides further evidence on the effect that UB have on employed workers' behavior by using establishment closures as a way to focus on workers who are at high risk of job loss. Therefore, this chapter also touches on the literature about the non-random selection of workers who are ultimately displaced by a plant closure.

In an early work, Pfann and Hamermesh (2001) studied the demise of Fokker Aircraft, a large Dutch corporation, and concluded that the firm learned about each worker's probability of quitting and adjusted its layoff policy accordingly (firing those workers more likely to quit). Thus, workers who remained in the firm until its closure were selected non-randomly from the group of workers present at the firm when the negative shocks initially arrived.

For the US, Lengermann and Vilhuber (2002) find evidence that the pattern of workers' separations prior to a plant closure is consistent with both highly qualified workers leaving distressed firms and with management actions to lay off low skilled workers. Similarly, for the case of Austria, Schwerdt (2011) finds evidence that a substantial fraction of all separations happening up to two quarters before a plant closure are directly related to "early leavers", or workers who decided to "abandon the sinking ship". These early leavers are distinct in terms of higher productivity than ultimately displaced workers (higher earnings even after controlling for observed characteristics).

This chapter analyzes whether UB have a role in altering the timing of separation from establishments that are approaching their demise or, put differently, whether they make workers more likely to stay until the establishment closes. Studying these potential effects is important for understanding sample composition bias in previous studies that have used plants closings as a natural experiment to study the effects of job loss on distinct outcomes.

2.3 Institutional background

2.3.1 The German unemployment insurance system

There are two types of benefits for the unemployed in Germany: unemployment benefits (UB) and unemployment assistance (UA). The former is funded by contributions of employers and job holders and is granted for a certain number of months depending of an individual's previous contribution period and age. During the period of my study (1982-2004) eligibility for UB was achieved after 12 months of work in the last three years.[3] If an individual exhausts the maximum number of months of UB, then he is eligible for UA. This benefit is funded from government revenue and is not time-limited. It is granted for a year and re-approved every year if a means test is passed and the claimant is younger than 65 (Schmitz and Steiner, 2007). The amount received under UB depends on prior income. Until 2005, the amount of benefits from UA also depended on prior income.[4] However the benefits from UA could be reduced considerably by spousal earnings and other sources of income.[5] Individuals who were not entitled to UB/UA or whose net income after receiving benefits was sufficiently low, received social assistance. Social assistance payments are non-time-limited transfers which raise the individual's net income up to the social mini-

[3] A person who voluntarily quits his job is subject to a waiting period sanction of 12 weeks before collecting benefits. In case of hardship the sanction could be limited to 6 weeks and if the job would have ended within four weeks anyway, the sanction could be limited to 3 weeks only (Hofmann, 2008). When a person is sanctioned for 12 weeks the duration of his entitlement is also shortened by 25% or at least twelve weeks.

[4] Until December 1983 the replacement rate for UB was 68% and for UA was 58% of the previous *net* wages, irrespective of whether the recipient had children. Since the UB and UA benefits are calculated from net earnings, they are not taxed. However, they can push total income into a higher tax bracket (Schmieder et al., 2012). Starting in 1984, the replacement rate of UB and UA was lowered for workers without dependents to 63% and 53%, respectively. The replacement rates were further lowered slightly in January 1994, to 60% for UB if the worker had no dependents and to 67% in case of dependents; and to 50% for UA if no dependents and to 57% in case of dependents. The replacement rates for UB have remained constant since then and were changed for UA in 2005 with the fourth Hartz Reform.

[5] Although the nominal replacement rate is above 50%, after taking into consideration deductions due to other sources of income, the effective replacement rate for older workers is around 35% for men and 10% for women (Schmieder et al., 2012).

mum income.[6]

As shown in Table 2.1, the formula determining the PDB for each age changed considerably in the 1980s and 1990s, which provides two quasi-natural experiments for the identification of the effects of interest. Before 1985, unemployed workers were only entitled to a maximum duration of 12 months of UB, regardless of age, as long as they were eligible for benefits. Starting in 1985, older workers were entitled to longer potential durations of UB, depending on the number of months they have worked in the last seven years prior to the start of the unemployment spell. Subsequent increases in the PDB were phased in between 1985 and 1987. Since July 1987, the PDB formula included increases in potential duration of benefits for workers age 42 or older, depending on their working history. The longest potential duration was 32 months for workers age 54 or older.[7] The rules determining PDB remained stable in Germany for over a decade. In April 1997, a new reform (the Employment Promotion Act) was introduced to reduce potential disincentive effects of unemployment insurance. The PDB were lowered by increasing the age requirements to qualify for longer UB durations by 3 years. The reform was phased in gradually, so that for most people it only took effect in April 1999 (Schmieder et al., 2012; Schmitz and Steiner, 2007).[8] The fourth Hartz reform, which was introduced in 2005 but became effective in February 2006, further reduced PDB. The PDB was set back to 12 months for all workers younger than 55 years old, and was reduced to 18 months for workers age 55 or older.[9] Furthermore, the reform also tightened the criteria for eligibility for UB. After the reform,

[6]In January 2005, with the introduction of the fourth Hartz reforms, UA was integrated with social assistance to become Unemployment Benefit II (UBII), which is still means-tested and, in principal, granted indefinitely. However, the amount does not depend on the former net income of the unemployed individual anymore, but on the legally defined social minimum of the household which depends on the number and age of the household members and includes costs for renting and heating up to certain amounts.

[7]For unemployed people who already received UB in the last 7 years the period between the last and the new unemployment spell is used to determine the entitlement length. The remaining months of UB from the last unemployment spell are then added. The total duration is still capped by the maximum PDB determined by age.

[8]Those who became unemployed after April 1997 but had worked at least 12 months out of the last three years prior to the spell before April 1997 were entitled to UB according to the old regulation (Schmitz and Steiner, 2007).

[9]Also, after the reform, the PDB is calculated on the number of months worked in the last three years, instead of seven.

a person has to have worked for at least 12 months in the last two years (instead of three) to qualify for UB (Schmitz and Steiner, 2007).

This chapter studies the effects of the first two changes in the determination of the PDB. The last reform is not included in the analysis because the data covers only the period 1982-2004. Future work will expand the analysis to incorporate the Hartz reforms.

2.3.2 Dismissal procedures

The German labor market is highly regulated and employers have to follow many procedures before dismissing their workers. For permanent (or open-ended contracts) dismissal protection sets in after a probationary period of 6 months during which only minimum requirements and a short notice period of 2 weeks apply. Legal dismissal protection currently does not apply to firms with fewer than 10 employees. However, this threshold has changed over time. It was increased from 5 to 10 in 1996, lowered to 5 in 1999, and then increased again to 10 in 2004. In order to avoid complications from these changes in the empirical analysis, I work only with establishments that had more than 10 workers in the year prior to their closure.[10]

The legal minimum notice period is 4 weeks for both the employer (layoffs) and the employee (voluntary quit). Minimum notice periods for employers increase with tenure: 1 month after 1 year of service, 2 months after 5 years of service, 3 months after 8 years of service, 4 months after 10 years of service, 5 months after 12 years of service, 6 months after 15 years of service and 7 months after 20 years of service. Longer notice periods and additional employment protections can be introduced through collective agreements, particularly for older or long-tenured workers, or by individual contracts. Every dismissal needs to be consulted with the works council, which is an organization that represents the workers of a firm or establishment.[11] In collective (mass) dismissals or closures, both the

[10] They account for over 75% of workers in Germany (Schmitz and Steiner, 2007).

[11] Works Councils are authorized, but are not automatic, in all establishments with five or more employees (Addison et al., 2002).

works council and the local employment agency need be informed in advance. Moreover, the employer has the obligation to check all options for continuing employment, e.g. through reorganization or employment at other organizations. Also, for collective dismissals in plants with more than 20 workers, the works council can request a social plan to mitigate the effects of the layoffs. They include agreements on severance payments and other provisions for promoting re-employment. Also, the selection of workers to be dismissed needs to consider some priority rules or social criteria, such as years of service, age, and family obligations, among others.

2.4 Establishment closures data

This chapter uses a matched establishment-worker dataset prepared by the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB).[12] The data set was constructed by sampling establishments that closed in West Germany during the period 1982-2004.[13] Establishment closures are in principle identified by the disappearance of the identification number from the administrative records, which would happen if there are no more workers (liable to social security) at that establishment. However, there are many reasons why an establishment identification number may disappear that are not related to real closures. For example, if a firm is taken over, the establishments belonging to the firm may change identification number, but they clearly continue to operate. In order to identify real closures, the FDZ have classified establishment closures in four categories following the work of Hethey and Schmieder (2010). I focus on the analysis of establishments closures classified as atomized deaths, meaning

[12]More information available online at http://fdz.iab.de/en.aspx

[13]Establishments are defined by their identification numbers, which are allocated to organizational units consisting of at least one worker liable to social security. Thus an establishment can be a plant, a restaurant, a gas station, a bank branch, etc. In other words, this definition of an establishment does not necessarily correspond to that of a firm, which may be comprised of many establishments. Instead, it is more accurate to think of an establishment as a local economic unit consisting of workers and capital, operating under a joint legal framework (such as being part of a firm), and producing some sorts of goods or services (Hethey and Schmieder 2010).

that workers from these establishments do not appear together (at least in a great proportion) at a subsequent establishment.[14] Atomized deaths are more likely to correspond to true closures. Also, working with atomized deaths minimizes the risk that the employers implemented a restructuring of its labor force, for example by relocating workers at other locations.[15] These relocation would complicate the interpretation of JTJ transitions as resulting from workers' behavior.

Among establishments classified as atomized deaths, I further limited the sample to those with at least 10 workers in the year before closure, as mentioned in Section 2.3.2. Overall, my sample represents about 2.6% of the atomized closures (with more than 10 employees) in West Germany during the period 1982-2004. The sampling design was stratified by establishment size in the year prior to the closure, with larger establishments being oversampled.[16]

The dataset also contains the full working biography of all workers who were present at the sampled establishments at any moment within the last five years of their existence. Access to the workers' complete biography allows me to estimate their potential entitlement to UB, based upon their work history. It also allows me to follow the worker after he separates from the establishment. Therefore, I can determine whether he moved to another establishment without any intervening nonemployment spell, i.e. a job-to-job (JTJ) transition, or whether he separated by entering nonemployment, i.e. a job-to-nonemployment transition (JTN).

Some caveats in interpreting and measuring these transitions are worth mentioning.

[14] All closing establishments with fewer than four workers are classified by FDZ as small establishment deaths. The FDZ defines a cluster of workers as a group of workers from the closing establishment that after the closure appear together in a new establishment. Closing establishments with more than four workers and for which the largest cluster of workers represented less than 30% of the employment in the last year before closure (at the exiting establishment) are classified as atomized deaths. Closing establishments where that percentage is between 30%-80% are classified as chunky deaths; and establishments where that ratio is above 80% are classified as not true closures. In this latter case, they are more likely to correspond to changes of identification numbers or to establishment take-overs.

[15] See Section 2.3.2 about the requirements of social plans in cases of collective dismissal of workers.

[16] Establishments with 11-50 workers were sampled with a probability of 0.025; establishments with 51-500 workers were sampled with a probability of 0.25; and establishments with more than 500 workers were sampled with a probability of 1.

First, a JTN transition can be initiated by the employer (layoff) or by the worker (voluntary quit or resignation). It is not possible to distinguish in the data between these two.[17] Second, the data set only includes employment that is within the social security system. This covers about 80% of all jobs (Schmieder et al., 2012). The main categories that are not included are the self-employed and government employees. Thus, all coded JTJ transitions are correct, whereas coded JTN transitions may be partly contaminated.

The main econometric analysis is restricted to workers aged 38 to 56 years who have worked (in a position covered by social security) at least 64 months in the last seven years at the beginning of the last year of existence of an establishment. The latter restriction allows me to include only workers with long labor force attachment who would have been eligible for the maximum changes in the PDB that are studied in this chapter (see Table 2.1). Workers age 38 to 41 are included to act as the control group since their entitlement to UB was not modified.[18] Workers age 57 years or older are excluded to avoid confounding the effects of PDB with incentives for early retirement. Although the legal retirement age in Germany is 65 years old, earlier retirement at age 60 is possible.[19] Thus, workers age 57 or older can potentially use long entitlements of UB as a means to step into early retirement, as suggested by Haan and Prowse (2010). In fact, during the 1980s and 1990s, the government promoted UB as a bridge between employment and early retirement.[20]

[17]Since workers who voluntarily quit their jobs are subject to a waiting period sanction, it should be possible to look at gaps between the last employment and the first benefit receipt spell to identify voluntary quits. However, these gaps can also occur if the worker voluntarily (or involuntarily) delays notifying the local employment agency that he is unemployed, or if the worker experiences a short period of self-employment. Moreover, the waiting period sanction is not clearly defined, since it can range from three weeks to twelve weeks (see footnote 3).

[18]I tried alternative cutoff points at 35 and 40 and the results were qualitatively similar.

[19]The legal retirement age in Germany is 65 years old. Retirement at age 60 was possible after 180 contribution months if unemployed at the commencement of the pension and if unemployed for 52 weeks after completion of the age of 58.5 years. Alternatively, retirement at age 63 was possible after 35 years of insurance (Ebbinghaus and Eichhorst, 2006; Tatsiramos, 2010). Recent changes in early 2000s increased this age limits up to 65, but retirement at ages 60 and 63 are still possible with the acceptance of pension reductions, which amounts to 0.3% of the pension for each month during which the pension is claimed earlier (Tatsiramos, 2010).

[20]Since January 1986 unemployed workers aged 58 or older who formally agreed to retire at the age of 60 years old could receive UB without being registered as searching for work (Fitzenberger and Wilke, 2009; Hunt, 1995; Schmitz and Steiner, 2007). However, the recent Hartz reforms introduced a break in this incentive. Not only the PDB for older workers were reduced (since 2006), but also the exemption from search

Empirical evidence suggests that most of the workers' reaction in OTJS behavior happens when the firm is relatively close to its closure and thus the impending risk of layoff is well known. For example, Schwerdt (2011) finds, for the Austrian labor market, that workers who decide to leave distressed employers (or "abandon the sinking ship") can be traced only up to two quarters before closure. Earlier separations are indistinguishable from normal turnover.[21] Schwerdt (2011) argues that the fact that selective turnover sets in only up to two quarters before closure and not earlier is because the maximum notice period is five months.[22] Thus, information of impending layoffs may not be available for workers earlier. Given that the length of the notice period in Austria is similar to that in Germany, it should be expected that most of the workers who strategically leave the exiting establishment (to another employer) in my analysis sample do so during the last year of its existence. Additional evidence is also provided by Kahn (2012), who studied job search behavior of workers under temporary contracts using the European Community Household Panel (ECHP) data.[23] He finds that workers in temporary jobs search harder than workers on permanent jobs and that the search intensity increases as the remaining duration of the contract falls. However, most of this increase (84%) happens in the last 6 months before the termination of the contract. Thus, the focus of this chapter will be on the analysis of workers' separations during the last year of existence of an establishment.

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requirements for workers aged 58 or older was abolished in December 2007.

[21] Schwerdt (2011) compares employment and earnings outcomes of workers separated from exiting establishments (plants) with those of workers separated from non-exiting establishments (i.e. the control group reflecting normal turnover). He finds that workers at exiting establishments who separated up to two quarters before closure, but not earlier, had on average better employment prospects than workers who separated from non-exiting establishments. Thus, these workers are considered "early leavers" or workers who strategically decided to leave the establishment in distress.

[22] In Austria, the maximum notice period for blue collar workers is two weeks before dismissal. White collar workers have a notice period of 1.5 months that can increase up to five months with tenure Schwerdt (2011).

[23] The data covers the period 1995-2011 and 11 countries: Austria, Belgium, Denmark, Finland, France, Greece, Ireland, Italy, the Netherlands, Portugal and Spain.

2.5 Theoretical framework

There are at least three mechanisms through which UB could affect firms' layoffs decisions. First, imperfectly experience rated UB promote and facilitate the use of temporary layoffs. Second, in highly regulated labor environments such as the German one, firms cannot easily separate employees, especially older ones with long tenure, due to the social criteria that must be considered for separations (see Section 2.3.2). Employers usually have to provide severance payments and other benefits to dismiss workers. In this case, UB can be used as a subsidy to reach a mutual agreement between employers and workers (Dlugosz et al., 2009). Third, an increase in UB decreases the employer-employee match surplus, by increasing the employee's outside option. Thus, more generous UB should result in an increase in the rate of job destruction as was shown in Pissarides (2000). However, the potential effect of UB on firms' layoff decisions would be arguably small or negligible for the case of establishments near their demise, regardless of the mechanism. Thus, establishment closures are not ideal to study these effects.

In contrast, establishment closures provide an interesting framework to study how UB affect the behavior of employed workers who are at risk of layoff. In this section I model the search effort and reservation wages of an employed worker to study how they are affected by changes in the PDB, denoted by T. I also explore how those effects vary depending upon the level of job insecurity. In order to capture the institutional settings in Germany, I assume that if an employer wants to lay off a worker he has to give him a notification N periods in advance. A worker can leave his employer for another job at any time if a suitable offer is available.[24] If a worker is ultimately displaced he can collect UB, denoted by b for a total of T periods. Upon exhaustion of the UB, the worker can collect UA, which is denoted by a (and $b > a$). The worker is entitled to UA indefinitely.

I assume that any new job that the worker takes on lasts forever. This assumption

[24] In the model all quits are related to moving to a new employer. There are no voluntary quits into nonemployment or self-employment. I also abstract from modeling retirement.

rules out that the value of future employment spells can be affected by changes in UB. In a dynamic job search model, changes in UB will affect not only the value of current unemployment spells but also of possible future ones. Thus, more generous UB (either higher levels or longer duration) also increase the value of future jobs by increasing their termination value.[25] This effect creates incentives for workers to increase job search efforts in response to more generous UB. In my model, I let UB affect the value of the current job by affecting the value of the next possible unemployment spell, but UB do not affect the value of any following job because they last forever. This eliminates some analytical indeterminacy in the comparative statics in the model. Nevertheless, as long as these effects are small or dominated by the effect of UB on the value of the next possible unemployment spell (as suggested by evidence from the effects of UB on unemployment duration) the results discussed here remain valid.

The insights of the model are built up by small steps, or propositions. I start by analyzing the effect of changes in PDB on an unemployed worker. Then I study their effects for a worker who is still employed but has already received a layoff notification. Finally, I study the effects of changes in the PDB for a worker who has not yet received a layoff notification. I use these comparative statics results to predict the patterns of JTJ transitions in the data and how it is modified by the changes in the PDB.

2.5.1 Modeling the value of unemployment

I start by assuming that the worker has become unemployed. Let $V(t)$ denote the continuation value of unemployment when there are t remaining periods of entitlement to unemployment benefits b, counting the current period. Workers can choose search effort intensity which is equivalent to choosing the probability of getting an offer s. The cost of search is given by $c(s)$, which for convenience is assumed to be $c(s) = 0.5s^2$. I assume

[25] This is similar to the "entitlement effect" described in Mortensen (1977) by which an increase in UB generosity increases job search efforts by the unemployed who are not eligible to receive them. This is because an employment spell is a port of entry into eligibility for UB (the "entitlement") and thus more generous UB increases the value of finding a job.

that job offers come (if any) at the end of each period and only one offer per period can be received. If the worker has already exhausted this UB entitlement, the continuation value of unemployment is given by $V(0)$, as described by equation (2.1):

$$V(0) = \underset{s}{Max}\left\{z + a + \beta\left\{sE\left[Max\left(W(x), V(0)\right)\right] + (1-s)V(0)\right\} - 0.5s^2\right\} \tag{2.1}$$

The term z denotes the value of leisure, β denotes the time discount factor, and $W(x)$ is the continuation value of a job which pays a wage of x. Since any new job lasts forever, we have $W(x) = \frac{x}{1-\beta}$. Let x_t^U denote the minimum (reservation) wage offer that an unemployed individual with t remaining periods of UB would be willing to accept. Thus, $V(0) = W(x_0^U)$ and the worker will take any offer with $x > x_0^U$. I assume that wage offers follow a distribution function $F(x)$, which is constant over time and equal for employed and unemployed workers. The continuation value of unemployment during the last period of entitlement for UB, denoted by $V(1)$, is described by equation (2.2). Note that $V(1) = V(0) + (b-a) > V(0)$.

$$V(1) = \underset{s}{Max}\left\{z + b + \beta\left\{sE\left[Max(W(x), V(0)\right] + (1-s)V(0)\right\} - 0.5s^2\right\} \tag{2.2}$$

Proposition II.1. *The value of unemployment and the reservation wage increase with longer (remaining) entitlement to UB.*

Proof. See Section 2.9.1.1 in the Appendix □

Proposition II.1 is intuitive. Since $b > a$, i.e. $UB > UA$, an individual who has a longer remaining entitlement to UB will have a higher expected utility from remaining unemployed or, to put it differently, a larger opportunity cost of accepting a job. Proposition II.1 also implies that an increase in the maximum PDB, denoted by T, leads to an increase in the initial value of unemployment $V(T)$.

2.5.2 Modeling the value of employment for a notified worker

Now I model the search decision of an employed worker who has received a layoff notification. Let $B^L(w,n,T)$ denote the worker's continuation value in his current employment given that he has received a layoff notification, has a maximum of n periods remaining (including the current one) before he is separated from his employer, earns a wage of w, and can collect UB benefits for a total of T periods if unemployed. I assume that $w > b+z$, so that the flow utility if employed is larger than that if unemployed. Let $x^L(w,n,T)$ denote the reservation wage for a notified individual, i.e. the minimum outside wage offer that the individual is willing to accept. Equations (2.3) and (2.4) provide expressions for $B^L(w,n,T)$ when $n=1$ and when $n>1$, respectively:

$$B^L(w,1,T) = \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),V(T)\right]+(1-s)V(T)\right\}-0.5s^2\right\} \tag{2.3}$$

$$\begin{aligned} B^L(w,n,T) =& \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),B^L(w,n-1,T)\right]\right.\right. \\ & \left.\left.+(1-s)B^L(w,n-1,T)\right\}-0.5s^2\right\} \end{aligned} \tag{2.4}$$

Proposition II.2. *The value of employment and the reservation wage are smaller the closer is the separation date.*

Proof. See Section 2.9.1.2 in the Appendix. □

Proposition II.2 is also intuitive. Given that $w > b+z$ (and that the duration of benefits b is limited), the value of employment is smaller the shorter the time period over which the worker can receive a certain flow utility of at least w.

Proposition II.3. *As the separation date approaches, the search effort is intensified.*

Proof. Let's denote by $S^L(w,n,T)$ the optimal search effort when a worker has received a layoff notification. Assuming an interior solution, and after some manipulation, $S^L(w,n,T)$ is described by equation (2.5). Given the result from proposition II.2, it can be easily shown from equation (2.5) that the optimal search effort increases as n goes to 1 because the lower bound of the integral gets smaller and the value of the integrand gets larger.

$$S^L(w,n,T) = \beta \left\{ \int_{(1-\beta)B^L(w,n-1,T)}^{\infty} \left[W(x) - B^L(w,n-1,T) \right] dF(x) dx \right\} \tag{2.5}$$

□

The intuition for Proposition II.3 follows from Proposition II.2. As the separation date approaches, the reservation wage decreases and, thus, the marginal returns to searching for another job increases as it is more likely that the worker will find a suitable offer.

Proposition II.4. *Longer PDB increases the value of current employment and the reservation wage, and reduces search effort. These effects are stronger the closer the worker is to the separation date.*

Proof. See Section 2.9.1.3 in the Appendix. □

The first part of proposition II.4 is intuitive. More generous UB duration increases the value of the current job by increasing the value of its termination (i.e. unemployment), and thus the returns to looking for another job are smaller. The intuition behind the second part of proposition II.4 is less evident. The value of unemployment becomes more important for the search decision as the worker approaches the separation date from this employer. Thus, any change in the value of unemployment, in this case due to longer PDB, has a stronger impact on search effort the closer the separation date is.

2.5.3 Modeling the value of employment for a non-notified worker

Now, let $E(w,\phi,N,T)$ denote the continuation value of employment if the worker has not received (yet) a layoff notification, but expects to receive it with probability ϕ. I assume that notifications come at the end of each period but before the worker chooses whether to accept a job offer if he has received one. Thus, there are two reservation wages. Let $x^{E,1}(w,N,T)$ denote the minimum wage offer that a worker would be willing to accept if he received a notification at the end of the period, and let $x^{E,0}(w,\phi,N,T)$ denote the minimum wage offer he would accept if he did not received a layoff notification. Equation (2.6) below defines the continuation value for $E(w,\phi,n,T)$:

$$
\begin{aligned}
E(w,\phi,N,T) = & \underset{s}{Max}\Bigg\{ w+\beta\Big\{\phi sE\left[Max\left(W(x),B^L(w,N,T)\right)\right] \\
& +\phi\left(1-s\right)B^L(w,N,T) \\
& +\left(1-\phi\right)sE\left[Max\left(W(x),E(w,\phi,N,T)\right)\right] \\
& +\left(1-\phi\right)\left(1-s\right)E(w,\phi,N,T)\Big\} \\
& -0.5s^2\Bigg\}
\end{aligned}
\tag{2.6}
$$

Notice that $E(w,\phi,N,T) > B^L(w,N,T)$ per Proposition II.2 because a non-notified worker can continue his employment for a least one more period than if he had received a notification.

Proposition II.5. *Longer PDB increases the value of the reservation wage for a worker who receives a notification at the end of the period (i.e. $x^{E,1}(w,N,T)$).*

Proof. A worker who receives a notification would be willing to take a new job that pays at least $x^{E,1}(w,N,T)$, where $x^{E,1}(w,N,T) = (1-\beta)B^L(w,N,T)$. Per proposition II.4 we know that $\frac{\partial B^L(w,N,T)}{\partial T} > 0$. Thus, $\frac{\partial x^{E,1}(w,N,T)}{\partial T} > 0$. □

Proposition II.6. *Longer PDB increases the value of current employment. It also increases the reservation wage for a worker who did not receive a notification at the end of the period. However, all these effects are zero if the worker has a zero probability of receiving notification (i.e. $\phi = 0$).*

Proof. See Section 2.9.1.4 in the Appendix. □

Proposition II.7. *Longer PDB decreases search effort but only if the worker has a positive probability of receiving a layoff notification ($\phi > 0$). Moreover, the effect is always lower than for workers who have already received a layoff notification.*

Proof. See Section 2.9.1.5 in the Appendix. □

The intuition behind the effects of changes in the PDB for a non-notified worker is similar to that for a notified worker. A longer entitlement to UB increases the value of employment by increasing the value of its termination. This leads to lower search effort because there is a lower probability of finding a job that the worker will take. The difference for non-notified workers arises when they are completely secure at their jobs, i.e. when the probability of receiving a notification is (or is perceived to be) zero. In this case, changes in the duration of UB do not affect the value of current unemployment and are irrelevant for the optimal search effort level.

2.5.4 Implications for JTJ transitions in the data

The probability of a JTJ transition at the end of a period is given by $P(JTJ) = s \times (1 - F(x))$. As described above s is the probability of getting an offer, which is the measure of search effort in the model; $F()$ is the wage offer distribution; and x is reservation wage to take a new job. Following the discussion in the previous section, an increase in the PDB will reduce search effort and increase the reservation wage. Therefore, an increase in the PDB will decrease the probability that a worker takes a new job. Moreover, according to propositions II.4 - II.7 the reduction in $P(JTJ)$ will be stronger for notified workers than for non-notified workers. Among notified workers, the effects will be larger the closer they are to the separation date. For non-notified workers, the effect will be zero if the worker has a zero probability of receiving a notification.

In the data I do not observe whether a worker has received a layoff notification or not. However, I expect that as the establishment's date of closure approaches, a larger proportion of workers should have received a layoff notification. Also, as the closure approaches, a higher fraction of notified workers should be reaching their effective date of separation. Thus, I expect to see two patterns in the data: First, the probability of observing a JTJ transition should increase as the date of the establishment closure approaches, both due to lower reservation wages (Proposition II.2) and to larger search effort (Proposition II.3). Second, the effect (in levels) of a change in the PDB on the probability of a JTJ transition (in comparison to the counter-factual of no change) should be larger as the date of closure approaches, due again to larger effects on job search efforts and on reservation wages. The next section specifies the econometric approach employed to test these implications from the theoretical framework.

2.6 Econometric approach

This section develops the econometric approach to test whether changes in the PDB have any effect on the timing of separation of a worker from a closing establishment. Although the focus of the chapter is on separations due to JTJ transitions during the last year of existence of an establishment, the analysis is repeated for total separations and separations due to JTN transitions.[26] The econometric approach relies on a difference-in-difference (DID) design within a survival analysis framework. The DID design arises from the changes in the PDB that took place in Germany during the 1980s and 1990s, which affected only workers aged 42 years or older. The survival analysis framework allows me to study whether these policy changes had any effect on the timing (and the type) of workers' separation from the closing establishments.

The next subsections describe each of the building blocks of the econometric approach. Subsection 2.6.1 specifies the measure of analysis time that will be used for the survival analysis in this chapter; Subsection 2.6.2 defines the cause-specific hazard rates of separation; Subsection 2.6.3 reviews the policy changes under study and defines the concept

[26] Separations due to JTJ transitions can be directly linked to workers' behavior, whereas JTN transitions can be either initiated by the workers or by the employer (see the discussion in Section 2.4).

of treatment dose; Subsection 2.6.4 introduces alternative measures of the treatment effects; Subsection 2.6.5 provides the identification assumptions required to estimate those treatment effects; finally, Subsection 2.6.6 discusses the estimation procedure and the advantages of the Cox Proportional Hazards Model (CPHM) as the estimation method.

2.6.1 Analysis time

In standard survival analysis researchers define the analysis time t as the time the individual has been at risk of failure since the onset of the risk. For example, when studying unemployment spells, the analysis time becomes the time that the individual has been looking for a job ("failure") since he became unemployed ("onset of the risk"). Researchers usually assign explanatory power to analysis time since it acts as a proxy for processes that are unobserved or difficult to measure. Going back to the example, the analysis time can proxy for the amount of information the individual has collected about the labor market, for potential changes in his reservation wages or in his expectations of finding a job, among other things (Cleves et al., 2010). Thus, in standard survival analysis, the analysis time is defined as $t = 0$ at the onset of the risk of failure and it accumulates as long as the individual has not failed, i.e. $t \in [0, \infty)$.

When studying worker separations from their employer, a natural candidate for analysis time would be the time elapsed since the worker was hired as one could argue that the risk of separation started at that moment. Denote that definition of analysis time as $\tilde{t}$. Thus, $\tilde{t} = 0$ at hiring and accumulates with tenure. In this case, $\tilde{t}$ conveys potential information on, for example, employers and employees learning about the match quality. However, since this chapter focuses on establishments that close down, an alternative is to define the analysis time as the calendar distance until the closure. In this case the analysis time would proxy for (unobserved) information about the financial conditions of the employer, the workers' knowledge of the impending risk of layoff (for instance, the probability of having received a layoff notification), etc. Denote this definition of analysis time as $\bar{t}$. To make this definition operational I define $\bar{t} = 0$ as some moment in time, for example one year before the establishment closure, and study the risk of separation in the following months for all workers who were present at the establishment at $\bar{t} = 0$.

The implications of the different definitions of analysis time become more evident when thinking about the risk of separation. Let $h(t|X)$ be the hazard rate of separation (for any reason) at analysis time t of an individual with observed characteristics X, which is defined as the (limiting) probability that he separates in a given period, conditional on being present at the establishment at the beginning of that period. Thus, if T denotes the time of separation, the hazard rate of separation at analysis time t is defined as:

$$h(t|X = x) = \lim_{\triangle t \to 0} \frac{P\left(t + \triangle t > T > t \middle| \text{At establishment in } t, X\right)}{\triangle t} \tag{2.1}$$

Ideally, the analysis time is chosen such that two individuals with the same value of t and of X must share the same risk of separating from their employer. If the analysis time is defined as $\tilde{t}$ (i.e. tenure), then it is assumed that two individuals with identical tenure and other observables have the same risk of separation. This will obviously fail if one of the individuals is observed in the last month of existence of an establishment, while the other individual is observed many years before the establishment's closure. Of course, calendar distance to closure can be introduced as an additional control in X. The empirical analysis will be based on a CPHM, which controls for the effect of the analysis time non-parametrically and for the effect of the covariates in X using a proportional hazard assumption. This assumption imposes some limitations to the flexibility with which I can control for calendar distance to closure. On the other hand, if the analysis time is defined as $\bar{t}$ (i.e. as the calendar distance to closure), then it is assumed that two individuals present at a firm at the same exact moment (say one year before closure) and with the same value of X have the same risk of separation. Here, the effect of calendar distance to closure is estimated non-parametrically and tenure can be included in X, but again the proportional hazards assumption imposes some restrictions on its effects. So, there is a trade-off about which information one believes it is more important to control for in a flexible way (i.e. non-parametrically): the information conveyed by tenure or the information conveyed by the proximity of establishment closure. The second approach is more sensible and more directly connected with the theoretical framework developed in Section 2.5. Thus, the CPHM specification in this chapter defines analysis time as the calendar distance to closure ($\bar{t}$).

2.6.2 Cause-specific separation analysis

The focus of this chapter is to study how the PDB affects the behavior of workers who are at risk of layoffs. Thus, it is important to distinguish whether separations from an establishment occurred because the worker moved to another employer, i.e. a JTJ transition, or because he moved into nonemployment, a JTN transition.[27] Define the following cause-specific hazard rates of separation:

$$h_{jtj}(\bar{t}|X) = \lim_{\triangle\bar{t}\to 0} \frac{P\left(\bar{t}+\triangle\bar{t} > T > \bar{t},\ \text{JTJ}\middle|\text{At establishment in } \bar{t}, X\right)}{\triangle\bar{t}} \tag{2.2}$$

$$h_{jtn}(\bar{t}|X) = \lim_{\triangle\bar{t}\to 0} \frac{P\left(\bar{t}+\triangle\bar{t} > T > \bar{t},\ \text{JTN}\middle|\text{At establishment in } \bar{t}, X\right)}{\triangle\bar{t}} \tag{2.3}$$

Thus, $h_{jtj}(\bar{t}\big|X)$ and $h_{jtn}(\bar{t}\big|X)$ are the hazard rates of separation due to a JTJ transition and to a JTN transition, respectively, given that the worker is still at the establishment at the beginning of the period.

[27] As discussed in Section 2.4, transitions into nonemployment include both layoffs and voluntary quits. It cannot be labeled as unemployment because some workers may not be looking for a job and others may have entered into retirement. Also, the data does not record self-employment and thus some transitions into self-employment may be miss-categorized as nonemployment.

2.6.3 Policy changes and treatment dose

I define *treatment dose* (D) as the difference (in months) in the PDB that a person would be entitled to when comparing the rules determining UB duration between two periods. The dashed red bars in Figure 2.2 represents the maximum treatment dose by age calculated by comparing the set of rules in effect during the period July 1987-1991 as opposed to those in effect during 1982-1984. Hereafter, I refer to this first comparison as Policy Change #1. Note that the period January 1985-June 1987 is left out of the comparison because this was a period of transitioning into the new unemployment insurance system (see Table 2.1). Also, although my sample includes establishments from West Germany only, I stop the first comparison shortly after the German reunification to avoid potential biases in the analysis coming from this institutional change.

Define the index j as equal to 1 if the observation belongs to the treatment period (i.e. after the policy change), and equal to 0 if it belongs to the pre-treatment period (i.e. before the policy change). Thus, for the analysis of Policy Change #1, $j = 0$ if the observation belongs to the period 1981-1984 and $j = 1$ if it belongs to the period July 1987-1991. As noted in Section 2.3.1 and depicted in Figure 2.1 and in Figure 2.2, this policy change was characterized by a substantial increase in the PDB for workers aged 42 and older with long working histories.

The solid blue bars in Figure 2.2 represents the maximum treatment dose when comparing the rules determining UB duration in the periods 1999-2004 and 1992-1997. Hereafter, I refer to this second comparison as Policy Change #2. In this case, the index j takes the value of 0 if the observation belongs to the period 1992-1997 and of 1 if it belongs to the period 1999-2004. As discussed in Section 2.3.1, this second policy change was the result of increasing the age requirements to qualify for longer UB durations by 3 years, which resulted in a reduction in the PDB for many workers. However, the magnitude of the reduction was much smaller than the previous expansion, especially for older workers, as shown in Figure 2.1 and in Figure 2.2.

Finally, I define *treatment status* as the treatment dose that a worker was actually subject to. In the DID setup it is given by the combination of the treatment dose variable (D) and the index j, i.e. by $D \times j$. In other words, a worker was exposed to treatment status D if he was eligible for treatment dose D and his observation belonged to the treatment period (i.e. $j = 1$) for a particular policy change analysis. Otherwise, the treatment status is equal to 0.

2.6.4 Treatment effects

Define $h^{j,C}(\bar{t}|X, D)$ as the *potential* hazard rate of separation in period j and analysis time $\bar{t}$ if the treatment status were hypothetically equal to C, for a worker with observed characteristics X and who was eligible for a treatment dose D. Thus, the *treatment effects* (TE) at analysis time $\bar{t}$, for a worker with observed characteristics X and a treatment dose of D can be defined in terms of the potential hazard rates of separation as:

$$TE_h(\bar{t},X,D) = h^{1,D}(\bar{t}|X,D) - h^{1,0}(\bar{t}|X,D) \tag{2.4}$$

In words, the TE is the difference between the (observed) factual hazard rates and the (unobserved) counter-factual hazard rates if the worker had not received any treatment. Following the same analysis for the cause-specific hazard rates of separations we obtain equation (2.5), where $k \in K := \{jtj,\ jtn\}$.

$$TE_{h_k}(\bar{t},X,D) = h_k^{1,D}(\bar{t}|X,D) - h_k^{1,0}(\bar{t}|X,D) \tag{2.5}$$

The TE can also be defined in terms of any transformation of the hazard rates. One useful transformation is the *failure function*. The failure function is the probability that a worker has separated from his employer by analysis time $\bar{t}$. The expression for the potential failure function is given by:

$$F^{j,D}(\bar{t},X,D) = 1 - exp\left\{-\int_0^{\bar{t}} h^{j,D}(u|X,D)du\right\} \tag{2.6}$$

Thus, the TE in the failure function is given by:

$$\begin{aligned} TE_F(\bar{t},X,D) =& F^{1,D}(\bar{t},X,D) - F^{1,0}(\bar{t},X,D) \\ =& -exp\left\{-\int_0^{\bar{t}} h^{1,D}(u|X,D)du\right\} + exp\left\{-\int_0^{\bar{t}} h^{1,0}(u|X,D)du\right\} \end{aligned} \tag{2.7}$$

An equivalent of the failure function for JTJ and JTN transitions needs to account for the fact that separations can occur by either of the two competing risks. I work with the *cumulative incidence function* (CIF), which is defined as the cumulative probability of separating due to a specific cause before or up to time $\bar{t}$ (Cleves et al., 2010). Formally, the potential CIF of separation type k at analysis time $\bar{t}$ for workers with observed characteristics (X,D) is defined as:

$$CIF_k^{j,D}(\bar{t},X,D) = \int_0^{\bar{t}} \left\{ h_k^{j,D}(u|X,D) \times exp\left(-\int_0^u \left\{\sum_K h_K^{j,D}(w|X,D)\right\}dw\right)\right\}du \tag{2.8}$$

It can be seen from equation (2.8) that the CIF for any type of separation depends both on the hazard rates for that type of separation and on the hazard rate for the competing type of separation. Thus, although the expression for the TE in the cause-specific CIF is omitted here (see Section 2.9.2.3 for details), it is straightforward that it depends on $h_{jtj}^{1,D}(\bar{t}|X,D)$, $h_{jtn}^{1,D}(\bar{t}|X,D)$ and on the counterfactuals $h_{jtj}^{1,0}(\bar{t}|X,D)$, $h_{jtn}^{1,0}(\bar{t}|X,D)$.

Therefore, in order to estimate the TE either for failure functions or for CIFs, it suffices to estimate the factual hazard rates and the non-treatment counter-factual hazard rates. The next section lays down the identifying assumptions to accomplish this.

2.6.5 Identifying assumptions

The first identifying assumption is that the true potential hazard rates $h^{j,D}(\bar{t}|X,D)$ and $h^{j,0}(\bar{t}|X,D)$ follow a proportional hazard functional form. More specifically, they can be specified as in equations (2.9) and (2.10) below. The term $h_0(\bar{t})$ is called the baseline hazard function and measures the role of analysis time in the risk of separation. The terms $exp(\delta j+\gamma D+\beta X)$ and $exp(\delta j+\gamma D+\beta X+\theta Dj)$ are called the relative hazards, and thus, $\delta j+\gamma D+\beta X$ and $\delta j+\gamma D+\beta X+\theta Dj$ are known as log-relative hazards or risk scores (Cleves et al. 2010).

$$h^{j,0}(\bar{t}|X,D) = h_0(\bar{t})\, exp(\delta j+\gamma D+\beta X) \tag{2.9}$$

$$h^{j,D}(\bar{t}|X,D) = h_0(\bar{t})\, exp(\delta j+\gamma D+\beta X+\theta Dj) \tag{2.10}$$

Besides the proportional hazard functional form, three other important assumptions are embedded in equations (2.9) and (2.10) which are worth highlighting:

1. The only difference between equations (2.9) and (2.10) is given by the term θDj that measures the change in the risk score in the treatment period for workers who are eligible for a treatment dose of D. Thus, treatment dose in the treatment period affects only the risk score, not the baseline hazard. This is a standard assumption in DID survival models.[28] I relax this assumption in the empirical analysis when I look at time-varying estimates of θ.

2. Equations (2.9) and (2.10) assume that the effect of the treatment dose D on the risk score, conditional on X, is linear.[29] This assumption is less flexible than a non-parametric specification using a full set of dummies, one for each treatment dose value. However, it allows the efficient use of all of the variation in the PDB, which is

[28] It is similar, for instance, to controlling for the role of time without interacting it with the treatment variable when working with discrete-time survival analysis models.

[29] A similar strategy is used in Haan and Prowse (2010) for identifying the effect of changes in entitlement periods on labor market status.

important because the sample sizes for looking at each specific treatment dose value are relatively small.[30]

3. Finally, the definitions of the risk scores in equations (2.9) and (2.10) implicitly assume a common trend in the potential non-treatment risk scores.[31]

Regarding the last point, assuming a common trend in the potential non-treatment risk scores actually precludes a common trend in potential non-treatment hazard rates, failure functions or CIFs. This is a common problem in DiD methods, namely the scale dependence of identifying assumptions (Lechner, 2010). In other words, a common trend on a given outcome (in this case the risk score) will not hold for non-linear transformations of that outcome (e.g. the hazard rate). However, this assumption is convenient because it allows me to build the TEs from the ground up. I first estimate the counter-factual non-treatment risk scores in the treatment period. Then I can recover the counter-factual non-treatment hazard rates. Using the counter-factual non-treatment hazard rates I estimate the counter-factual non-treatment failure function and CIFs. After having calculated all these objects I can easily estimate the TEs. Moreover, the common trend assumption in the potential non-treatment risk scores, joint with the proportional hazard functional form assumption, allows me to determine the direction of most of the TEs (with the exception of those for the CIFs) based solely on the sign of θ. To see this, plug equations (2.9) and (2.10) into equations (2.4) and (2.7). After a few manipulations, the following expressions

[30]Moreover, as shown by Schmieder et al. (2010, 2012), the increase in the duration of nonemployment spells per month of increase in the PDB is similar across age thresholds, even when the total increase in the PDB is different. This also supports the linearity of the specification of the effect of D.

[31]To see this, define $RS^{j,0}(X,D)$ as the potential non-treatment risk score in period j for workers with observed characteristics X who were eligible for treatment dose D. The common trend assumption implies that the differences in the potential non-treatment risk scores between the pre-treatment period and the treatment period would have been the same regardless of treatment dose, conditional on having the same values of X. In other words, it assumes that $\left\{RS^{1,0}(X,D) - RS^{0,0}(X,D)\right\} = \left\{RS^{1,0}(X,0) - RS^{0,0}(X,0)\right\}\ \forall D$. The definitions of the risk scores in equations (2.9) and (2.10) satisfy this assumption:

$$\begin{aligned} RS^{1,0}(X,D) - RS^{0,0}(X,D) &= RS^{1,0}(X,0) - RS^{0,0}(X,0) \\ \delta + \gamma D + \beta X - (\gamma D + \beta X) &= \delta + \beta X - (\beta X) \\ \delta &= \delta \end{aligned}$$

can be obtained (see Appendix 2.9.2 for more details):

$$TE_h(\bar{t},X,D) = h^{1,0}(\bar{t}|X,D)\,[exp(\theta D)-1] \tag{2.11}$$

$$TE_F(\bar{t},X,D) = \left(1-F^{1,0}(\bar{t},X,D)\right)\left[1-\left(1-F^{1,0}(\bar{t},X,D)\right)^{exp(\theta D)-1}\right] \tag{2.12}$$

Equations (2.11) and (2.12) show that the direction of the two alternative measures of the TE are given by the sign of θ. Specifically, if $\theta = 0$, both TE equal zero; if $\theta > 0$, meaning that treatment dose increases the risk of separation in the treatment period, then $TE_h(\bar{t},X,D) > 0$ and $TE_F(\bar{t},X,D) > 0$; similarly, if $\theta < 0$, meaning that treatment dose reduces the risk of separation, then $TE_h(\bar{t},X,D) < 0$ and $TE_F(\bar{t},X,D) < 0$.

Notice also that for small values of θD, the expression $exp(\theta D)-1$ can be approximated by θD. Thus, it follows from equation (2.11) that if θ is sufficiently small, it can be directly interpreted as the percentage change in the hazard rate of separation resulting from a one-month expansion in the PDB.

The analysis of the TE on the hazard rates for each type of separation follows the same structure as in equations (2.4) and (2.11). Thus, the TE for the cause-specific hazard rates of separation is given by equation (2.13):

$$TE_{h_k}(\bar{t},X,D) = h_k^{1,0}(\bar{t}|X,D)\,[exp(\theta_k D)-1] \tag{2.13}$$

The TE for a cause-specific CIF depends not only on the effects of the PDB on the cause-specific hazard rates of separation alone but on the alternative cause hazard rates as well. More formally, the TE for the cause-specific CIFs are given by equation (2.14) (see Appendix 2.9.2 for its derivation):

$$TE_{CIF_k}(\bar{t},X,D) = \int_0^{\bar{t}} \Bigg\{ h_k^{1,0}(u|X,D) \times exp(-\int_0^u \Big\{ \sum_{i \in K} h_i^{1,0}(w|X,D) \Big\} dw)$$
$$\times \left[exp\left(\theta_k D - \int_0^u \Big\{ \sum_{i \in K} h_i^{1,0}(w|X,D)\,(exp(\theta_i D) - 1) \Big\} dw \right) - 1 \right] \Bigg\} du \quad (2.14)$$

Thus, the TE will be zero only if both θ_{jtj} and θ_{jtn} are zero. For example, even if θ_{jtn} is zero we would have $TE_{CIF_{jtn}}(\bar{t},X,D) > 0$ if $\theta_{jtj} < 0$ and $D > 0$. In other words, even if the PDB does not directly affect the hazard risk of JTN transitions, the cumulative probability of separation due to a JTN transition increases when the increase in the PDB reduces the hazard risk of JTJ transitions.[32]

2.6.6 Parameter estimation

The coefficients in equations (2.9) and (2.10) are estimated using a Cox Proportional Hazard Model (CPHM).[33] The analysis includes all workers aged 38 to 56 years who are present in an establishment exactly one year before its closure and who have worked at least 64 months in the prior seven years. The date that marks exactly one year before plant closure is labeled $t = 0$, and all time-varying covariates, with the exception of age, are fixed at that moment. Age (and the corresponding treatment dose) is calculated using the year of birth and the year of closure of the establishment. Workers are followed until they separate

[32]A similar situation arises in multinomial logit models, which are used to estimate discrete-time competing hazard models. In multinomial logit models, the marginal effect of a covariate z on the probability of a given outcome depends not only on the coefficient of z on that outcome equation, but also on its coefficients for the other (competing) outcomes. Cameron and Trivedi (2005, page 502) provides further details on computing marginal effects in a multinomial logit model.

[33]The CPHM leaves the baseline hazard unspecified and estimates the coefficients in the risk score by comparing individuals at failure times. If workers separate at $\bar{t}$, the CPHM compares the characteristics of that worker to the characteristics of other workers who were present at any establishment at $\bar{t}$ and did not separate, i.e. workers in the same risk set. By doing the comparison at every failure time, the coefficients of the risk scores in equations (2.9) and (2.10) are estimated by maximum likelihood in order to maximize the probability of having the observed order of separations. After estimating the coefficients of the risk score, the baseline hazard (and the functions related to it, such as the baseline failure function) can be recovered non-parametrically.

from the establishment and the type of separation, i.e. a JTJ transition or a JTN transition, is recorded.

The CPHM has two characteristics that make it ideal for the empirical problem at hand. First, the non-parametric estimation of the baseline hazard is convenient when using $\bar{t}$, or the calendar distance to the establishment's closure date, as the definition for the analysis time. This is because the risk of separation increases faster as the establishment approaches its closure and it is unlikely that any parametrization of the baseline hazard will have enough flexibility to accommodate this pattern. Second, the CPHM does not attach any specific significance to the value of $\bar{t}$. The analysis time is only used to order the data and to define the risk sets for estimation purposes (Cleves et al., 2010). Thus, labeling $t = 0$ as one year before plant closure has no special meaning other than defining the risk sets for estimation of the coefficients of the risk score.

It can be shown that if, after conditioning on X and D, the hazard rates $h_{jtj}(\bar{t}|X,D)$ and $h_{jtn}(\bar{t}|X,D)$ are independent, then the (log) likelihood of observing the failure times for each type of transition can be factored into two parts, where each part depends only on the parameters for one type of transition. Thus, the estimation can proceed by maximizing the two component parts separately (Jenkins, 2005) and treating separations due to the other type of transition as randomly censored observations. In the CPHM, this is achieved just by keeping those observations in the risk sets until they have failed due to the competing risk and excluding them thereafter. However, if after conditioning on X and D, the cause-specific risks are not independent, then treating separations due to the competing risk as randomly censored observation will introduce bias in the results.

Identification of the parameters in the case of correlated risks is more difficult. It is usually done by introducing unobserved components in $h_{jtj}(\bar{t}|X,D)$ and in $h_{jtn}(\bar{t}|X,D)$ that are allowed to be correlated but are independent of X. I conducted several tests that have allowed me to conclude that the unobserved heterogeneity and its correlation between the competing risks can be safely ignored.[34] Thus, I treat each separation risk, after conditioning on X and D, as independent.

2.7 Results

2.7.1 CPHM estimation results

Table 2.3 presents the CPHM estimation results for the analysis of the first policy change. As mentioned earlier the sample consists of workers aged from 38 to 56 years who were present at a closing establishment one year before its demise. Also, all workers in the sample had at least 64 months of prior working history in the last seven years. Estimations are weighted by the probability of observing each establishment and standard errors take into account the stratification of the sampling design (by establishment size) and the clustering of workers at the establishment level (see footnote 16).

The estimates of the parameters of interest θ are highlighted in Table 2.3. Following the interpretation of θ discussed in Section 2.6, I find that a one-month increase in the PDB resulted in a decrease of 0.4% in the hazard rate of any separation. This is a small and not statistically significant effect. However, it masks two opposing effects on the hazard rates for JTJ transitions and for JTN transitions. I find that a one-month expansion in the PDB decreased the hazard rate of JTJ transitions by 2.1%. This effect is significant at the 5% level. I also find that a one-month expansion in the PDB increased the hazard rate of a JTN transition by 1.2%, although this effect is not statistically significant (p-value of 0.154).

A potential concern with the analysis of the effects of the first policy change is that the

[34]This can be explained since, given that everybody will eventually leave the establishment within a year or less, there is less room for any unobserved heterogeneity to have an important role in explaining the observed separation patterns. Two different tests corroborate this empirically. First, I assume that each separation risk is independent and run my analysis specifying an unobserved component that follows either a gamma or a log-normal distribution (using the frailty option in Stata). For both risks I find that the variance of the unobserved component is minimal. Then, I allow the unobserved components to be correlated and to follow either a bivariate normal distribution or a discrete distribution with three points of support for each risk. Using maximum likelihood estimation I find that the variance of the unobserved components and their correlation are again small (and not statistically significant). Moreover, the coefficients on all the covariates are robust to the introduction of the unobserved components in the analysis.

treatment dose increases monotonically with age. Thus, one may argue that the estimated effects are coming only from the very large expansions in the PDB for the older workers, who would have less opportunities in the labor markets and also more incentives to look for earlier retirement (even though my sample only includes workers up to 56 years old). In order to investigate this issue, in Table 2.4 I re-estimate the CPHM allowing for differential effects for two age groups: workers who were 42-48 years old and workers who were 49-56 years old. Panel A presents the coefficients of the interactions between the corresponding age group dummy and the time dummy for being in the treatment period. Panel B linearizes those coefficients by dividing the effect from Panel A by the average increase in the PDB for each age group. I find that the increase in the PDB reduced the hazard rates of JTJ transitions for all workers, not only the older ones. In fact, the linearized effects are larger (in absolute value) for workers in the 42-48 age group than for workers in the 49-56 age group, although the standard errors are sufficiently large to prevent concluding that the estimates are statistically different from each other.

Table 2.5 presents the CPHM estimation results for the analysis of the second policy change. The point estimate of θ for JTJ transitions is smaller than the one found for Policy Change #1. I find that a one month reduction in the PDB increased the hazard rate of JTJ transitions by 1.5%. However, this effect is not statistically significant at conventional levels (p-value of 0.172). The estimates of θ for any separations and for JTN transitions are very small and not statistically significant as well.

One possible explanation for why the estimates of θ are different in magnitude between the two policy changes may be non-linearities in the effects of changing the PDB depending on the starting point: Policy Change #1 implied a change in the potential earnings profile starting at month 13 of unemployment for workers aged 42 or older. In contrast, Policy Change #2 implied a change in the potential earnings profile at month 13 for workers age 42-44; at month 19 for workers aged 45-46; at month 23 for workers aged 49-51 and at month 27 for workers aged 54-56. It is plausible that workers' search decisions are less responsive to changes in the right tail of potential future unemployment durations because they do not expect their actual income profile to be affected. This is more likely to be true for workers aged 45 and older for whom the maximum PDB was still very generous even after the reduction implied by the second policy change. For example, using data from my analysis sample for the period 1992-1997, I found that the average 50-year-old worker who separated from the closing establishment by entering unemployment had an UB spell duration of 13.2 months, while his maximum entitlement was 26 months. Thus, a reduction in the PDB to 22 months, as it happened after 1999, would likely have little effect on his pre-displacement search behavior. In fact, Figure 2.3 shows that the reduction to 22 months only "bites" 33% of the UB spells. These numbers are less dramatic if one excludes spells that are clustered at the exhaustion point (26 months), since these spells may belong to workers who are less likely to exert effort to search for a job or who are not capable of finding one. Excluding the spells that exhausted benefits, I find that reducing the PDB to 22 months would affect only 4% of the UB spells. Figure 2.4 presents similar analysis on the potential "bite" of the reduction in the PDB that happened after Policy Change #2 (excluding spells that exhausted benefits) for workers aged 42-56. It is clear from the graph that this "bite" for most ages is very small.

In order to further investigate the non-linearity of the effects depending on the strength of the potential "bite" of the policy change, I re-estimate the CPHM models using only workers aged 38 to 44 years. Workers aged 38 to 41 years act again as the control group, for whom there were no changes in their PDB. Workers aged 42 to 44 years are the treated group. Policy Change #2 had a larger "bite" on their potential UB durations as shown in Figure 2.4. Moreover, for this group of workers, the treatment dose under Policy Change #1 and Policy Change #2 are exactly the same but with opposite sign (see Figure 2.2). In other words, for workers aged 42 to 44 years Policy Change #2 just reversed the previous expansion in their PDB by setting it equal to 12 months. The new estimates of θ are presented in Table 2.6. The point estimate of θ for JTJ transitions is now larger than before. I find that a one-month decrease in the PDB led to a 2.6% increase in the hazard rate of JTJ transitions (p-value 0.053). Thus, results from the analysis of Policy Change #2 corroborate the previous evidence from Policy Change #1: longer (shorter) PDB decreases (increases) the probability that workers leave to a new employer before the establishment closes.

Although it is not the main focus of this chapter, I discuss also some of the coefficients associated with other observed worker characteristics. I find that men, shorter-tenured workers and white-collar workers are more likely to exit the closing establishments earlier, especially because of JTJ transitions. These are workers who may have better opportunities to find new employment. For example, for the case of white-collar workers the literature has provided evidence that they are less negatively affected by a separation from their employers since a smaller fraction of their acquired skills are job-specific. In contrast, blue-collar workers seem to have skills that are less transferable across jobs (Podgursky and Swaim, 1987; Kletzer, 1989). Therefore, it is plausible that white-collar workers have better opportunities for finding new jobs than blue-collar workers.

Regarding establishment size, I find strikingly opposite results between the time-periods covered by the two policy changes, especially with respect to JTJ transitions. For the time period covered in the analysis of the first policy change (1982-1984 and 1987-1991), workers in larger establishment have lower hazard rates of JTJ transitions than those in smaller establishments. The converse is true for the time period covered in the analysis of the second policy change (1992-1997 and 1999-2004). The reasons why there is this difference in the effect of establishment size over time has yet to be rigorously investigated.

2.7.2 Placebo tests

Before continuing the discussion of the empirical results, I present evidence from placebo tests that supports the validity of the causal nature of the previous findings. First, I test for common trends in the risk scores prior to the policy changes. In order to conduct these tests I use only information from the pre-treatment period and pretend that the change in the PDB happened at some earlier date in the pre-treatment period. In the case of Policy Change #1, I assume that the change in the PDB applied to establishments that closed in 1984 but not to establishments that closed in 1982-1983. In the case of Policy Change #2, I assume that the change in the PDB applied to establishments that closed in 1995 to 1997, but not to those that closed in 1992 to 1994. This artificial earlier change in the PDB is the placebo. Thus, it should not have any effect on the risk scores unless there were differential trends prior to the actual changes in the PDB between those who were affected by them and those who were not. The credibility of the common trend assumption (and of the identification strategy) would be enhanced if the coefficient of the interaction of the treatment dose (D) and the dummy marking the artificial change (placebo) is equal to zero. Table 2.7 presents the results of these tests. All the coefficients are not statistically different from zero at conventional significance levels. Furthermore, the estimated coefficients for JTJ transitions are positive rather than negative. The point estimates are also particularly close to zero for Policy Change #1 (Panel A) and for Policy Change #2 when I only include workers from ages 38-44 (Panel C). Thus, the tests shows no evidence of differential trends in the non-treatment risk scores between the treated and control groups prior to the changes in the maximum PDB.

The second placebo test is to falsely assume that the policy changes affected a group of workers who were actually not affected by them. In my analysis, I assume that the policy changes affected workers of age 37-40, and I use as control group individuals of age 32-36. I re-run the CPHM including a dummy variable for being in the age group 37-40, a dummy variable for being in the treatment period, and an interaction of both. Since the PDB for workers younger than 42 years old remained unchanged, the interaction term should not be different from zero unless there were a change in the age gradient for the risk scores between the pre-treatment period and the treatment period. Thus, the over-identifying assumption is that the age gradient for workers who were not affected by the changes in the PDB remained stable before and after the policy change. The results of these tests are shown in Table 2.8. I find that the interaction terms are not statistically significant. Moreover, the point estimates are very small in magnitude. For example, compare the estimate -0.030 for JTJ transitions in Panel A (Policy Change #1) with the estimate -0.278 obtained for workers age 42-48 years old in Panel A of Table 2.4. For the case of Policy Change #2, the placebo estimate for JTJ transitions is -0.022 (Table 2.8, Panel B), which is not only small but also implies that the risk scores of a JTJ transition for workers aged 37-40 decreased in the treatment period in comparison to the younger control group of workers aged 32-36. This finding is opposite to the results discussed earlier in Table 2.6, which indicated that workers aged 42-44, whose PDB were reduced in the treatment period, were finding new jobs faster than before in comparison to their younger control group (workers aged 38-41). In conclusion, the results from these tests support that the changes in the PDB caused the differential change in the JTJ transition risk scores (in the treatment period) for workers aged 42+ years in comparison to their younger counterparts.

The last test relies on the insights provided by the theoretical discussion in Section 2.5. The changes in the PDB should only affect workers who have received a layoff notification or perceive that they will receive one. Thus, the effects of a change in the PDB should be stronger as the closure date approaches since more workers would have received a layoff notification or would be aware of the impending closure of the establishment. Figure 2.5 helps to clarify this argument. One year before closure, establishments have already reduced their personnel by about 20%. Thus workers are likely to be aware of the distressed situation of their employers. In contrast, three years before closure the number of employees is relatively stable. Thus workers who were present at the establishments three years before closure would be less likely to feel job insecure.

Table 2.9 presents the CPHM estimation results for workers who were present at the establishments one, two and three years before their closure. The sample selection criteria are the same as before: workers aged 38 to 56 years who have worked at least 64 months in the last seven years. In each case, the worker is followed only up to 12 months. If at the end of that period the worker is still present at the establishment then his failure time (and type) is treated as censored. Notice that the exercise for workers present three years before closure can only be done for the second policy change since the administrative records start at 1975 and thus it is not possible to identify workers with sufficiently long working history.

The estimated effects for workers present two years before closure are similar to those obtained for workers present one year before closure that were discussed earlier. However, in the former case the coefficients are less precisely estimated. This may be explained because, as mentioned in Section 2.3, employers have to communicate in advance their decision of mass-layoffs to the works council and the local employment agency. Thus, workers present at the establishments two years before closure may become aware that, on average, their employers are going to downsize by about 20%. However, it is important to recall that the estimated parameters θ measures the proportional change in the hazard rates as a result of a one-month expansion in the PDB. The final effect in levels of the hazard rates, failure function and CIF would depend also on the counter-factual non-treatment hazard rates, which increase substantially as the closure dates approaches. Thus the changes in the PDB would have a much stronger effect in levels in the last year of existence of an establishment than two years before its closure, as predicted by the theoretical model.

Panels B and C shows that for workers present three years before closure, the estimated parameters θ are much smaller, which is consistent with the previous argument that the level of job insecurity should be smaller as well.

2.7.3 Time-varying effects

As mentioned above, the theoretical model in Section 2.5 predicted that the effects of changes in the PDB on the probability of JTJ transitions should get stronger as the closure approaches. However, the specification of the hazard rates in equations (2.9) and (2.10), and the empirical results in Tables 2.3, 2.5 and 2.6 imply that the treatment dose only delivers a constant proportional change in the hazard rates during the last year of existence of the establishment. Nevertheless, the TE in the hazard rates when measured in levels, as in equations (2.4) and (2.5), will increase with the proximity to closure as predicted by the theoretical model because the underlying counter-factual non-treatment hazard rates also increase with the proximity to closure. However, in order to investigate if I am imposing a strong restriction by assuming a constant proportional change, I fit a more flexible specification of the hazard rates. I re-estimate my base CPHM for workers present one year before closure and allow θ to vary with the proximity to the establishment demise as measured in quarters (the fourth quarter being when the establishment closes). Table 2.10 presents the estimation results. In the case of Policy Change #1, I do not find a clear pattern of the estimates of θ for JTJ transitions as closure approaches. Moreover, I cannot reject the null hypothesis of equality of the coefficients of θ across quarters (the p-value of the F-test of joint equality of coefficients is given in parentheses in Table 2.10). In the case of Policy Change #2, the quarterly coefficients of θ seems to get smaller as closure approaches, especially when I focus only on workers aged 38-44. Although, I again cannot reject the null hypothesis of equality of coefficients. Therefore, I will keep the original specification as the preferred one, which delivers a constant proportional change in the hazard rates of JTJ transitions and an effect that is increasing with the proximity to closure when measured in levels. This is evident in figures 2.6 to 2.11, which show larger treatment effects on the hazard rates as the establishment approaches closure, and in particular in the last month of existence of the establishment. These figures are discussed in more detail below.

2.7.4 Treatment effects calculations for the average treated worker

The average treated worker in Policy Change #1 had an increase in his PDB of about 13.4 months, or 112%.[35] Therefore, using the estimates of θ from Table 2.3, I obtain that

[35] I define the average treated worker as a worker whose observed characteristics are evaluated at the mean values among all workers affected by the change in the PDB.

his hazard rates of all separations decreased by 5.6% (p-value 0.231), his hazard rates of JTJ transitions decreased by 24.6% (p-value of 0.008) and his hazard rates of JTN transitions increased by 19.5% (p-value 0.084).[36]

Using the formulas in equations (2.11), (2.12), (2.13) and (2.14), I also estimated the TE in levels on the hazard rates, failure function and cause-specific CIF. These are shown in Figure 2.6, Figure 2.7 and Figure 2.8, along with the point-wise 2nd and 98th percentiles of 500 bootstrap replications.[37] I cannot reject (with 96% confidence) that the TE on the hazard rates for all separations and the failure function are equal to zero, which is not surprising given that the estimate of θ for all separations in Table 2.3 was not statistically significant. In the case of JTJ transitions, the TE on the hazard rates are negative and become stronger in the last two quarters up to establishment closure, as supported by the theoretical framework. I also find that the increase in the PDB led the average treated worker to be about 9.7 percentage points less likely to have moved to another establishment by the time of closure. I can reject the null hypothesis of a zero effect with 96% confidence, although the confidence intervals are still relatively wide. The policy change also led the average treated worker to be about 7.5 percentage points more likely to have separated due to a JTN transition by the time of closure (see Figure 2.8). Notice that this effect is statistically significant even though the estimate of θ in the hazard rates for JTN transitions is not. This is because although there is no statistically strong evidence that the expansion in the PDB directly affected the hazard rates of a JTN transition, there is strong evidence that it decreased the hazard rate of a JTJ transition. Thus, the expansion in the PDB made workers less likely to voluntarily abandon the closing establishment for a new job and therefore increased their probability of being effectively separated by entering nonemployment.

For the analysis of Policy Change #2, I focus only on treated workers aged 42-44 years old, since they were the ones more affected by the policy change as discussed earlier. The

[36] Standard errors were calculated using the delta method and p-values were obtained using the normal distribution.

[37] The hazard rates were smoothed using an Epanechnikov kernel with a bandwidth of 60 days.

average treated worker had a reduction of 6.9 months in his PDB. As a result, using the estimates of θ from Table 2.6, I find that his hazard rate for all separations increased by 9.5% (p-value 0.059), his hazard rate for JTJ transitions increased by 19.6% (p-value of 0.047) and his hazard rate for JTN transitions increased by 3.1% (p-value 0.391). I also find that the average treated worker was 4.0 percentage points more likely to have moved to a new job and 0.7 percentage points less likely to have entered nonemployment by the time of closure. However, these effects are not statistically significant, as can be seen in Figure 2.10 and in Figure 2.11.

To summarize, the TE calculations show that an increase (decrease) in the PDB reduces (increases) the hazard rates of JTJ transitions and the probability of moving to a new job before the establishments closes down. The statistical evidence is stronger for the first policy change, that implied a large extension in the PDB, than for the second policy change, that implied a more moderate reduction in the benefits.

2.7.5 Estimates of θ by subgroups

Finally, in this section I analyze how the estimates of the parameter θ vary across different subgroups. I focus the discussion mainly on the estimates of θ in the JTJ transition equations. First, I distinguish between low-wage earners and non-low wage earners. Low earners are more likely to receive social assistance to bring their income to an established social minimum. Thus, as long as they are entitled to social assistance, changes in their PDB do not actually change their expected income profile. Non-low earners, in contrast, are more likely to have deductions in their UA payments and thus an expansion in the PDB would have a higher impact on their expected income profile. Therefore, I expect that changes in the PDB would have a smaller effect for low-wage workers than for non-low-wage workers. To implement this test, I define low-wage workers as those whose daily wage rate is less than two-thirds of the median wage, as it is defined in official statistics (Lo et al., 2012). Panels A in Tables 2.11 and 2.12 present the estimates of θ for low-wage and non-low-wage workers for the analysis of both policy changes. For Policy Change #1, I find a larger negative coefficient θ for JTJ transitions for the case of non-low-wage workers, as expected. However, the point estimates between low-wage and non-low-wage workers are not statistically different as indicated by the p-value from the test of the null hypothesis of equality of coefficients (shown in parentheses). For Policy Change #2, I found the opposite results: the point estimate for low-wage earners is more negative than for non-low-wage earners. However, again I cannot reject the null hypothesis of equality of coefficients.

I also estimate the coefficient θ by gender, occupation and tenure. In general, I find that in the analysis of Policy Change #1 the increase in the PDB had stronger negative effects on the hazard rates of JTJ transitions for those subgroups that were more likely to move earlier to new jobs. In other words, the increase in the PDB had stronger effects for males, white-collar workers and workers with shorter tenure. Two alternative explanations may account for this pattern. On the one hand, these groups of workers may exert more OTJS effort upon the imminent risk of job loss, which explains why they are more likely to separate earlier. Thus, increases in the PDB can have stronger incentives to discourage OTJS for them. On the other hand, it is possible that all workers exert comparable levels of OTJS but the subgroups mentioned above are on average more successful in finding new jobs. Then, everything else equal, increases in the PDB would result in larger observed effects in reducing JTJ transitions for them than for workers who are less likely to get job offers. Since I do not directly observe OTJS efforts (or reservation wages) but only JTJ transitions, I cannot determine which alternative explanation is more likely to be true. It is also important to mention that for all of these groups of workers, although the point estimates are in some cases very different, I cannot reject the null hypothesis of equality of coefficients. For the case of Policy Change #2, the difference in the coefficients' point-estimates are less dramatic and again I cannot reject the null hypothesis of equality of coefficients.

Two cases where I can reject the null hypothesis of equality of coefficients are the estimates of θ by educational level and establishment size for the case of Policy Change #1. Moreover, the estimates of θ for JTJ transitions are positive for workers with college or university degree and for workers at large establishments (501-1000 workers). These results go against the predictions from the model in Section 2.5. However, an implicit assumption in the model was that changes in the PDB have no effect on the probability of layoff. I found evidence that this assumption may not hold for workers with a college degree or workers at large establishments. For these groups of workers the expansion in the PDB also increased their hazard rates of JTN transition, as can be shown by the positive and statistically significant estimates of θ. If I allow expansions in the PDB to increase the risk of layoff in the model, and also let workers be aware of this effect, then the total effect on the probability of JTJ transition is ambiguous. On the one hand, the increase in job insecurity due to the expansion in the PDB increases on-the-job search, reduces reservation wages and increases the probability of JTJ transitions. On the other hand, the expansion in the PDB increases the value of future unemployment, reduces on-the-job search, increases reservation wages, and reduces the probability of JTJ transitions. A priori it is not possible to determine which effect is larger. Further evidence that the increase in job insecurity may drive the positive estimates of θ in the JTJ transition equations is given by the results in Policy Change #2. In this case, the reduction in the PDB did not have an effect on the hazard risks of JTN transitions for college graduates, and I find a negative estimate of θ on the hazard rates for JTJ transitions, as predicted by the model.

2.8 Conclusions

This chapter aims at filling the gap in the literature on the effects of UB on employed workers' behavior, particularly JTJ transitions. The theoretical framework presented in this chapter shows that such effects should vary depending on workers' job insecurity, with larger effects for workers who have received a layoff notification and have a short period remaining before separation from their employers. Therefore, studying the effects of UB on workers' behavior can be empirically challenging because in general it is difficult to obtain measures of job insecurity that are uncorrelated with workers' unobserved characteristics. In this chapter, I overcome this problem by focusing on workers at establishment closures in West Germany. Using difference-in-difference methods within a competing risks survival analysis, I test whether changes in the PDB affect the timing of the workers' separation from the closing establishments, distinguishing between JTJ transitions and JTN transitions. The identification strategy relies in exploiting changes in the PDB in the mid-1980s and mid-1990s for older workers in Germany.

In general, I do not find evidence that changes in the PDB affects the hazard rates for JTN transitions. This can be explained by the fact that I focus my analysis on workers present at the establishments one year before their closure. Thus, all of them will be laid off within a year and considerations other than UB, for example age and seniority, are likely to be the most important factors in the timing of the layoffs.

In contrast, I find evidence that changes in the PDB affect workers' probability of moving to another job before the closure of an establishment, i.e. the hazard rates of JTJ transitions. As mentioned above, I analyze two changes in the PDB in Germany. The first change, which occurred in the mid-1980s and is referred in the chapter as Policy Change #1, brought an expansion in the maximum PDB for workers aged 42 or older. I find that the hazard rates of JTJ transitions decreased by approximately 2.1% per month of increase in the PDB. The second change, which occurred in the mid-1990s and is referred in the chapter as Policy Change #2, brought a reduction in the maximum PDB for certain age-groups of workers aged 42 or older. I find that a one-month reduction in the PDB increased workers' hazard rates of JTJ transitions by 1.5%, although this effect is not statistically significant. The smaller effects in Policy Change #2 in comparison to Policy Change #1 can be explained by the moderate reductions in the maximum PDB in the second policy change as opposed to the previous large expansions in the first policy change. In fact, even after the reductions in the maximum PDB, the average length of the entitlement to UB was still very generous. As a consequence, only for a small fraction of workers would the second policy change have had an effect in their expectations of potential future income.

To compare my results with those found in previous work, I calculate the implied elasticities. For the reasons discussed in the previous paragraph I focus the analysis on the results from Policy Change #1. For the average treated worker, the elasticity of the hazard rates of JTJ transitions to the extension in the PDB was -0.23. The main difficulty in benchmarking this elasticity is that previous studies have analyzed the effects of changes in the level of benefits rather than changes in their duration. To make the comparisons as close as possible, I transformed the expansion in the PDB into a change in the present discounted value of potential UB receipt. I did this calculation for the average treated worker, who was entitled to 12 months of UB before the policy change and to 25 months of UB after the change. I assume that before the expansion in the PDB, the worker collects UB for 12 months at a replacement rate of 67% and then he collects UA for 13 months at a replacement rate of 35% (see footnote 5). After the expansion in the PDB the worker collects UB for 25 months. The monthly discount rate is 0.99. Under these assumptions, I find an elasticity of the hazard rate of JTJ transitions to the potential increase in UB receipt of approximately -0.84.

Light and Omori (2004) found that the elasticity of the probability of a JTJ transition (over a period of 15 weeks) with respect to the level of UB was only -0.09. This small elasticity can be explained by the fact that the authors did not use a sample of workers at high risk of job loss, but a representative sample of the working population. As discussed in Section 2.5 the generosity of UB would only matter to workers who feel at risk of job loss. In contrast in Chapter I, using older Americans workers at downsizing firms (i.e. firms that have recently reduced personnel), I find that the elasticity of the monthly probability of JTJ transitions with respect to the replacement rate provided by UB was -0.88. Thus, the elasticity I find in this chapter for the analysis of Policy Change #1 is similar to the one I found in the US for older workers at downsizing employers. However, one need to be cautious about these comparisons because the generosity of the UB systems in the US and Germany are very different.[38]

Evidence presented in this chapter should encourage further studies on this topic. One avenue of research would be the incorporation of UB effects on workers' search behavior in the analysis of optimal design of unemployment insurance systems. Previous studies have mostly focused on the effects that UB have on transitions from unemployment to employment. Thus, they have neglected the fact that UB can also affect the entry rate into unemployment by affecting the behavior of workers. One exception is Wang and Williamson (1996). The authors consider an environment where the worker's probability of remaining employed depends on his work effort. Higher UB creates incentives for the worker to shirk and thus makes job destruction endogenous. In their analysis, Wang and Williamson (1996) show that the optimal system involves a large penalty for a transition from employment to unemployment (to discourage shirking) and a large subsidy for a transition from unemployment to employment (to encourage search effort). Put differently, workers receive a large drop in consumption in the first period of unemployment and a large reemployment bonus. There is no empirical evidence in favor of the work effort-UB relationship in the literature (Fredriksson and Holmlund, 2006). However, this chapter presents evidence that workers at risk of layoff may exert less search effort to find an alternative job when they are entitled to more generous UB. Thus the recommendations from Wang and Williamson's analysis remain valid. In fact, many existing unemployment insurance systems involve (although Germany's does not) a waiting period before benefits are paid out. The existence of such a waiting period may be defended as a way to discourage entry into unemployment.[39] Another policy that could be considered is the introduction of search requirements

[38]In Germany, the PDB in the pre-treatment period (before it was extended) was 12 months for the sample of workers under analysis (i.e. with long-labor force attachment). Moreover, the replacement rate of UB was above 60%. In the US the PDB is, under normal circumstances, about 26 weeks (or 6 months). Also, the group of workers studied in Chapter I consisted of men aged 50 years or older for whom the average replacement rate, taking into consideration states' limits on weekly benefits, was effectively about 35%.

[39]Other considerations include for example potential benefits in reducing the administrative burden of the unemployment insurance system since many unemployment spells may end before the waiting period is over or it may discourage workers who expect to be reemployed soon to claim UB.

for workers who have received a layoff notification, just as those requirements exist for unemployed workers. Both theoretical and empirical literature have provided support for the case of imposing penalties on less active job search for the unemployed (Fredriksson and Holmlund, 2006). A similar system could be implemented for employed workers who have received a layoff notification.

Another avenue for further research is the role that UB can play in managing human resources at distressed firms. Recent work by Brown and Matsa (2012) has shown that employers in the US who are experiencing financial distress receive fewer applications for open positions, both in comparison to the period before entering distress and to other employers who are not having financial problems. However, the authors also find that workers are more willing to apply to positions at distressed firms in states where the costs of unemployment are lower because of more generous UB. The evidence I present in this chapter indicates that UB can not only help distressed employers to recruit personnel but also to retain them longer. For instance, one of the arguments provided by employers against the advance layoff notifications required by the 1988 Worker Adjustment and Retraining Notification Act (in the US) was that early departures of workers would hamper operations and could lead to shutting down before schedule or else sustain losses in keeping the plant open (Fallick, 1994). As shown in this chapter, UB would provide incentives for workers to stay longer with distressed employers, which may facilitate an orderly process of shutting down or downsizing.

Finally, the results of this chapter also provide a cautionary note about using establishment closures to study the effects of job loss on different outcomes. Workers present at the moment of closure are likely to be entitled to more generous UB than those who left earlier. Thus, researchers should be aware of the potential contamination of their estimates due to this source of selection bias, which has not been addressed in the literature before. Further research should address the effect of other institutional arrangements, such as notification periods, severance payments, seniority protections, among others, on the non-random selection of workers at establishment closures.

Table 2.1: Potential unemployment benefits duration (qualifying age in parentheses)

Months worked in last seven years	Potential duration of Unemployment Benefits (months)				
	Period 1: Until Dec 1984	Period 2: Jan 1985 - Dec 1985	Period 3: Jan 1986 - Jun 1987	Period 4: Jul 1987 - Mar 1999	Period 5: Apr 1999 - Jan 2006 1/
12	4	4	4	6	6
16	4	4	4	8	8
18	6	6	6	8	8
20	6	6	6	10	10
24	8	8	8	12	12
28	8	8	8	14 (>=42)	14 (>=45)
30	10	10	10	14 (>=42)	14 (>=45)
32	10	10	10	16 (>=42)	16 (>=45)
36	12	12	12	18 (>=42)	18 (>=45)
40	12	12	12	20 (>=44)	20 (>=47)
42	12	14 (>=49)	14 (>=44)	20 (>=44)	20 (>=47)
44	12	14 (>=49)	14 (>=44)	22 (>=44)	22 (>=47)
48	12	16 (>=49)	16 (>=44)	24 (>=49)	24 (>=52)
52	12	16 (>=49)	16 (>=44)	26 (>=49)	26 (>=52)
54	12	18 (>=49)	18 (>=49)	26 (>=49)	26 (>=52)
56	12	18 (>=49)	18 (>=49)	28 (>=54)	28 (>=57)
60	12	18 (>=49)	20 (>=49)	30 (>=54)	30 (>=57)
64	12	18 (>=49)	20 (>=49)	32 (>=54)	32 (>=57)
66	12	18 (>=49)	22 (>=54)	32 (>=54)	32 (>=57)
72	12	18 (>=49)	24 (>=54)	32 (>=54)	32 (>=57)

Source: Schmieder et al. (2010, 2012).

1/ The reform was phased in gradually, so that for most people it only took effect in April 1999.

Table 2.2: Sample means

	Policy Change #1		Policy Change #2	
	1982-1984	1987-1991	1992-1997	1999-2004
# Establishments	197	169	526	720
# Workers	6,669	6,822	14,456	15,123
Potential duration of UB	12.000	22.982	22.744	18.513
Age	46.494	47.849	47.552	46.517
Months employed in last seven years	82.349	82.474	82.465	82.367
Female	0.250	0.315	0.332	0.306
Daily wage (in 2005 euros)	77.150	86.830	86.928	87.261
Low wage earners	0.134	0.147	0.124	0.151
Tenure at establishment (years)				
$x < 5$ years	0.418	0.360	0.356	0.506
5 years $\leq x < 8$ years	0.344	0.129	0.170	0.139
$x \geq 8$ years (Policy Change #1)	0.238	0.511	—	—
8 years $\leq x < 10$ years	—	—	0.073	0.088
10 years $\leq x < 12$ years	—	—	0.062	0.064
12 years $\leq x < 15$ years	—	—	0.073	0.055
$x \geq 15$ years	—	—	0.266	0.149
Education				
Secondary/intermediate w/o vocational training	0.260	0.245	0.242	0.151
Secondary/intermediate w/ vocational training	0.600	0.619	0.634	0.625
Upper secondary school w/o vocational training	0.002	0.005	0.002	0.002
Upper secondary school w/ vocational training	0.004	0.005	0.012	0.024
Completion of a university of applied sciences	0.021	0.040	0.018	0.026
College / university degree	0.011	0.014	0.020	0.031
Missing	0.102	0.072	0.072	0.141
Occupation				
White-collar worker	0.294	0.409	0.322	0.373
Blue-collar worker	0.629	0.497	0.607	0.524
Part-time worker	0.077	0.095	0.071	0.103
Plant-size				
10-50 employees	0.662	0.543	0.608	0.656
51-100 employees	0.148	0.164	0.184	0.190
101-500 employees	0.170	0.135	0.168	0.119
501-1000 employees	0.020	0.028	0.023	0.011
1001+ employees	—	0.130	0.017	0.024

Industry				
Agriculture, energy, mining	0.027	0.146	0.005	0.046
Primary production	—	0.114	0.076	0.040
Structural metal products	0.112	0.042	0.095	0.076
Steel deformation, vehicle construction	0.282	0.049	0.179	0.061
Consumer goods	0.135	0.203	0.165	0.090
Food and luxury good industry	0.008	0.018	0.023	0.017
Main construction industry	0.134	0.058	0.068	0.090
Finishing trade	0.011	0.019	0.021	0.038
Wholesale trade	0.040	0.064	0.041	0.049
Retail industry	0.061	0.065	0.061	0.088
Transportation & communication	0.031	0.044	0.021	0.051
Economic services	0.020	0.050	0.034	0.096
Household services	0.009	0.006	0.032	0.020
Education, social & health care services	0.011	0.013	0.010	0.025
(Street) cleaning organizations	—	—	0.005	0.018
Public administration, social security	—	—	0.024	0.005
Missing	0.120	0.110	0.140	0.192

Source: Own calculations

Table 2.3: Policy Change #1: CPHM results

	All Separations		JTJ Transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Treatment Dose (D)	-0.485***	0.134	-1.054***	0.201	5.471***	0.008
Treatment Period (j)	0.169	0.197	0.637**	0.307	-0.358	0.259
$D \times j$ (**Coefficient** θ)	**-0.004**	**0.006**	**-0.021****	**0.010**	**0.012**	**0.009**
Work history in last seven years (months)	0.000	0.006	0.017**	0.009	-0.015**	0.006
Female	-0.244***	0.075	-0.271***	0.091	-0.200*	0.096
Daily wage (in 2005 euros)	-0.002*	0.001	0.001	0.002	-0.005***	0.001
Tenure at establishment						
5 years $\leq x < 8$ years	-0.178**	0.09	-0.267*	0.139	-0.095	0.096
$x \geq 8$ years	-0.143	0.101	-0.141	0.137	-0.166	0.11
Education						
Secondary/intermediate w/ vocational training	-0.175***	0.066	-0.072	0.094	-0.284***	0.094
Upper secondary school w/o vocational training	0.249	0.184	0.05	0.277	0.363	0.469
Upper secondary school w/ vocational training	-0.158	0.32	-0.271	0.610	0.030	0.292
Completion of a university of applied sciences	-0.232	0.158	-0.224	0.174	-0.175	0.289
College / university degree	-0.06	0.229	-0.010	0.256	-0.258	0.245
Missing	-0.137	0.119	-0.327	0.208	-0.048	0.162
Occupation						
Blue-collar worker	-0.164**	0.068	-0.257**	0.102	-0.038	0.101
Part-time worker	-0.270**	0.111	-0.405**	0.193	-0.196	0.156
Plant-size						
51-100 employees	-0.335***	0.108	-0.493***	0.157	-0.186	0.137
101-500 employees	-0.447***	0.147	-0.648***	0.228	-0.174	0.166
501-1000 employees	0.120	0.239	-0.778**	0.369	0.999**	0.500
1001+ employees	-0.963***	0.233	-0.205	0.291	-2.197***	0.315
Industry						
Primary production	0.284	0.231	-0.06	0.270	0.621*	0.366
Structural metal products	0.046	0.222	-0.131	0.313	0.183	0.302
Steel deformation, vehicle construction	0.636***	0.212	0.852***	0.267	0.213	0.336
Consumer goods	0.272	0.230	0.036	0.323	0.414	0.298
Food and luxury good industry	-0.290	0.236	-0.847*	0.447	0.081	0.267

Main construction industry	-0.095	0.229	0.118	0.368	-0.248	0.310
Finishing trade	0.001	0.244	-0.294	0.364	0.355	0.338
Wholesale trade	0.395	0.268	0.434	0.340	0.360	0.322
Retail industry	-0.087	0.218	-0.658*	0.350	0.246	0.309
Transportation & communication	-0.164	0.242	-0.069	0.327	-0.281	0.373
Economic services	0.600	0.479	0.855	0.634	0.355	0.327
Household services	-0.032	0.275	0.199	0.364	-0.229	0.379
Education, social & health care services	-0.251	0.342	-0.476	0.531	-0.074	0.401
Missing	-0.198	0.225	-0.218	0.277	-0.151	0.318
Age Dummies						
39	-0.066	0.11	0.016	0.133	-0.182	0.182
40	-0.015	0.109	-0.049	0.126	0.028	0.166
41	-0.246*	0.141	-0.392**	0.164	-0.047	0.157
42	2.758***	0.815	6.073***	1.199	-32.852***	0.127
43	2.84***	0.822	6.246***	1.255	-32.89***	0.131
44	4.801***	1.358	10.471***	2.026	-54.709***	0.115
45	4.696***	1.355	10.354***	2.013	-54.798***	0.137
46	4.806***	1.372	10.404***	2.035	-54.597***	0.125
47	4.797***	1.345	10.358***	2.014	-54.602***	0.114
48	4.706***	1.360	10.359***	2.011	-54.754***	0.103
49	6.658***	1.890	14.55***	2.813	-76.599***	0.130
50	6.647***	1.897	14.389***	2.816	-76.493***	0.112
51	6.679***	1.898	14.472***	2.826	-76.509***	0.125
52	6.615***	1.906	14.517***	2.835	-76.642***	0.12
53	6.723***	1.901	14.682***	2.82	-76.599***	0.123
54	9.62***	2.699	20.911***	3.981	-109.373	.
55	9.489***	2.684	20.504***	3.985	-109.284***	0.131
56	9.507***	2.688	20.761***	3.992	-109.456***	0.155
Year Dummies						
1982	0.259	0.163	0.202	0.319	0.302*	0.182
1983	0.151	0.161	0.353	0.281	-0.074	0.171
1984	0.668	0.413	0.231	0.604	0.836*	0.479
1987	-0.447*	0.244	-0.598*	0.358	-0.216	0.367
1988	0.096	0.211	-0.532**	0.238	0.591**	0.298
1989	0.049	0.241	0.052	0.338	0.089	0.256
# of Establishments	366		366		366	
# of Workers	13,491		13,491		13,491	

Notes: *** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance at 10% level.

Table 2.4: Policy Change #1: CPHM results by age groups

	All separations		JTJ Transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Age Group*Time Coefficient						
42-48 years	-0.126	0.082	-0.278**	0.123	0.191	0.142
49-56 years	-0.093	0.100	-0.286*	0.150	0.175	0.150
B. Linearized Estimates						
42-48 years	-0.011	0.009	-0.031**	0.014	0.021	0.016
49-56 years	-0.006	0.006	-0.018*	0.009	0.011	0.009

Notes:

*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size. The linearized estimates were calculated by dividing the estimates from panel A by the average treatment dose, which was 9.0 months for workers 42-48 years old and 16.1 months for workers 49-56 years old. The standard errors of the linearized estimates were calculated using the delta method.

Table 2.5: Policy Change #2: CPHM results

	All Separations		JTJ transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Treatment Dose (D)	-0.037	0.086	-0.400	0.361	0.071	0.137
Treatment Period (j)	0.169	0.136	0.078	0.195	0.237	0.183
$D \times j$ (COEFFICIENT θ)	**-0.002**	**0.007**	**-0.015**	**0.011**	**0.003**	**0.010**
Work history in last seven years (months)	-0.002	0.003	0.005	0.005	-0.006	0.004
Female	-0.07	0.046	-0.168**	0.069	-0.003	0.059
Daily wage (in 2005 euros)	-0.001	0.001	0.001	0.001	-0.002**	0.001
Tenure at establishment (years)						
5 years $\leq x <$ 8 years	-0.093*	0.048	-0.139*	0.073	-0.056	0.063
8 years $\leq x <$ 10 years	0.002	0.061	-0.047	0.086	0.046	0.085
10 years $\leq x <$ 12 years	-0.065	0.057	-0.085	0.09	-0.039	0.086
12 years $\leq x <$ 15 years	-0.144***	0.055	-0.202**	0.085	-0.091	0.08
$x \geq$ 15 years	-0.118**	0.055	-0.153*	0.084	-0.087	0.067
Education						
Secondary/intermediate w/ vocational training	-0.101**	0.043	-0.09	0.061	-0.101*	0.057
Upper secondary school w/o vocational training	-0.323**	0.141	0.005	0.226	-0.814***	0.263
Upper secondary school w/ vocational training	-0.138	0.088	-0.025	0.115	-0.27*	0.139
Completion of a university of applied sciences	0.028	0.084	0.07	0.118	-0.039	0.125
College / university degree	0.032	0.101	0.023	0.13	0.019	0.148
Missing	0.02	0.088	-0.135	0.104	0.117	0.125
Occupation						
Blue-collar worker	0.061	0.042	-0.068	0.061	0.157***	0.056
Part-time worker	-0.045	0.061	0.102	0.087	-0.123	0.086
Plant-size						
51-100 employees	0.081	0.062	0.289***	0.094	-0.075	0.078
101-500 employees	0.075	0.091	0.267**	0.121	-0.054	0.119
501-1000 employees	0.332	0.229	0.938***	0.295	-0.541	0.437
1001+ employees	1.244***	0.216	1.898***	0.213	0.139	0.224

Industry						
Primary production	0.413**	0.21	-0.028	0.248	1.355***	0.457
Structural metal products	0.379*	0.203	-0.041	0.207	1.318***	0.458
Steel deformation, vehicle construction	0.484***	0.22	0.202	0.324	1.344***	0.458
Consumer goods	0.229	0.21	-0.139	0.206	1.127***	0.467
Food and luxury good industry	0.166	0.232	-0.003	0.24	0.935*	0.479
Main construction industry	0.326	0.234	-0.249	0.198	1.332***	0.484
Finishing trade	0.238	0.204	-0.296	0.249	1.243***	0.457
Wholesale trade	0.32	0.209	0.208	0.221	0.997*	0.464
Retail industry	0.266	0.219	0.01	0.206	1.096*	0.474
Transportation & communication	0.268	0.211	0.208	0.218	0.932*	0.469
Economic services	0.396**	0.196	0.146	0.192	1.199***	0.453
Household services	0.069	0.229	-0.023	0.221	0.782	0.486
Education, social & health care services	0.335	0.212	0.065	0.228	1.196***	0.46
(Street) cleaning organizations	0.408*	0.247	0.098	<.306	1.302**	0.535
Public administration, social security	0.225	0.252	-0.888**	0.415	1.974***	0.423
Missing	0.193	0.192	-0.146	0.187	1.077**	0.451
Age Dummies						
39	0.004	0.059	-0.047	0.080	0.054	0.097
40	-0.033	0.058	-0.063	0.079	-0.002	0.093
41	-0.001	0.063	-0.088	0.084	0.08	0.093
42	-0.343	0.518	-2.688	2.169	0.429	0.816
43	-0.342	0.519	-2.57	2.169	0.325	0.817
44	-0.471	0.87	-4.316	.	0.75	1.365
45	-0.238	0.352	-1.859	1.45	0.323	0.548
46	-0.187	0.351	-1.915	1.449	0.447	0.549
47	-0.008	0.061	-0.238***	0.088	0.172*	0.093
48	-0.086	0.06	-0.279***	0.09	0.074	0.094
49	-0.317	0.354	-1.975	1.451	0.275	0.547
50	-0.241	0.351	-1.922	1.448	0.364	0.548
51	-0.251	0.351	-1.912	1.453	0.349	0.547
52	-0.093	0.06	-0.300***	0.088	0.077	0.092
53	-0.076	0.059	-0.345***	0.098	0.121	0.089
54	-0.377	0.509	-2.977	2.167	0.553	0.807
55	-0.348	0.515	-2.952	2.166	0.577	0.816
56	-0.351	0.52	-3.147	2.173	0.642	0.818

Year Dummies						
1992	0.122	0.129	0.058	0.205	0.171	0.184
1993	0.374**	0.159	0.039	0.251	0.554***	0.195
1994	0.354**	0.145	0.057	0.214	0.557***	0.186
1995	0.587***	0.192	0.602	0.366	0.582***	0.177
1996	0.401***	0.148	0.296	0.21	0.469**	0.199
1998	0.157	0.101	0.224*	0.13	0.092	0.155
1999	0.392***	0.144	0.475**	0.216	0.342*	0.186
2000	0.169*	0.088	0.304***	0.111	0.035	0.121
2001	0.208**	0.094	0.385***	0.118	0.074	0.132
# of Establishments	1,246		1,246		1,246	
# of Workers	29,579		29,579		29,579	

Notes: *** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance at 10% level.

Table 2.6: Policy Change #2: CPHM results using workers 38-44 years old

	All Separations		JTJ transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
Treatment Dose (D)	0.016**	0.007	0.038***	0.012	0.001	0.011
Treatment Period (j)	0.218	0.171	0.160	0.206	0.296	0.257
$D \times j$ (COEFFICIENT θ)	**-0.012**	**0.008**	**-0.026***	**0.014**	**-0.003**	**0.013**
Work history in last seven years (months)	-0.004	0.004	0.008	0.007	-0.012*	0.006
Female	-0.058	0.051	-0.164**	0.078	0.032	0.075
Daily wage (in 2005 euros)	-0.002**	0.001	0.000	0.001	-0.003***	0.001
Tenure at establishment (years)						
5 years $\leq x <$ 8 years	-0.085	0.058	-0.085	0.097	-0.076	0.084
8 years $\leq x <$ 10 years	0.030	0.072	0.057	0.102	0.013	0.115
10 years $\leq x <$ 12 years	-0.016	0.073	-0.029	0.117	0.025	0.113
12 years $\leq x <$ 15 years	-0.092	0.073	-0.181	0.12	-0.003	0.108
$x \geq$ 15 years	-0.028	0.073	-0.042	0.122	-0.002	0.09
Education						
Secondary/intermediate w/ vocational training	-0.196***	0.057	-0.234***	0.085	-0.155**	0.081
Upper secondary school w/o vocational training	-0.085	0.237	0.000	0.354	-0.262	0.286
Upper secondary school w/ vocational training	-0.089	0.111	-0.098	0.145	-0.105	0.193
Completion of a university of applied sciences	0.006	0.13	-0.150	0.149	0.146	0.245
College / university degree	-0.035	0.112	-0.188	0.155	0.110	0.188
Missing	-0.082	0.089	-0.245*	0.129	0.049	0.123
Occupation						
Blue-collar worker	-0.017	0.051	-0.225***	0.074	0.178**	0.074
Part-time worker	-0.136	0.084	0.032	0.119	-0.261**	0.129
Plant-size						
51-100 employees	0.085	0.066	0.255***	0.096	-0.073	0.086
101-500 employees	0.075	0.097	0.282**	0.124	-0.105	0.135
501-1000 employees	0.100	0.223	0.669**	0.282	-0.819**	0.415
1001+ employees	1.407	0.229	2.08***	0.242	-0.009	0.241

Industry						
Primary production	0.382*	0.211	-0.006	0.266	1.359***	0.550
Structural metal products	0.399*	0.208	0.095	0.230	1.327***	0.558
Steel deformation, vehicle construction	0.557**	0.230	0.374	0.344	1.402***	0.556
Consumer goods	0.275	0.220	-0.021	0.221	1.178*	0.574
Food and luxury good industry	0.176	0.234	0.131	0.235	0.878	0.596
Main construction industry	0.339	0.228	-0.148	0.228	1.371*	0.566
Finishing trade	0.175	0.222	-0.129	0.286	1.101***	0.561
Wholesale trade	0.269	0.214	0.182	0.229	0.987*	0.580
Retail industry	0.214	0.219	0.059	0.229	1.018*	0.562
Transportation & communication	0.296	0.211	0.374	0.245	0.841	0.561
Economic services	0.372*	0.200	0.126	0.210	1.26**	0.554
Household services	0.105	0.226	0.026	0.265	0.829	0.598
Education, social & health care services	0.537**	0.249	0.416	0.295	1.333**	0.565
(Street) cleaning organizations	0.420	0.300	-0.046	0.293	1.497**	0.685
Public administration, social security	0.124	0.263	-0.916**	0.396	2.137***	0.488
Missing	0.188	0.194	0.032	0.211	0.997**	0.549
Age Dummies						
39	0.009	0.058	-0.041	0.081	0.055	0.097
40	-0.040	0.057	-0.068	0.080	-0.012	0.093
41	0.000	0.063	-0.089	0.084	0.073	0.094
42	-0.072	0.047	-0.108	0.074	-0.033	0.075
43	-0.074	0.048	0.009	0.074	-0.142*	0.078
Year Dummies						
1992	0.128	0.162	0.179	0.209	0.101	0.255
1993	0.477**	0.192	0.180	0.252	0.696***	0.268
1994	0.368**	0.174	0.195	0.208	0.528**	0.255
1995	0.676***	0.212	0.724**	0.368	0.644***	0.250
1996	0.454**	0.181	0.52**	0.210	0.416	0.265
1998	0.208*	0.114	0.424***	0.150	-0.021	0.174
1999	0.318**	0.160	0.481**	0.231	0.178	0.216
2000	0.227**	0.099	0.358***	0.124	0.071	0.140
2001	0.215*	0.114	0.365***	0.134	0.078	0.166
# of Establishments	1,078		1,078		1,078	
# of Workers	11,341		11,341		11,341	

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level.

Table 2.7: Placebo Test 1: Common trends for treated and non-treated groups prior to policy changes

	All separations		JTJ Transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Policy Change #1						
$\theta_{Placebo}$	0.001	0.006	0.002	0.012	0.013	0.010
# of Establishments	197		197		197	
# of Workers	6,669		6,669		6,669	
B. Policy Change #2						
$\theta_{Placebo}$	0.014	0.009	0.016	0.018	0.014	0.013
# of Establishments	526		526		526	
# of Workers	14,456		14,456		14,456	
C. Policy Change #2 (workers 38-44 years old)						
$\theta_{Placebo}$	0.014	0.012	0.003	0.022	0.020	0.017
# of Establishments	453		453		453	
# of Workers	4,919		4,919		4,919	

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size.

Table 2.8: Placebo Test 2: Common trends for non-treated groups after policy changes (workers aged 32-40 years)

	All separations		JTJ Transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Policy Change #1						
$\theta_{Placebo}$	-0.002	0.113	-0.030	0.159	-0.014	0.169
# of Establishments	339		339		339	
# of Workers	5,323		5,323		5,323	
B. Policy Change #2						
$\theta_{Placebo}$	-0.025	0.059	-0.022	0.090	-0.021	0.087
# of Establishments	1,119		1,119		1,119	
# of Workers	13,595		13,595		13,595	

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size. The table compares the trends in the risk scores for workers aged 32-36 versus those aged 37-40 years old.

Table 2.9: Estimates of θ for up to three years before closure

	All separations		JTJ Transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Policy Change #1						
$\theta_{\text{last year}}$	-0.004	0.006	-0.021**	0.010	0.012	0.009
$\theta_{\text{2 years before closure}}$	-0.006	0.011	-0.025*	0.013	0.020	0.020
B. Policy Change #2						
$\theta_{\text{last year}}$	-0.002	0.007	-0.015	0.011	0.003	0.010
$\theta_{\text{2 years before closure}}$	-0.006	0.009	-0.021	0.015	0.001	0.012
$\theta_{\text{3 years before closure}}$	0.012	0.013	-0.005	0.022	0.020	0.018
C. Policy Change #2 (only workers 38-44 years old)						
$\theta_{\text{last year}}$	-0.012	0.008	-0.026*	0.014	-0.003	0.013
$\theta_{\text{2 years before closure}}$	-0.001	0.012	-0.031	0.019	0.023	0.017
$\theta_{\text{3 years before closure}}$	0.018	0.017	-0.007	0.026	0.034	0.024

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size.

Table 2.10: Time-varying effects

	All separations		JTJ Transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Policy Change #1						
First quarter*	-0.025	0.020	-0.021	0.026	-0.002	0.024
Second quarter	0.006	0.016	-0.041*	0.024	0.032	0.022
Third quarter	-0.007	0.015	-0.011	0.018	-0.015	0.024
Fourth quarter	-0.001	0.007	-0.021*	0.013	0.019*	0.011
	(0.587)		(0.785)		(0.506)	
B. Policy Change #2						
First quarter*	0.037	0.023	-0.011	0.037	0.063**	0.032
Second quarter	-0.033	0.021	-0.055**	0.027	-0.02	0.027
Third quarter	-0.014	0.015	-0.022	0.021	-0.018	0.022
Fourth quarter	0.000	0.007	-0.003	0.014	0.001	0.012
	(0.120)		(0.376)		(0.158)	
C. Policy Change #2 (workers 38-44 years old)						
First quarter*	0.003	0.030	-0.057	0.045	0.047	0.043
Second quarter	-0.051**	0.024	-0.077**	0.035	-0.038	0.034
Third quarter	-0.002	0.019	0.009	0.028	-0.016	0.026
Fourth quarter	-0.009	0.10	-0.020	0.017	0.002	0.016
	(0.378)		(0.182)		(0.431)	

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size. The p-values from F-tests of the null hypothesis of joint equality of coefficients to the base case (marked with an asterisk) are presented in parentheses.

Table 2.11: Policy Change #1: Estimates of θ by subgroups

	All Separations		JTJ transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Income						
Non-low wage earners*	-0.005	0.007	-0.021**	0.01	0.015	0.010
Low wage earners	-0.003	0.014	-0.016	0.029	0.003	0.018
	(0.938)		(0.868)		(0.586)	
B. Gender						
Male*	-0.005	0.008	-0.022**	0.011	0.017	0.012
Female	-0.004	0.010	-0.011	0.020	0.003	0.013
	(0.942)		(0.592)		(0.411)	
C. Occupation						
White-collar worker*	-0.011	0.013	-0.027*	0.014	0.018	0.022
Blue-collar worker	0.000	0.008	-0.016	0.014	0.013	0.010
Part-time Worker	0.013	0.016	0.016	0.036	0.007	0.023
	(0.511)		(0.597)		(0.811)	
D. Tenure						
$x < 5$ years*	-0.011	0.010	-0.035**	0.014	0.007	0.013
5 years $\leq x < 8$ years	-0.003	0.010	0.003	0.017	0.002	0.016
$x \geq 8$ years	0.001	0.010	-0.019	0.016	0.023*	0.014
	(0.706)		(0.132)		(0.625)	
E. Education						
Secondary/intermediate w/o vocational training*	-0.001	0.011	-0.016	0.020	0.004	0.016
Secondary/intermediate w/ vocational training	-0.009	0.008	-0.025**	0.012	0.013	0.011
Upper secondary school w/o vocational training	-0.027	0.056	0.085	0.121	-0.042	0.102
Upper secondary school w/ vocational training	0.040	0.045	-0.005	0.060	0.045	0.065
Completion of a university of applied sciences	-0.013	0.055	-0.007	0.049	0.078	0.084
College / university degree	0.143***	0.027	0.143***	0.046	0.143***	0.061
Missing	-0.003	0.019	-0.012	0.037	0.010	0.022
	(0.000)		(0.001)		(0.773)	

F. Establishment Size						
11-50 workers*	-0.013	0.010	-0.037***	0.014	0.007	0.012
51-100 workers	0.013*	0.007	0.021*	0.013	0.010	0.010
101-500 workers	-0.009	0.008	-0.019	0.016	0.003	0.010
501-1000 workers	0.038***	0.006	0.034***	0.007	0.050***	0.013
	(0.000)		(0.000)		(0.000)	

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size. The p-values from F-tests of the null hypothesis of joint equality of coefficients to the base case (marked with an asterisk) are presented in parentheses.

Table 2.12: Policy Change #2: Estimates of θ by subgroups

	All Separations		JTJ transitions		JTN Transitions	
	Estimate	S.E.	Estimate	S.E.	Estimate	S.E.
A. Income						
Non-low wage earners*	-0.001	0.007	-0.010	0.012	0.003	0.011
Low wage earners	-0.007	0.018	-0.049	0.034	0.005	0.023
	(0.769)		(0.281)		(0.913)	
B. Gender						
Male*	-0.001	0.008	-0.012	0.012	0.004	0.011
Female	-0.006	0.012	-0.022	0.025	-0.001	0.017
	(0.734)		(0.714)		(0.758)	
C. Occupation						
White-collar worker*	0.002	0.012	-0.015	0.019	0.014	0.017
Blue-collar worker	-0.004	0.008	-0.015	0.015	0.000	0.013
Part-time Worker	-0.011	0.021	-0.038	0.037	-0.009	0.031
	(0.674)		(0.986)		(0.531)	
D. Tenure						
$x < 5$ years*	-0.007	0.01	-0.009	0.017	-0.008	0.015
5 years $\leq x < 8$ years	-0.019	0.017	-0.017	0.028	-0.022	0.025
8 years $\leq x < 10$ years	-0.003	0.022	-0.039	0.037	0.018	0.032
10 years $\leq x < 12$ years	0.006	0.022	0.037	0.039	-0.013	0.037
12 years $\leq x < 15$ years	-0.036*	0.02	-0.083**	0.036	-0.012	0.033
$x \geq 15$ years	0.022	0.014	-0.018	0.024	0.046**	0.021
	(0.456)		(0.632)		(0.486)	

E. Education						
Secondary/intermediate w/o vocational training*	-0.013	0.016	-0.023	0.026	-0.019	0.022
Secondary/intermediate w/ vocational training	-0.002	0.008	-0.005	0.014	-0.003	0.012
Upper secondary school w/o vocational training	-0.072	0.056	-0.128	0.083	0.005	0.158
Upper secondary school w/ vocational training	-0.003	0.052	-0.148	0.128	0.118	0.105
Completion of a university of applied sciences	-0.004	0.063	0.071	0.053	-0.063	0.111
College / university degree	-0.035	0.03	-0.089*	0.048	0.015	0.054
Missing	0.010	0.021	-0.025	0.036	0.030	0.029
	(0.968)		(0.099)		(0.936)	
F. Establishment Size						
11-50 workers*	-0.005	0.01	-0.017	0.018	0.001	0.014
51-100 employees	-0.001	0.007	-0.012	0.010	0.005	0.009
101-500 employees	0.006	0.006	-0.005	0.009	0.014	0.011
501-1000 employees	0.005	0.012	-0.021	0.016	0.067	0.041
1001+ employees	0.000	0.007	-0.017	0.022	0.048	0.037
	(0.011)		(0.419)		(0.445)	

Notes: *** denotes statistical significance at 1% level ; ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level. Controls include gender, education, daily wage (in 2005 euros), occupation, industry, work experience in last seven years, tenure, and establishment size. The p-values from F-tests of the null hypothesis of joint equality of coefficients to the base case (marked with an asterisk) are presented in parentheses.

Figure 2.1: Maximum PDB by age and period (in months)

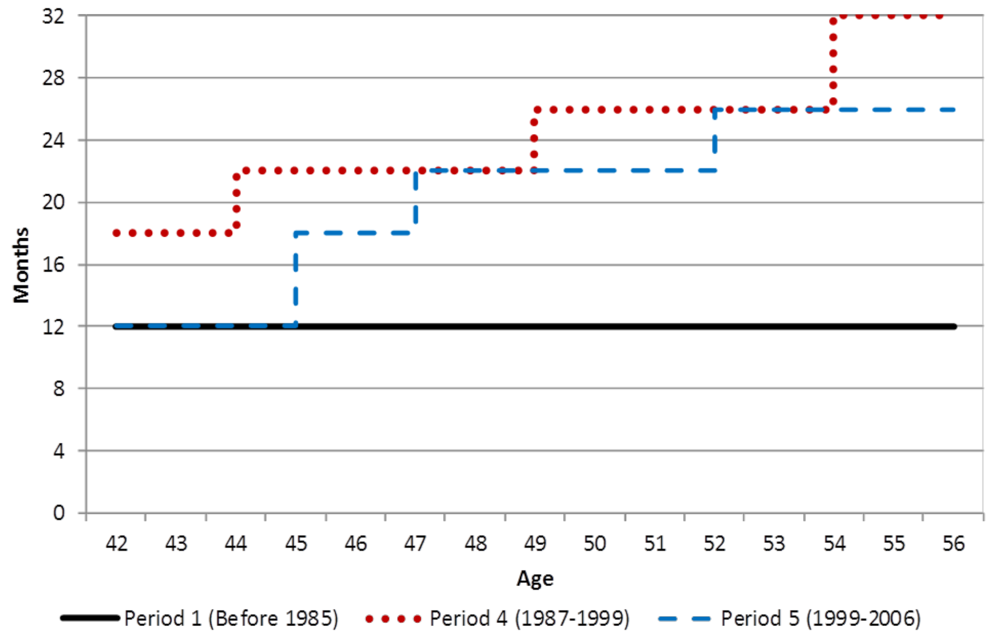


Figure 2.2: Changes in the maximum PDB by age (in months)

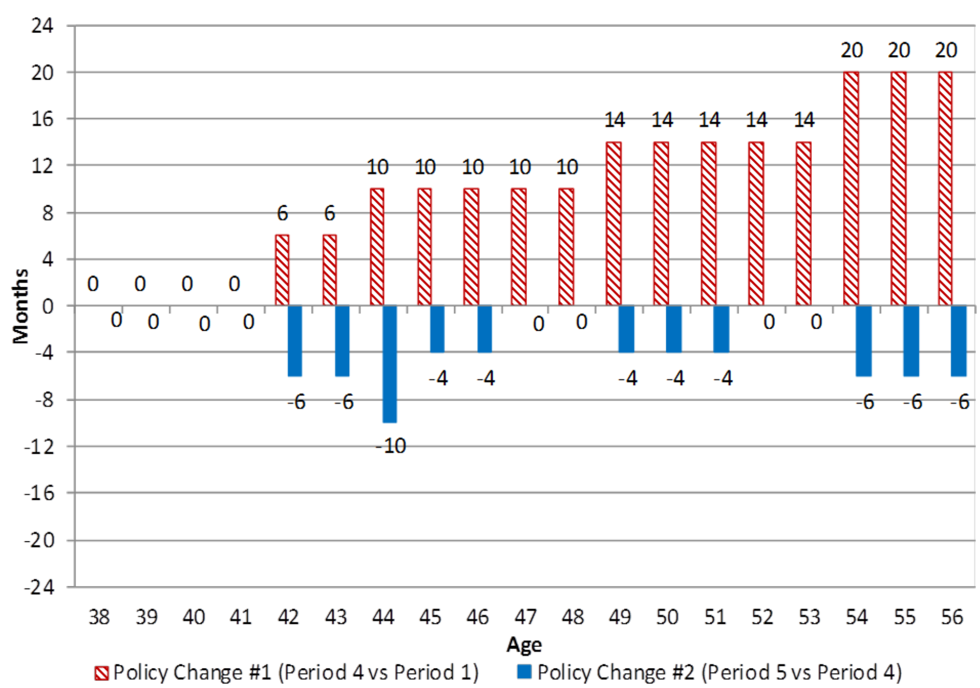
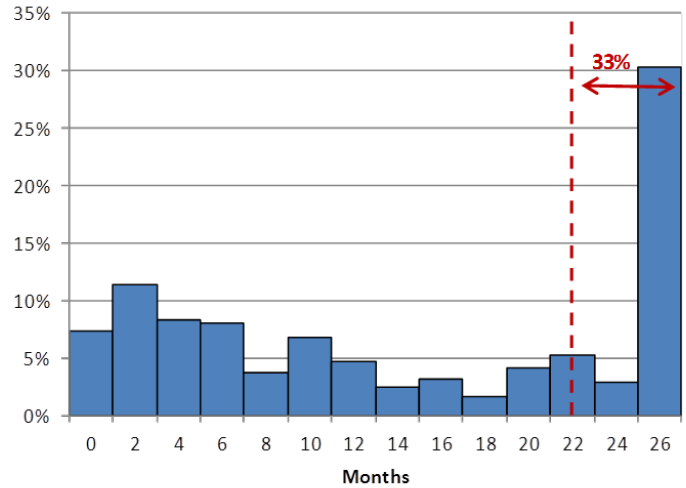
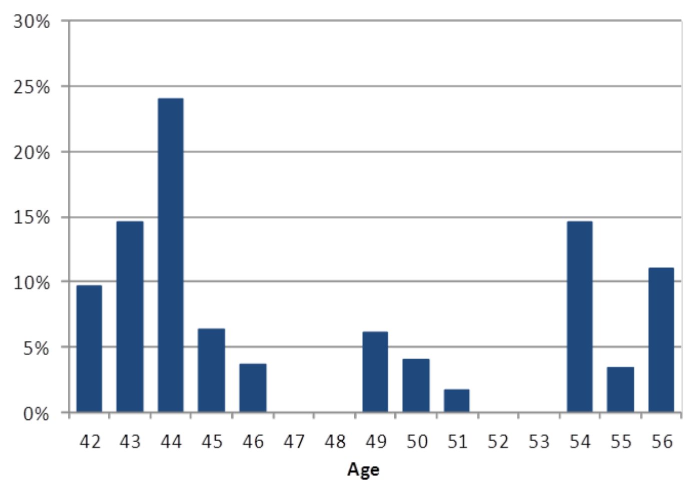


Figure 2.3: Histogram of UB durations for 50 year old workers (1992-1997)



Notes: 33% of the UB spells of 50-years-old workers that separated from closing establishments by entering into unemployment (in 1992-1997) had a duration larger than 22 months.

Figure 2.4: Percentage of UB spells in 1992-1997 potentially affected by Policy Change #2 (excluding spells that exhausted benefits)



Note: The figure shows the percentage of UB spells above the maximum PDB in 1999-2006 (Policy Change #2). The calculations include UB spells of workers that separated from closing establishments by entering unemployment during 1992-1997. Spells that exhausted UB benefits are excluded.

Figure 2.5: Evolution of establishment size (three years before closure = 100)

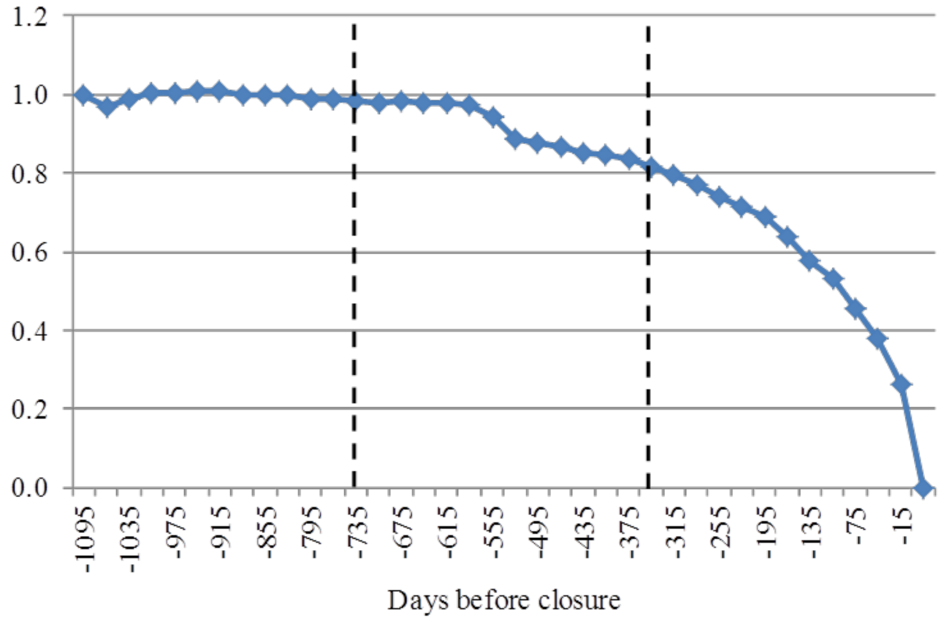
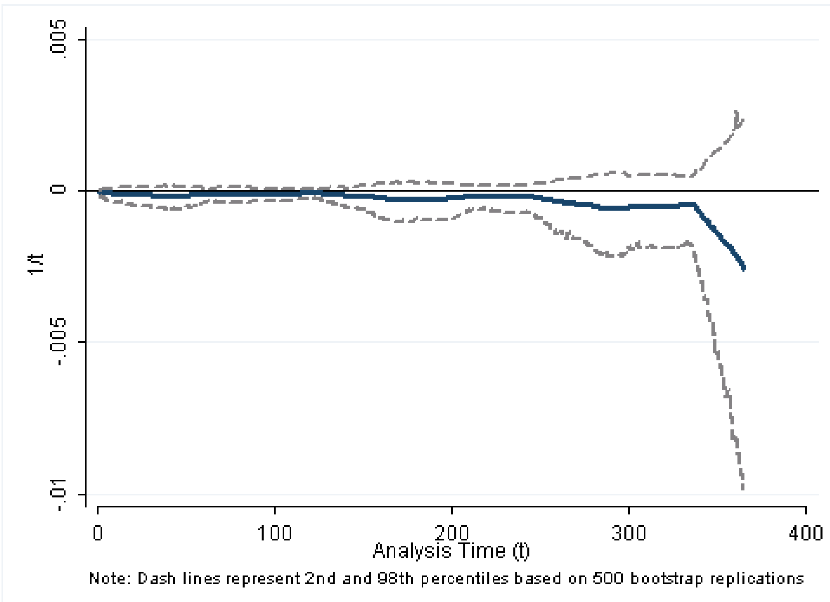


Figure 2.6: Policy Change #1: Treatment effects on the hazard rates and failure function for all separations (for the average treated worker)

Treatment effects on the hazard rates



Treatment effects on the failure function

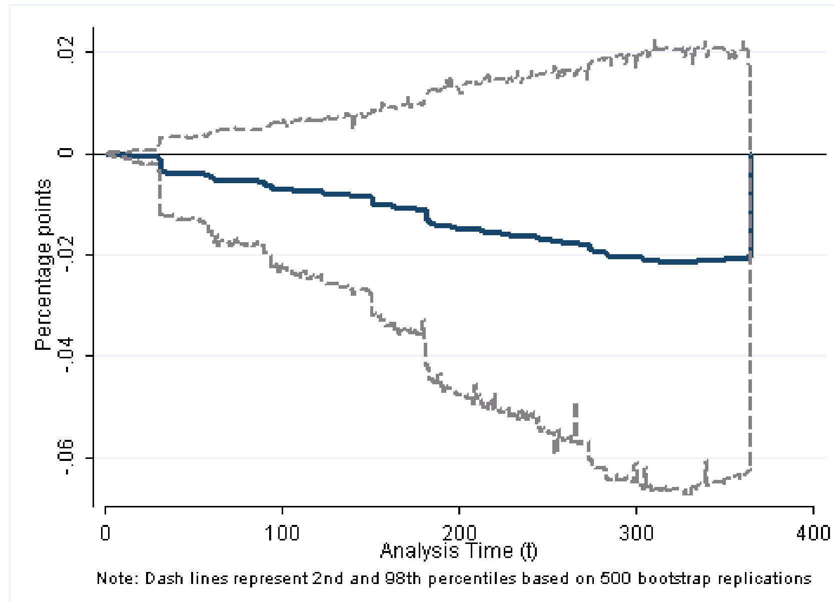
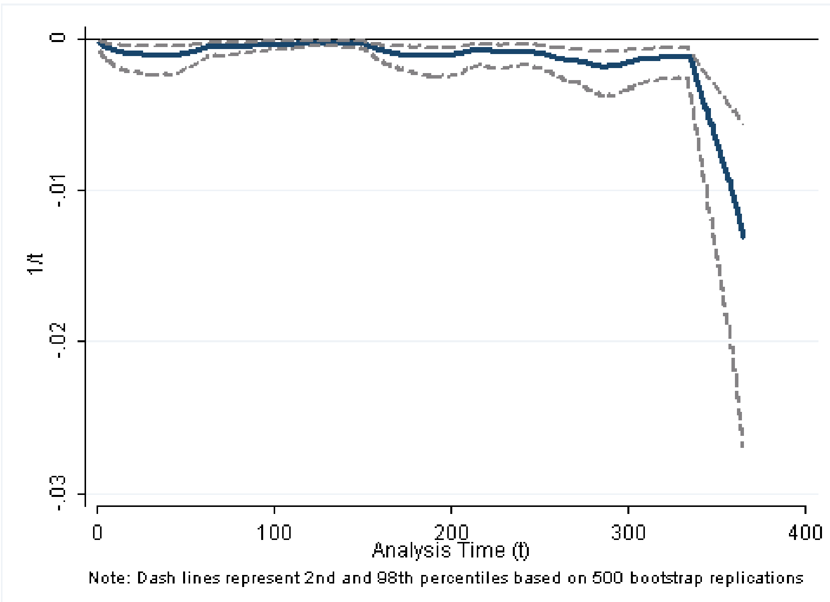


Figure 2.7: Policy Change #1: Treatment effects on the hazard rates and CIF for JTJ transitions (for the average treated worker)

Treatment effects on the hazard rates



Treatment effects on the CIF

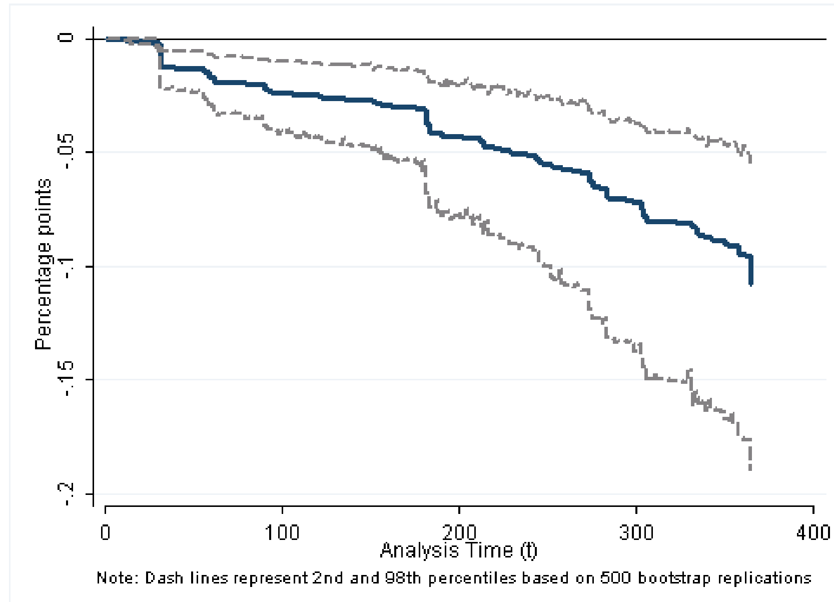


Figure 2.8: Policy Change #1: Treatment effects on the hazard rates and CIF for JTN transitions (for the average treated worker)

Treatment effects on the hazard rates

Treatment effects on the CIF

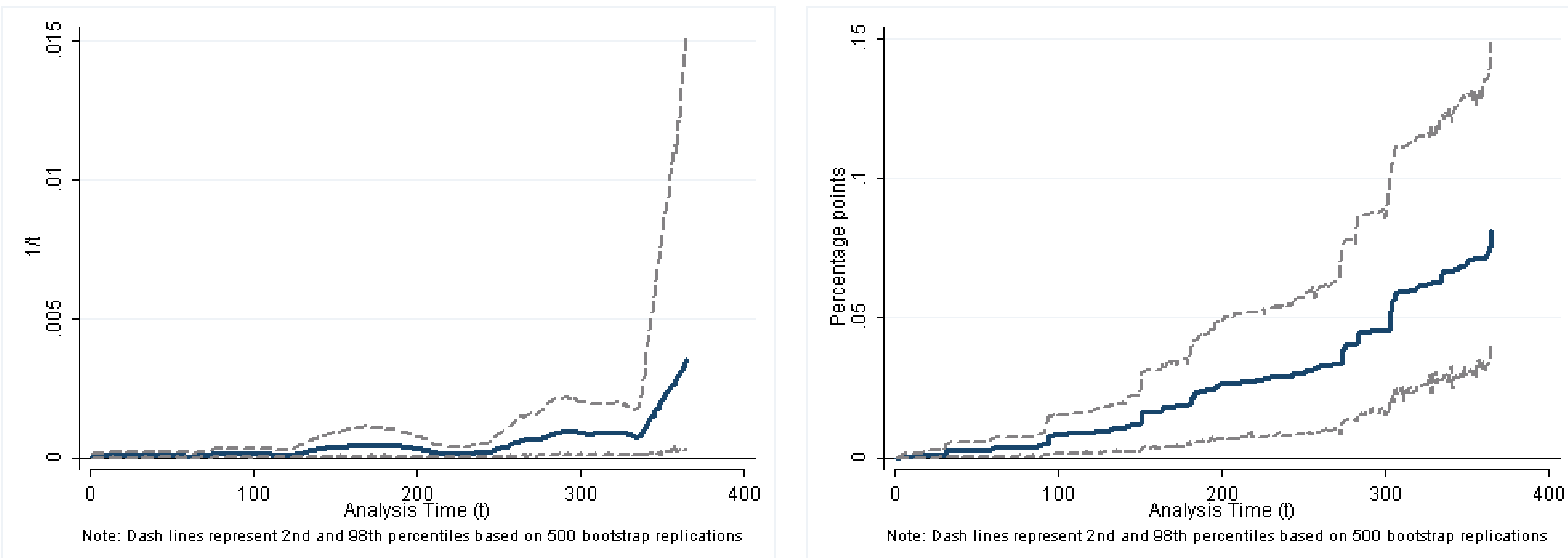
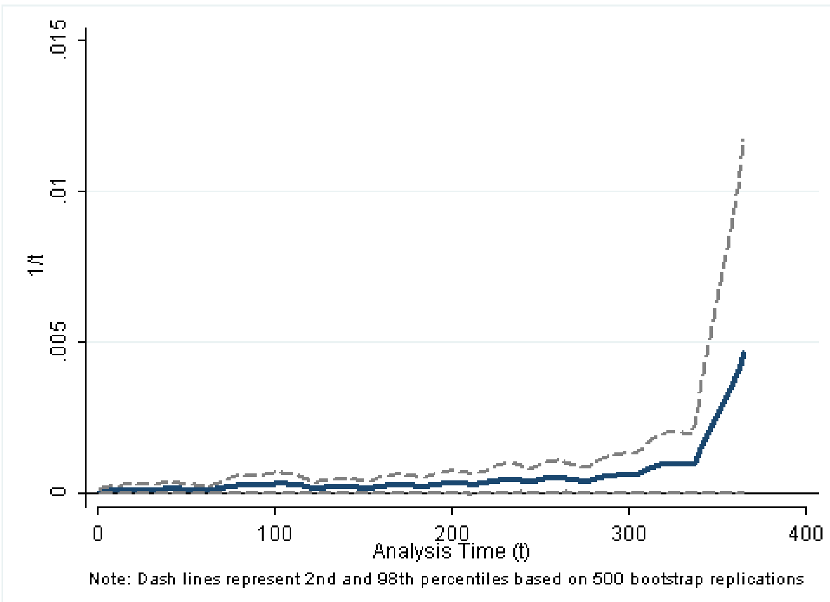


Figure 2.9: Policy Change #2: Treatment effects on the hazard rates and failure function for all separations (for the average treated worker of age 42-44)

Treatment effects on the hazard rates



Treatment effects on the failure function

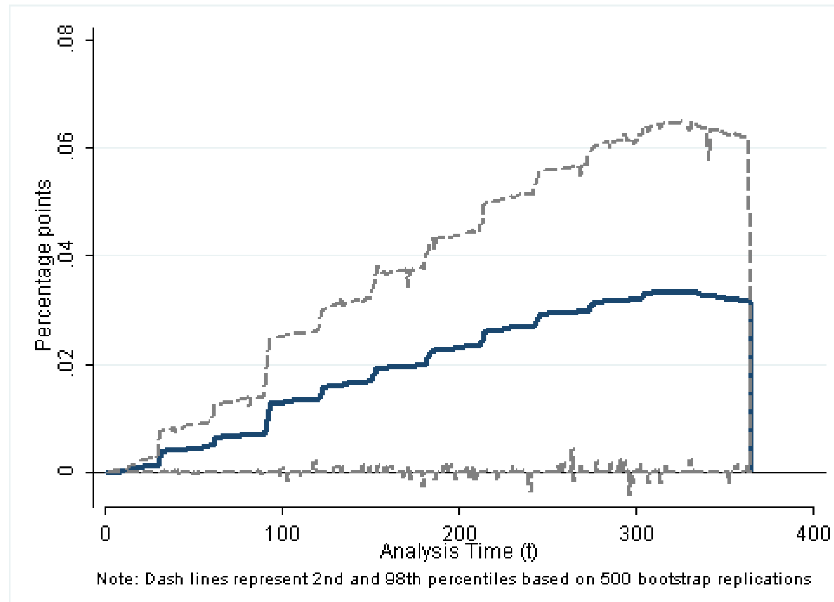
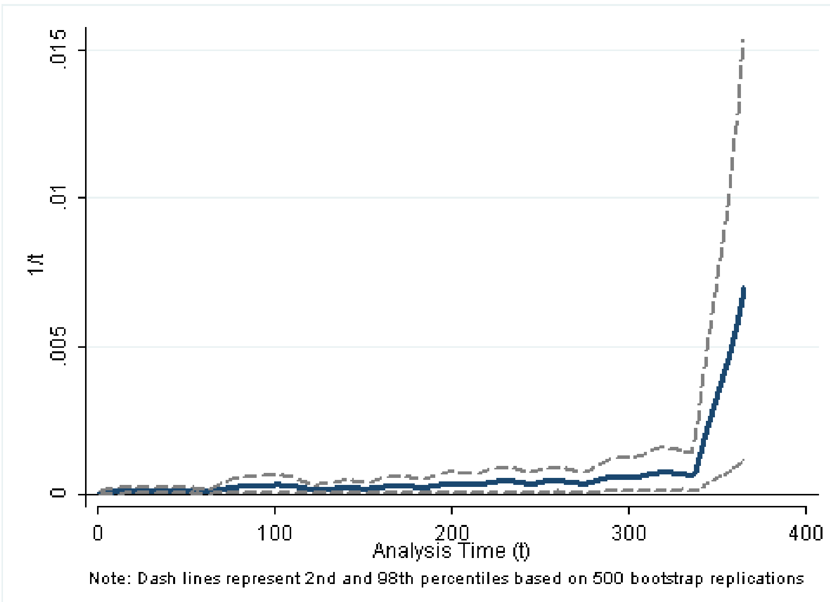


Figure 2.10: Policy Change #2: Treatment effects on the hazard rates and CIF for JTJ transitions (for the average treated worker of age 42-44)

Treatment effects on the hazard rates



Treatment effects on the CIF

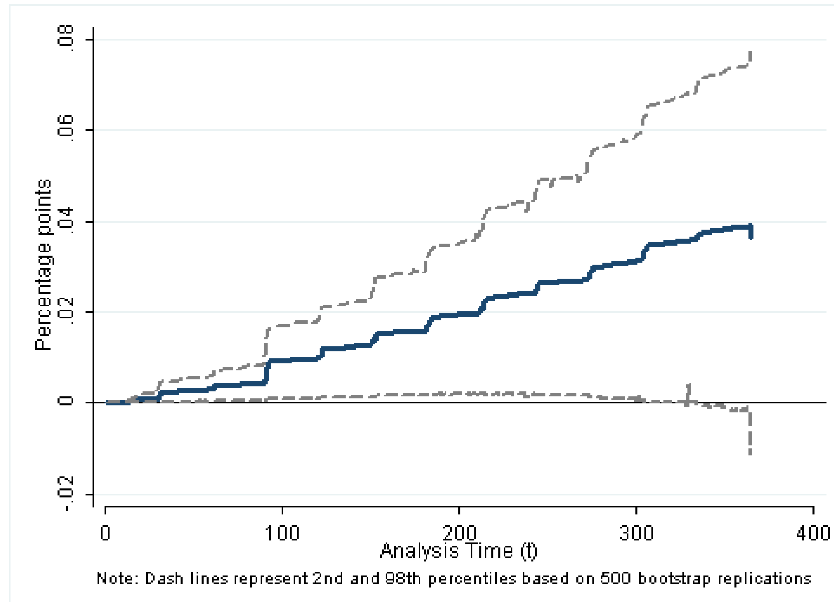
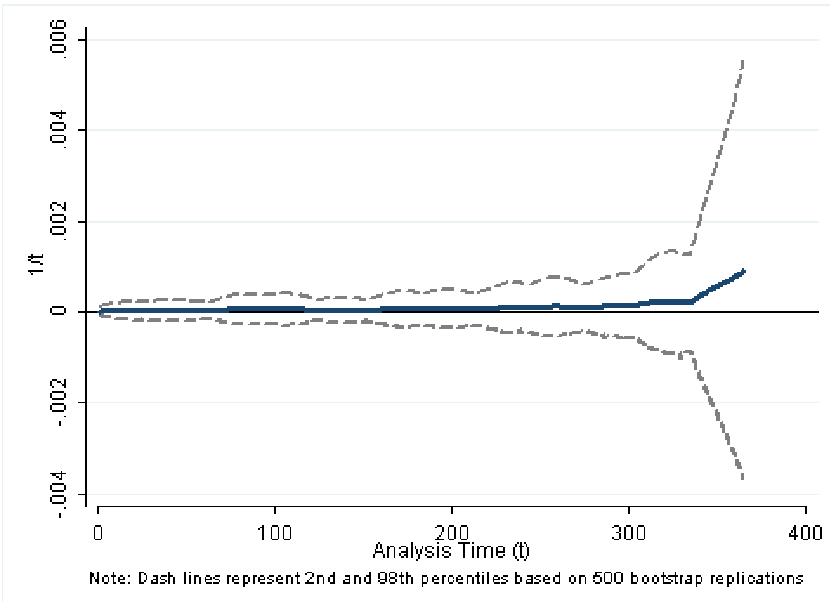
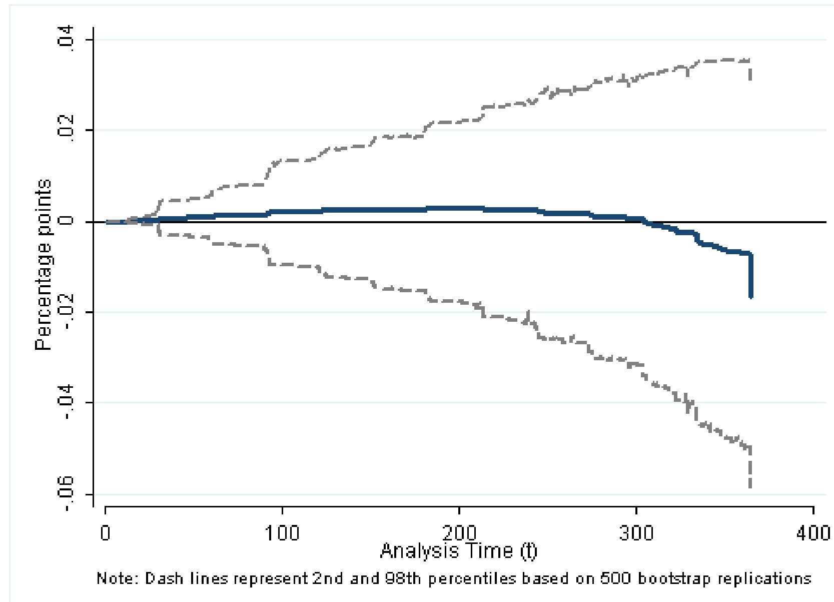


Figure 2.11: Policy Change #2: Treatment effects on the hazard rates and CIF for JTN transitions (for the average treated worker of age 42-44)

Treatment effects on the hazard rates



Treatment effects on the CIF



2.9 Appendixes

2.9.1 Proofs of Propositions

2.9.1.1 Proof of Proposition II.1

I use proof by induction. The continuation value of unemployment when $t = 2$ is given by equation (2.1):

$$V(2) = \underset{s}{Max}\left\{b+z+\beta\left\{sE\left[Max(W(x),V(1)\right]+(1-s)V(1)\right\}-0.5s^2\right\} \tag{2.1}$$

It follows from equations (2.1) and (2.2) that $V(1) = b + V(0) > V(0)$. With a little manipulation the following inequalities are obtained:

$$V(2) > \underset{s}{Max}\left\{b+z+\beta\left\{sE\left[Max(W(x),V(0)\right]+(1-s)V(0)\right\}-0.5s^2\right\} \tag{2.2}$$

$$V(2) > V(1) \tag{2.3}$$

Now, if I assume that $V(t-1) > V(t-2)$, then:

$$V(t) = \underset{s}{Max}\left\{b+z+\beta\left\{\alpha E\left[Max(W(x),V(t-1)\right]+(1-\alpha)V(t-1)\right\}-0.5s^2\right\} \tag{2.4}$$

$$V(t) > \underset{s}{Max}\left\{b+z+\beta\left\{\alpha E\left[Max(W(x),V(t-2)\right]+(1-\alpha)V(t-2)\right\}-0.5s^2\right\} \tag{2.5}$$

$$V(t) > V(t-1) \tag{2.6}$$

which concludes the proof that the continuation value of unemployment increases with t or the length of the remaining length of entitlement to UB.

Regarding the reservation wages, it follows from inspection that $x_1^U = x_0^U$ because the expected value of declining a job offer at the end of the last period of entitlement is the same as the expected value of declining an offer at any period after that. Now, since $V(t) = W(x_t^U) = \frac{x_t^U}{1-\beta}$ and I have shown that $V(t) > V(t-1)$, then it follows that $x_t^U > x_{t-1}^U$ for $t > 1$.

2.9.1.2 Proof of Proposition II.2

Again, I use proof by induction. Notice that:

$$
\begin{aligned}
B^L(w,1,T) =& \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),V(T)\right]\right.\right. \\
& \left.\left.+(1-s)V(T)\right\} - 0.5s^2\right\} && (2.7) \\
B^L(w,1,T) >& \underset{s}{Max}\left\{b+z+\beta\left\{sE\left[Max(W(x),V(T-1)\right]\right.\right. \\
& \left.\left.+(1-s)V(T-1)\right\} - 0.5s^2\right\} && (2.8) \\
B^L(w,1,T) >& V(T) && (2.9)
\end{aligned}
$$

Moving from the first to the second line is supported by the fact that $V(T) > V(T-1)$ (by Proposition II.1) and that $w > b+z$ (by assumption). Similarly I show below that $B^L(w,2,T) > B^L(w,1,T)$:

$$
\begin{aligned}
B^L(w,2,T) =& \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),B^L(w,1,T))\right]\right.\right. \\
& \left.\left.+(1-s)B^L(w,1,T)\right\} - 0.5s^2\right\} && (2.10) \\
B^L(w,2,T) >& \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),V(T)\right]\right.\right. \\
& \left.\left.+(1-s)V(T)\right\} - 0.5s^2\right\} && (2.11) \\
B^L(w,2,T) >& B^L(w,1,T) && (2.12)
\end{aligned}
$$

Now if I assume that $B^L(w,n-1,T) > B^L(w,n-2,T)$ then the following relationships hold, which complete the proof that the value of employment decreases as the workers approaches the separation date:

$$
\begin{aligned}
B^L(w,n,T) =& \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),B^L(w,n-1,T))\right]\right.\right.\\
&\left.\left.+(1-s)B^L(w,n-1,T)\right\}-0.5s^2\right\} \qquad (2.13)\\
B^L(w,n,T) >& \underset{s}{Max}\left\{w+\beta\left\{sE\left[Max(W(x),B^L(w,n-2,T)\right]\right.\right.\\
&\left.\left.+(1-s)B^L(w,n-2,T)\right\}-0.5s^2\right\} \qquad (2.14)\\
B^L(w,n,T) >& B^L(w,n-1,T) \qquad (2.15)
\end{aligned}
$$

Regarding the reservation wage, recall that $W(x^L(w,n,T)) = \frac{x^L(w,n,T)}{1-\beta} = B^L(w,n,T)$. Then, $x^L(w,n,T) = (1-\beta)*B^L(w,n,T)$. So given that $B^L(w,n,T)$ decreases as the separation date approaches (n gets smaller) so does the reservation wage.

2.9.1.3 Proof of Proposition II.4

Let's start by analyzing how the value of employment $B^L(w,n,T)$ changes with changes in the PDB (denoted by T). The optimal value for $B^L(w,n,T)$, assuming an interior solution, is given by equation (2.16):

$$
\begin{aligned}
B^L(w,n,T) =& w+\beta\left\{S^L(w,n,T)*E\left[Max(W(x),B^L(w,n-1,T)\right]\right.\\
&\left.+\left(1-S^L(w,n,T)\right)B^L(w,n-1,T)\right\}-0.5\left(S^L(w,n,T)\right)^2 \qquad (2.16)
\end{aligned}
$$

I use the envelope theorem to show that:

$$\frac{\partial B^L(w,n,T)}{\partial T} = \beta \left\{ \frac{\partial B^L(w,n-1,T)}{\partial T} - S^L(w,n,T) \right.$$
$$\left. * \int_{(1-\beta)B^L(w,n-1,T)}^{\infty} \frac{\partial B^L(w,n-1,T)}{\partial T} dF(x)dx \right\} \tag{2.17}$$
$$\frac{\partial B^L(w,n,T)}{\partial T} = \frac{\partial B^L(w,n-1,T)}{\partial T} \beta \left\{ 1 - S^L(w,1n,T) \right.$$
$$\left. * \left[1 - F\left((1-\beta)B^L(w,n-1,T)\right)\right] \right\} \tag{2.18}$$

And the corresponding expression when $n = 1$ is given by:

$$\frac{\partial B^L(w,1,T)}{\partial T} = \frac{\partial V(T)}{\partial T} \beta \left\{ 1 - S^L(w,1,T) * [1 - F((1-\beta)V(T))] \right\} \tag{2.19}$$

From Proposition II.1, I know that $\frac{\partial V(T)}{\partial T} > 0$. Then, I use the results from equations (2.18) and (2.19), to establish the following inequalities:

$$0 < \frac{\partial B^L(w,N,T)}{\partial T} < \frac{\partial B^L(w,N-1,T)}{\partial T} .... < \frac{\partial B^L(w,1,T)}{\partial T} < \frac{\partial V(T)}{\partial T} \tag{2.20}$$

By definition, the reservation wage is given by $x^L(w,n,T) = (1-\beta) * B^L(w,n,T)$. Using the result in equation (2.20), the inequalities below follow:

$$0 < \frac{\partial x^L(w,N,T)}{\partial T} < \frac{\partial x^L(w,N-1,T)}{\partial T} .... < \frac{\partial x^L(w,1,T)}{\partial T} \tag{2.21}$$

This completes the proof that and increase in the PDB increases the value of employment and the reservation wages and that the effect is stronger the closer the worker is to the separation date. Now, I analyze the effect of changes in the PDB on the the optimal search effort. In the last period of employment the optimal search effort and its derivative with respect to T are given by:

$$S^L(w,1,T) = \beta\left\{\int_{(1-\beta)V(T)}^{\infty}[W(x)-V(T)]\,dF(x)dx\right\} \tag{2.22}$$
$$\frac{\partial S^L(w,1,T)}{\partial T} = -\beta\int_{(1-\beta)V(T)}^{\infty}\frac{\partial V(T)}{\partial T} \tag{2.23}$$

I know that $\frac{\partial V(T)}{\partial T} > 0$ from Proposition II.1. Then, $\frac{\partial S^L(w,1,T)}{\partial T} < 0$. A similar analysis can be done for $S^L(w,2,T)$:

$$S^L(w,2,T) = \beta\left\{\int_{(1-\beta)B^L(w,1,T)}^{\infty}\left[W(x)-B^L(w,1,T)\right]dF(x)dx\right\} \tag{2.24}$$
$$\frac{\partial S^L(w,2,T)}{\partial T} = -\beta\int_{(1-\beta)B^L(w,1,T)}^{\infty}\frac{\partial B^L(w,1,T)}{\partial T} \tag{2.25}$$

Given that $B^L(w,1,T) > V(T)$ per equation (2.9) and that $0 < \frac{\partial B^L(w,1,T)}{\partial T} < \frac{\partial V(T)}{\partial T}$ per equation (2.18), I obtain that $\frac{\partial S^L(w,1,T)}{\partial T} < \frac{\partial S^L(w,2,T)}{\partial T} < 0$. In general, for periods n and $n-1$ for $n \geqslant 2$ the comparative statics are given by:

$$\frac{\partial S^L(w,n,T)}{\partial T} = -\beta\int_{(1-\beta)B^L(w,n-1,T)}^{\infty}\frac{\partial B^L(w,n-1,T)}{\partial T} \tag{2.26}$$
$$\frac{\partial S^L(w,n-1,T)}{\partial T} = -\beta\int_{(1-\beta)B^L(w,n-2,T)}^{\infty}\frac{\partial B^L(w,n-2,T)}{\partial T} \tag{2.27}$$

Since $B^L(w,n-1,T) > B^L(w,n-2,T)$ per Proposition (II.2) and $\frac{\partial B^L(w,n-1,T)}{\partial T} < \frac{\partial B^L(w,n-2,T)}{\partial T}$ per equation (2.20), and using the previous result, I can establish that:

$$\frac{\partial S^L(w,1,T)}{\partial T} < \frac{\partial S^L(w,2,T)}{\partial T} .... < \frac{\partial S^L(w,N-1,T)}{\partial T} < \frac{\partial S^L(w,N,T)}{\partial T} < 0 \tag{2.28}$$

which concludes the proof that an increase in the PDB reduces search effort and the effect is larger the closer is the worker to the separation date.

2.9.1.4 Proof of Proposition II.6

Let $S^E(w,\phi,N,T)$ denote optimal search effort decision for a non-notified worker. Then, using equation (2.6), the value of employment is given by:

$$
\begin{aligned}
E(w,\phi,N,T) =& w+\beta\Big\{\phi S^E(w,\phi,N,T)*E\left[Max\left(W(x),B^L(w,N,T)\right)\right]\\
&+\phi\left(1-S^E(w,\phi,N,T)\right)B^L(w,N,T)\\
&+(1-\phi)S^E(w,\phi,N,T)*E\left[Max(W(x),E(w,\phi,N,T))\right]\\
&+(1-\phi)\left(1-S^E(w,\phi,N,T)\right)E(w,\phi,N,T)\Big\}\\
&-0.5(S^E(w,\phi,N,T))^2
\end{aligned}
\tag{2.29}
$$

Invoking the envelop theorem and after some manipulation I obtain:

$$
\begin{aligned}
\frac{\partial E(w,\phi,N,T)}{\partial T} =& \beta\Big\{\phi\frac{B^L(w,N,T)}{\partial T}+\phi S^E(w,\phi,N,T)\int_{(1-\beta)B^L(w,N,T)}^{\infty}\\
&-\frac{\partial B^L(w,N,T)}{\partial T}dF(x)dx\\
&+(1-\phi)\frac{\partial E(w,\phi,N,T)}{\partial T}\\
&+(1-\phi)S^E(w,\phi,N,T)\times\\
&\times\int_{(1-\beta)E(w,\phi,N,T)}^{\infty}-\frac{\partial E(w,\phi,N,T)}{\partial T}dF(x)dx\Big\}
\end{aligned}
\tag{2.30}
$$

$$
\begin{aligned}
\frac{\partial E(w,\phi,N,T)}{\partial T} =& \beta\Big\{\phi\frac{B^L(w,N,T)}{\partial T}\big[1-S^E(w,\phi,N,T)\\
&\times\left(1-F\left((1-\beta)B^L(w,N,T)\right)\right)\big]\\
&+(1-\phi)\frac{\partial E(w,\phi,N,T)}{\partial T}\big[1-S^E(w,\phi,N,T)\\
&\times(1-F((1-\beta)E(w,\phi,N,T)))\big]\Big\}
\end{aligned}
\tag{2.31}
$$

After some further manipulation I have:

$$\frac{\partial E(w,\phi,N,T)}{\partial T} = \frac{\beta\phi\left[1-S^E(w,\phi,N,T)\left(1-F\left((1-\beta)B^L(w,N,T)\right)\right)\right]}{1-\beta(1-\phi)\left[1-S^E(w,\phi,N,T)\left(1-F\left((1-\beta)E(w,\phi,N,T)\right)\right)\right]} \times \frac{B^L(w,N,T)}{\partial T} \tag{2.32}$$

Notice that $\frac{\beta\phi\left[1-S^E(w,\phi,N,T)\left(1-F\left((1-\beta)B^L(w,N,T)\right)\right)\right]}{1-\beta(1-\phi)\left[1-S^E(w,\phi,N,T)\left(1-F\left(E(w,\phi,N,T)\right)\right)\right]} < 1$. Equation (2.18) shows that $\frac{B^L(w,N,T)}{\partial T} > 0$. Thus, I obtain the following results:

$$\frac{\partial E(w,\phi,N,T)}{\partial T} = 0 \quad if \quad \phi = 0 \tag{2.33}$$

$$0 < \frac{\partial E(w,\phi,N,T)}{\partial T} < \frac{B^L(w,N,T)}{\partial T} \quad if \quad \phi > 0 \tag{2.34}$$

which concludes the proof that an increase in the PDB increases the value of employment for non-notified workers $E(w,\phi,N,T)$ only if they have positive expectations of layoff ($\phi > 0$).

The reservation wage for taking a new job in case the worker did not receive a notification at the end of the period, denoted by $x^{E,0}(w,\phi,N,T)$, is such that $W(x^{E,0}(w,\phi,N,T)) = \frac{x^{E,0}(w,\phi,N,T)}{1-\beta} = E(w,\phi,N,T)$. Thus, using equations (2.33) and (2.34) I obtain the following results:

$$\frac{\partial x^{E,0}(w,\phi,N,T)}{\partial T} = 0 \quad if \quad \phi = 0 \tag{2.35}$$

$$\frac{\partial x^{E,0}(w,\phi,N,T)}{\partial T} = (1-\beta)\frac{\partial B^L(w,N,T)}{\partial T} > 0 \quad if \quad \phi > 0 \tag{2.36}$$

which concludes the proof that an increase in the PDB increases the reservation wage $x^{E,0}(w,\phi,N,T)$ for taking a new job if no notification is received, but only if the probability of receiving such notification is non-zero.

2.9.1.5 Proof of Proposition II.7

Assuming an interior solution, the optimal search effort for a non-notified worker, denoted by $S^E(w,\phi,N,T)$, is given by:

$$\begin{aligned} S^E(w,\phi,N,T) =& \beta\Bigg\{\phi\int_{B^L(w,N,T)(1-\beta)}^{\infty}\left(W(x)-B^L(w,N,T)\right)dF(x)dx \\ &+(1-\phi)\int_{E(w,\phi,N,T)(1-\beta)}^{\infty}\left(W(x)-E(w,\phi,N,T)\right)dF(x)dx\Bigg\} \end{aligned} \tag{2.37}$$

Taking the partial derivative with respect to T:

$$\begin{aligned} \frac{\partial S^E(w,\phi,N,T)}{\partial T} =& -\beta\Bigg\{\phi\int_{B^L(w,N,T)(1-\beta)}^{\infty}\frac{\partial B^L(w,N,T)}{\partial T}dF(x)dx \\ &+(1-\phi)\int_{E(w,\phi,N,T)(1-\beta)}^{\infty}\frac{\partial E(w,\phi,N,T)}{\partial T}dF(x)dx\Bigg\} \end{aligned} \tag{2.38}$$

Given the results in equation (2.18), in equation (2.33) and in equation (2.34) I can establish the following results:

$$\frac{\partial S^E(w,\phi,N,T)}{\partial T} = 0 \quad if \quad \phi = 0 \tag{2.39}$$

$$\frac{\partial S^E(w,\phi,N,T)}{\partial T} < 0 \quad if \quad \phi > 0 \tag{2.40}$$

which concludes the proof that an increase in the PDB decreases search effort but only if workers have a positive probability of receiving a layoff notification.

Now, for $\phi > 0$, I know that $\frac{\partial E(w,\phi,N,T)}{\partial T} < \frac{B^L(w,N,T)}{\partial T}$ per equation 2.34. And since $E(w,\phi,N,T) > B^L(w,N,T)$, I can then establish that:

$$\frac{\partial S^L(w,1,T)}{\partial T} < \frac{\partial S^L(w,2,T)}{\partial T} .... < \frac{\partial S^L(w,N-1,T)}{\partial T} < \frac{\partial S^L(w,N,T)}{\partial T} < \frac{\partial S^E(w,\phi,N,T)}{\partial T} < 0 \tag{2.41}$$

which concludes the proof that the effect of increasing the PDB on discouraging search effort is smaller for non-notified workers than for notified workers.

2.9.2 Treatment effects formulas

2.9.2.1 Treatment effects on the hazard rate

Equation (2.4) specified the TE on the hazard rate of separation as:

$$TE_h(\bar{t},X,D) = h^{1,D}(\bar{t}|X,D) - h^{1,0}(\bar{t}|X,D) \tag{2.42}$$

Plugging in equations (2.9) and (2.10), I obtain:

$$\begin{aligned} TE_h(\bar{t},X,D) &= h_0(\bar{t})\, exp\,(\delta j + \gamma D + \beta X + \theta d) - h_0(\bar{t})\, exp\,(\delta + \gamma D + \beta X) \\ TE_h(\bar{t},X,D) &= h_0(\bar{t})\, exp\,(\delta + \gamma D + \beta X)\,[exp(\theta D) - 1] \\ TE_h(\bar{t},X,D) &= h^{1,0}(\bar{t}|X,D)\,[exp(\theta D) - 1] \end{aligned} \tag{2.43}$$

2.9.2.2 Treatment effects on the failure function

Equation (2.7) specified the TE on the failure function as:

$$\begin{aligned} TE_F(\bar{t},X,D) &= F^{1,D}(\bar{t},X,D) - F^{1,0}(\bar{t},X,D) \\ TE_F(\bar{t},X,D) &= -\,exp\left\{-\int_0^{\bar{t}} h^{1,D}(u|X,D)du\right\} + exp\left\{-\int_0^{\bar{t}} h^{1,0}(u|X,D)du\right\} \end{aligned} \tag{2.44}$$

Plugging in equations (2.9) and (2.10), I obtain:

$$
\begin{aligned}
TE_F(\bar{t},X,D) =& F^{1,D}(\bar{t},X,D) - F^{1,0}(\bar{t},X,D) \\
TE_F(\bar{t},X,D) =& -exp\left\{-exp(\theta D)\int_0^{\bar{t}} h_0(u)\, exp\,(\delta+\gamma D+\beta X)\, du\right\} \\
& + exp\left\{-\int_0^{\bar{t}} h_0(u)\, exp\,(\delta+\gamma D+\beta X)\, du\right\} \\
TE_F(\bar{t},X,D) =& -\left(exp\left\{-\int_0^{\bar{t}} h^{1,0}(u|X,D)du\right\}\right)^{exp(\theta D)} \\
& + exp\left\{-\int_0^{\bar{t}} h^{1,0}(u|X,D)du\right\} \\
TE_F(\bar{t},X,D) =& exp\left\{-\int_0^{\bar{t}} h^{1,0}(u|X,D)du\right\} \\
& \times\left[1-\left(exp\left\{-\int_0^{\bar{t}} h^{1,0}(u|X,D)du\right\}\right)^{exp(\theta d)-1}\right] \\
TE_F(\bar{t},X,D) =& \left(1-F^{1,0}(\bar{t},X,D)\right)\left[1-\left(1-F^{1,0}(\bar{t},X,D)\right)^{exp(\theta d)-1}\right] \qquad (2.45)
\end{aligned}
$$

2.9.2.3 Treatment effects on the cumulative incidence function

The TE for the CIF is given by:

$$
\begin{aligned}
TE_{CIF_k}(\bar{t},X,D) =& \int_0^t h_k^{1,D}(u|X,D)\times exp\left(-\int_0^u \left[\sum_{i\in K} h_i^{1,D}(w|X,D)\right] dw\right) du \\
& - \int_0^t h_k^{1,0}(u|X,D)\times exp\left(-\int_0^u \left[\sum_{i\in K} h_i^{1,0}(w|X,D)\right] dw\right) du \qquad (2.46)
\end{aligned}
$$

Using the fact that $h_i^{1,d}(u|X,D) = h_i^{1,0}(u|X,D)\times exp(\theta_i D)$, I can re-write equation (2.47) as:

$$
\begin{aligned}
TE_{CIF_k}(\bar{t},X,D) = &\int_0^t \Bigg\{ h_k^{1,0}(u \big| X,D) \times exp(\theta_k d) \\
&\times exp\left(-\int_0^u \left[\sum_{i \in K} h_i^{1,0}(w \big| X,D) \times exp(\theta_i D) \right] dw \right) \Bigg\} du \\
&- \int_0^t h_k^{1,0}(u \big| X,D) \times exp\left(-\int_0^u \left[\sum_{i \in K} h_i^{1,0}(w \big| X,D) \right] dw \right) du
\end{aligned}
$$

$$
\begin{aligned}
TE_{CIF_k}(\bar{t},X,D) = &\int_0^{\bar{t}} \Bigg\{ h_k^{1,0}(u \big| X,D) \times exp(-\int_0^u \sum_{i \in K} h_i^{1,0}(w \big| X,D) dw) \\
&\times \left[exp(\theta_k D) \left(exp(-\int_0^u \sum_{i \in K} \left[h_i^{1,0}(w \big| X,D) \left(exp(\theta_i D) - 1 \right) \right] dw) \right) - 1 \right] \Bigg\} du
\end{aligned}
$$

$$
\begin{aligned}
TE_{CIF_k}(\bar{t},X,D) = &\int_0^{\bar{t}} h_k^{1,0}(u \big| X,D) \times exp(-\int_0^u \sum_{i \in K} h_i^{1,0}(w \big| X,D) dw) \\
&\times \left\{ exp\left(\theta_k D - \int_0^u \sum_K \left[h_{i \in K}^{1,0}(w \big| X,D) \left(exp(\theta_i D) - 1 \right) \right] dw \right) - 1 \right\} du \qquad (2.47)
\end{aligned}
$$

CHAPTER III

The Supplemental Nutrition Assistance Program and Material Hardships among Low-Income Households with Children[1]

3.1 Introduction

This study examines the effects of participation in the Supplemental Nutrition Assistance Program (SNAP) on the food and non-food material hardships of low-income households with children.[2] A primary goal of SNAP is to reduce food insecurity among recipients, and recent studies have found that SNAP reduces food insecurity (Mykerezi and Mills, 2010; Ratcliffe et al., 2011). Beyond food insecurity, however, there is little research on the effects of SNAP participation on measures of non-food material hardship, even though there is reason to think that recipient households may effectively use some of their SNAP benefit for non-food consumption.

Data are drawn from the 1996, 2001 and 2004 panels of the Survey of Income and Program Participation (SIPP). Endogenous selection of individuals into SNAP raises basic identification concerns in identifying the causal impact of SNAP benefits for recipients. I

[1]This study was co-authored with Assistant Professor H. Luke Shaefer. It was funded by a cooperative research contract (58-5000-0-0083) between the National Poverty Center (NPC) at the University of Michigan and the U.S. Department of Agriculture, Economic Research Service (ERS) Food and Nutrition Assistance Research Program (FANRP). The ERS project representative is Alisha Coleman-Jensen. The views expressed are those of the authors and not necessarily those of the NPC, ERS, or USDA.

[2]SNAP was formerly known as the Food Stamp Program.

thus identify the effects of SNAP on both food and non-food material hardships among low-income households with children by estimating jointly the likelihood of household participation in SNAP and of experiencing food and non-food material hardships using a bivariate probit model. My main model specifications include instrumental variables that exploit changes in state SNAP program recertification period lengths and use of biometric eligibility requirements. These instruments meet standard metrics of strength.

My estimates of the negative impacts of SNAP on the risk of food insecurity—a reduction of 13.0 percentage points—are in line with recent existing studies (Ratcliffe et al., 2011). I further find a substantive and statistically significant negative relationship between SNAP participation and the risk that households will fall behind on their essential expenses including housing (by 7.4 percentage points), utilities (by 15.7 percentage points), and medical costs (by 8.5 percentage points). It is important to note that my point estimates are virtually identical in models with and without instruments. Thus, I am confident that identification of my estimates is coming from the structure of the bivariate probit. This may be true for other papers using similar non-linear methods with instruments to assess the effects of SNAP, such as Ratcliffe et al. (2011).

Although the identification of the effects are not driven by the instruments, my findings are robust to numerous sensitivity analyses and are suggestive that SNAP has a sizeable effect not just on the food security of households with children, but also on their non-food material well-being as well. This should have implications for federal policymakers, given that SNAP is currently the largest income transfer program in the US.

3.2 Background

SNAP benefits were received by 46.2 million individuals in October 2011; in fiscal year 2011, spending on SNAP totaled \$75.3 billion. Food security, a primary outcome used to evaluate SNAP, is defined as "access by all people at all times to enough food for an active, healthy life," while food insecurity is the absence of food security (Nord et al., 2002).

Beyond food insecurity, SNAP participation may reduce non-food material hardships by allowing recipients to reallocate resources originally directed toward the purchase of food to other essential expenses, such as housing, utilities and medical costs.

In recent years, scholars have analyzed measures of material hardships as alternatives to the official poverty line for assessing the well-being of low-income families (Cancian and Meyer, 2004; Heflin et al., 2009; Mayer and Jencks, 1989; Nolan and Whelan, 2010; Sullivan et al., 2008; Ouellette et al., 2004). Such measures "employ direct indicators of consumption and physical living conditions to examine whether families meet certain basic needs" (Ouellette et al., 2004, p. V). To my knowledge no existing study uses rigorous econometric methods to assess the effects of SNAP on non-food material hardship.

As noted, endogenous program take-up complicates efforts to evaluate the relationship between SNAP receipt and material hardships because it is likely that households with the most serious problems, after holding observed characteristics constant, are also the most likely to apply for benefits. Wilde (2007) and others have shown that low-income households who receive food stamps are more likely to report food insecurity than similar nonparticipating households (see also Jensen, 2002; Gundersen et al., 2009). Gibson-Davis and Foster (2006) write, "the problem with analyzing the impact of food stamps on food insecurity is that unmeasured or unobserved characteristics are likely correlated with both food stamps use and food security" (p.94, see also Bartfeld and Dunifon, 2005; Gundersen and Kreider, 2008; Wilde and Nord, 2005). Recent studies have used more sophisticated techniques, including instrumental variables approaches, and found a negative relationship between SNAP participation and food insecurity (Borjas, 2004; Gundersen and Oliveira, 2001; Mykerezi and Mills, 2010; Nord and Golla, 2009; Ratcliffe et al., 2011; Yen et al., 2008).

Borjas (2004) uses state variation in the treatment of immigrants before and after the 1996 welfare reform to test the effects of participation in means-tested programs on food insecurity within this population. He concludes that the evidence "suggests an important [negative] causal link between public assistance and food insecurity" for immigrants (p.1439). Yen et al. (2008) use the 1996-1997 National Food Stamp Program Survey, a small survey of income eligible households, to examine the effects of SNAP participation on food insecurity. They utilize a non-linear instrumental variable approach, with instruments measuring stigma as well as cross-sectional variation in some state SNAP policies and state-level immigrant population shares (state controls are not included). They also find a negative association between SNAP participation and food insecurity. Households receiving SNAP in their sample were less likely to report food insecurity than eligible households not receiving SNAP. This finding differs from virtually all nationally-representative samples, which may reflect specific characteristics of this sample.

Ratcliffe et al. (2011) pool data from the 1996-2004 SIPP panels and take a bivariate probit approach similar to the one I employ here to measure the effects of SNAP on food insecurity among households who are below 150% of poverty and have low assets. They include as instruments changes over time in state outreach spending per capita, use of biometric requirements, and a term interacting states' treatment of immigrants with noncitizen immigrant status of household heads. They find that SNAP participation substantially and statistically significantly decreases the risk of household food insecurity.

Mykerezi and Mills (2010) use cross-sectional data from the 1999 PSID and utilize static state-level error rates in benefits payments as an instrument, without including state-level controls. This leaves open the possibility that these instruments are capturing state characteristics other than error rates. Mykerezi and Mills also examine the impact on food insecurity of self-reported loss of benefits reportedly due to a decision by a government office. Like Ratcliffe et al. (2011), they find that SNAP participation has a substantial and statistically significant negative effect on food insecurity.

Beyond improving food security through increased food consumption, economic models stemming back to at least Southworth's (1945) canonical model indicate that households will, in most circumstances, indirectly use part of their SNAP benefits for non-food consumption by reducing their out-of-pocket food expenditures and redirecting those resources to other uses. Hoynes and Schanzenbach (2007) use county-level variations in the original date of implementation of the Food Stamps Program (FSP), from 1963 to 1975, and data from the PSID and the Decennial Censuses to show that the introduction of the FSP led to an overall increase in household food expenditures, but also to a decrease in out-of-pocket food spending, suggesting that households were redirecting some dollars originally spent on food to other expenses.

The Southworth (1945) model predicts that SNAP works essentially as an unconditional cash transfer program, unless participants are "constrained," meaning that their desired food consumption level is less than their SNAP benefit. Evidence from experimental designs (Fraker et al., 1995) and from nationwide consumption surveys (Fraker, 1990) indicates that only a small fraction of SNAP participants are constrained. Therefore, for most households, the economic effects of SNAP should be similar to those of a cash transfer program, warranting the analysis of its effects not only on food consumption but also on other non-food expenses. A first step in this investigation is to look at the impact of SNAP on other essential household expenditures such as rent, utilities, and medical care expenditures, which are captured in standard measures of non-food material hardship included in the SIPP.

To my knowledge, the current chapter is the first to use a bivariate probit approach to examine the effects of SNAP participation on measures of both non-food material hardships and household food insecurity among households with children. SNAP serves a heterogeneous population, and the program's impacts may be different for the various sub-groups, such as individuals and families without children and the elderly. By focusing on households with children, the largest group of SNAP recipients, I more precisely model both participation and program effects. I hypothesize that SNAP receipt should increase total household consumption and allow recipients to reallocate out of pocket resources across both food and non-food essential expenses.

3.3 Data

Data are drawn from public use files of the SIPP, collected by the U.S. Census Bureau. SIPP interviews are conducted every four months about each individual in the household for each intervening month, gathering data on demographics, income sources, public assistance program participation, household and family structure, and jobs and work history. I pool data from the 1996, 2001, and 2004 panels of the SIPP, each of which is 3-4 years long.[3]

Recent analyses of a number of large nationally representative surveys that measure income and program participation find that the SIPP generally does a superior job of measuring the income of poor households and measuring public program participation (Czajka and Denmead, 2008; Meyer et al., 2009). Under-reporting of benefits receipt in household surveys (in which respondents do not report public benefits that they have accessed) remains a limitation (Gundersen and Kreider, 2008). However, the SIPP does relatively well in terms of SNAP reporting rates. Meyer et al. (2009) estimate that the SIPP reported 87.7% of SNAP participants for 1998, 84.8% for 2003, and 82.9% for 2005, the years in my study frame that include the material hardship measures.

My sample includes households with resident children under 18 with at least one adult member over 18. Rather than trying to simulate SNAP eligibility, I follow Mykerezi and Mills (2010) and Ratcliffe et al. (2011) and restrict my sample to households based on low-income.[4] I restrict my main sample to households with an average gross income at or below 150% of poverty during the reporting wave (up to 4 reference months), using the monthly household-level poverty thresholds provided in the SIPP. If my sample were restricted by simulated eligibility, a significant proportion of households reporting SNAP participation would be coded as ineligible. This may relate to limitations in comparing income and as-

[3] A few states (Maine, Vermont, Wyoming, North Dakota and South Dakota) were not uniquely identifiable in the 1996 and 2001 panels, so observations from these states are dropped because they cannot be matched with state SNAP policy data (as is done by Gruber and Simon, 2008; and Ratcliffe et al., 2011).

[4] Unlike Ratcliffe et al. (2011), Mykerezi and Mills (2010) and I do not restrict by household assets. Doing so only marginally changes the sample composition and requires merging in assets data collected in other waves, which may not be representative of the household's circumstances when they applied for SNAP or when they completed the topical module with the material hardship questions.

sets reported in the SIPP with state eligibility calculations, or may be a result of fluctuating household incomes and assets following initial certification. The most important reason for using a gross income threshold for sample selection rather than simulating SNAP eligibility, though, is that there are concerns that income may be endogenous to participation. Households near the eligibility threshold may modify their earnings or assets in ways that makes them eligible (Ashenfelter, 1983). Thus, the effective eligibility threshold may be somewhat higher than the official one. In order to account for this, I use a threshold of 150% of the poverty line (rather than SNAP's gross income limit of 130%). Table 3.1 shows that 36% of households (with children) below 150% of the poverty line participate in SNAP. In contrast, only 2.6% of households above 150% of the poverty line participate. I test the robustness of my findings to sample selection with sensitivity analyses.

My key outcome variables are drawn from the SIPP's adult well-being topical modules administered once per panel in wave 8 of the 1996 panel (administered during 1998), wave 8 of the 2001 panel (administered during 2003), and wave 5 of the 2004 panel (administered during 2005). The SIPP is the primary source of nationally-representative data on material hardship in the US (Bauman, 1999; Beverly, 2001; Heflin et al., 2009; Ouellette et al., 2004; Wu and Eamon, 2010).

My first measure indicates whether a household broadly had difficulty meeting its essential household expenses. Households were asked "Next are questions about difficulties people sometimes have in meeting their essential household expenses for such things as mortgage or rent payments, utility bills, or important medical care. During the past 12 months, has there been a time when (YOU/YOUR HOUSEHOLD) did not meet all of your essential expenses?" Households that responded affirmatively were classified as having trouble meeting essential expenses. I also examine three additional, more specific, measures that ask whether a household reported falling behind on their rent/mortgage; whether they reported falling behind on their utility bills; and whether anyone in the household did not see a doctor or go to the hospital when needed because of cost. These are the hardships measured in SIPP that are most likely to be impacted by SNAP participation.[5]

I also report models in which the outcome is food insecurity to benchmark my estimates against existing studies that focus only on this outcome. The SIPP adult food security measures do not conform exactly to the official USDA food security scale; however, they have been used in several studies and are closely related to the official food security measure (Bitler et al., 2005; Gundersen et al., 2009; Nord, 2006; Ratcliffe et al., 2011).[6] Households are classified as food insecure if they responded affirmatively to at least two of a set of questions that can be used to measure food insecurity in the Adult Well-being Topical Module. See the appendix for further details. SIPP households only report on the main food insecurity measures once, in reference to the four months of the wave.

3.4 Econometric model

When dealing with two binary outcomes, an alternative to a linear instrumental variable approach such as two-stage least squares (2SLS) is to jointly estimate the system of equations describing each outcome using non-linear models, in particular a fully observed recursive bivariate probit model, as is done by Ratcliffe et al., 2011 (see also Heckman, 1978; Greene, 1998; Angrist and Pischke, 2009). Consider the following system where i indexes households:

[5]The SIPP adult well-being topical modules (TMs) ask households that reported trouble paying housing and utility costs whether they faced eviction or utility shut off. However, the incidence of these outcomes is so small that they do not adequately allow for the statistical power needed to test the relationship between them and SNAP. The TMs also include questions on housing quality, however, SNAP participants may have greater difficulty reallocating resources formerly spent on food to these expenses. I did estimate models in which the outcome was phone line disconnection. I found no negative effect across all model specifications. I think this may be an outmoded material hardship measure, given the increasing reliance of low-income households on pre-paid cell phones.

[6]Nord (2006) reports that an "assessment of the food security items using statistical methods based on the Rasch measurement model indicated that relative item severities were very nearly identical to those in the 1998 Current Population Survey Food Security Supplement, and analysis of CPS data comparing the SIPP scale with the standard U.S. Food Security Scale indicated that the SIPP scale was reasonably reliable and only moderately biased" (p. 2).

$$SNAP_i^* = Z_i\beta + \varepsilon_i \tag{3.1}$$

$$y_i^* = X_i\gamma + \delta SNAP_i + v_i \tag{3.2}$$

I posit that (potentially) eligible households decide to participate in SNAP by comparing costs and benefits using a net benefit function or latent index ($SNAP_i^*$), as described by equation (3.1). I do not observe directly the net benefit index $SNAP_i^*$, but only the program participation decisions. Thus I observe the dummy variable $SNAP_i = 1$ if $SNAP_i^* > 0$, and $SNAP_i = 0$ otherwise.[7]

My outcomes of interest are several measures of material hardship. Conceptually, I model that households report experiencing a hardship if an underlying latent index of financial distress (y_i^*), as described by equation (3.2), is above a certain threshold, which can be set to zero without loss of generality. As above, I do not observe y_i^* but only whether the household reports they are experiencing material hardship or not. In other words, I observe the dummy variable $y_i = 1$ if $y_i^* > 0$ and $y_i = 0$ otherwise.

I assume that the error terms ε_i and v_i follow a bivariate normal distribution, scaled such that variances equal to one and covariance equal to ρ. Placing a restriction on the variances of the random components allows for unique identification of the parameters. I also assume that the errors are serially uncorrelated and homoscedastic.

The system described by equations (3.1) and (3.2) is fully-observed and recursive. The fully-observed condition means that endogenous variables appear on the right hand side only as observed (Roodman, 2009). For example, in equation (3.2) the endogenous variable that appears in the right hand side is $SNAP_i$ (program participation) and not $SNAP_i^*$(the net benefit latent index). The recursive nature of the system means that there are clearly defined

[7]In my main specification, $SNAP_i$ is measured as *SNAP* participation in the final month of the wave because respondents' reporting is known to be most accurate in the month closest to the interview (Moore, 2007). In sensitivity analyses I utilize alternative definitions of *SNAP* participation, including requiring *SNAP* receipt in all months of the wave, any month of the wave, and just the first month of the wave. Results are robust.

stages of causation (Roodman, 2009; Wilde, 2000). In other words, SNAP participation has a causal impact on material hardship, and thus is included in equation (3.2), but material hardship does not affect the program participation net benefit latent index and therefore is excluded from equation (3.1). At first, this may seem a strong assumption, but that is not necessarily true since I am only ruling out any independent causal effect of actually reporting material hardship after controlling for the effect of observed factors Z_i (which could be equal to those in X_i) and modeling the unobserved terms ε_i and v_i. Moreover, the recursive nature of the system follows from the condition of logical consistency (Maddala and Lee, 1976).[8]

3.4.1 Parameters identification

The parameters of equation (3.1) can be consistently estimated via a probit regression. However, if $\rho \neq 0$, then a standard probit regression of equation (3.2) using the observed SNAP participation variable would produce biased results because $Cov(SNAP_i, v_i) \neq 0$. In particular, if $\rho > 0$, meaning that, after controlling for the effects of observed characteristics, households who are more likely to participate in SNAP are also more likely to experience material hardship, then the estimated value of δ would be biased upwards. This

[8] Consider rewriting the system in its non-recursive form as follows:

$$SNAP_i^* = Z_i\beta + \theta y_i + \varepsilon_i \tag{3.3}$$
$$y_i^* = X_i\gamma + \delta SNAP_i + v_i \tag{3.4}$$

Then we could substitute equation (3.3) into (3.4) and obtain the following expression:

$$y_i^* = X_i\gamma + \delta \times 1\left[\varepsilon_i > -Z_i\beta - \theta y_i\right] + v_i \tag{3.5}$$

Thus, we would observe:

$$y_i = 1 \text{ if } v_i > -X_i\gamma - \delta \times 1\left[\varepsilon_i > -Z_i\beta - \theta\right] \tag{3.6}$$
$$y_i = 0 \text{ if } v_i \leqq -X_i\gamma - \delta \times 1\left[\varepsilon_i > -Z_i\beta\right] \tag{3.7}$$

Note that if $\theta \neq 0$, then it is possible to find values of v_i and ε_i (given the parameters in the model) such that y_i equals both 0 and 1 or neither. Thus, the model is logically consistent only if $\theta = 0$, i.e. if it is recursive.

is the source of the bias that, if not accounted for, produces positive associations between SNAP participation and material hardship, as has been documented between SNAP and food insecurity. Under the distributional assumptions of the error terms, though, consistent estimation of δ requires jointly estimating equations (3.1) and (3.2) within a bivariate probit model.

One might assume that the identification of the parameters in the system described by equations (3.1) and (3.2) requires the use of instrumental variables. For instance, Maddala and Lee (1976) argue that the parameters of the second equation are not identified without exclusion restrictions on the exogenous variables. However, McCall (1992) showed that in a fully-observed recursive system of two binary choices the existence of at least two continuous regressors with positive support over the real plane is sufficient for identification of the parameters of the model including the joint distribution of the errors. Exclusion restrictions or instrumental variables are not necessary for identification. Furthermore, Wilde (2000) showed that if the distribution of the errors is known to be bivariate normal (as we assume is our case), then only the existence of at least one exogenous regressor with sufficient variation in its values is sufficient for identification. In other words, the parameters in equations (3.1) and (3.2) would be identified even if $Z_i = X_i$ as long as they contained one regressor with enough variation in its values. The estimates will be valid as long as the covariates included in X_i (or Z_i) are exogenous (uncorrelated with the errors) and the normality assumption of the disturbance terms is correct.[9]

It is important, however, to recognize that, without exclusion restrictions, identification is coming from the non-linearities introduced by censoring and from the structure of the model, rather than by a (quasi) natural experiment as in the standard linear instrumental variables approach. Even in the case where instruments are included and meet standard metrics for strength, it is still possible that the non-linearities are driving the estimates, which may lead to a misinterpretation of the type of evidence produced by these models.

[9]For example, whereas heteroskedasticity only affects the efficiency of linear models, it may be a more serious threat for the consistency of limited dependent variable models like the probit.

In this case, it is important to verify to what extent the results are being driven by the instruments or by the structure of the model. I conducted two tests: 1) estimating the bivariate model without instruments and 2) using a standard linear instrumental variables (IV) approach.

3.4.2 Instrumental variables

In my main specification, vector Z_i encompasses X_i but also includes SNAP state policy variables that are predicted to increase the cost of participation. Given that my models include state dummies (in X_i), the instruments used for identification are given by the changes in those state policy variables (rather than by their levels). The policy data by state-year are drawn from a dataset prepared by USDA ERS researchers, similar to that used by Ratcliffe et al. (2011). I selected two instruments that are strong predictors of SNAP participation in my sample. Standard errors in the estimations were clustered at the state level.

My first instrument is the proportion of assistance units with earners within each state with a recertification period of 3 months or less, by state-year. Numerous studies have shown that the length of recertification periods has a significant effect on SNAP participation (Hanratty, 2006; Ratcliffe et al., 2008; Ribar et al., 2008; Schmeiser, 2012), and various constructions of state recertification periods have been used for instrumenting SNAP participation (Yen et al., 2008; Schmeiser, 2012). Recertification periods typically range between 1 and 12 months, and in some cases longer. Over my period of analysis the proportion of assistance units with earners with a recertification period of 3 months or shorter has changed within all states. This is because federal encouragement in the late 1990s led to a large increase in the proportion of recipients—especially those in assistance units with earners—recertified within three months. This proportion, though, fell considerably after 2000 (Hanratty, 2006).[10]

My second instrument is the use of biometric technology (mostly fingerprinting of

[10]The average percentage of states' caseloads that had a recertification period of three months or less fell considerably from 1998 to 2003 and from 2003 to 2005.

applicants), used with the goal of reducing fraud. I hypothesize—as did Ratcliffe et al. (2011)—that this should discourage program participation. Biometrics technology was used by Texas, Arizona, and New York throughout my study period, but was introduced in California halfway through the study period. Massachusetts implemented biometrics and then ended it during my study period. While this instrument relies on changes in only two states, biometric requirements have a statistically significant impact on the probability of SNAP participation.[11] When my models are run using 2SLS, the F-statistic associated with the excluded instruments in the first stage is 21.2, above the standard suggested cut-off value of 10.0 (Stock et al., 2002).

Other controls included in X_i (and in Z_i) are demographic and geographic characteristics that have been shown to be related to SNAP participation and/or material hardship. I include a count variable for the number of children in the household and an indicator for household headship (headed by husband/wife, single-male headed, and single-female headed).[12] I also control for the highest level of schooling reported by an adult household member, and include an indicator for the presence of a full-time worker. Race and ethnicity, age (and age squared), sex, metropolitan residence and U.S. citizenship of the household head are included. I also control for the state-month unemployment rate. Finally, dummies for state, year, and calendar month are included in all models.

To estimate the average causal effect of SNAP participation on the probability of experiencing material hardship, I average the difference between the predicted hardship probability with and without SNAP for each individual in the sample. In other words, I use the following formula:[13]

[11] The percentage of my sample subject to biometric requirements rose from 1998 to 2003 and fell from 2003 to 2005.

[12] Originally I used the number of children in the household within three age categories, but consistency in the point estimates led me to collapse this variable into one.

[13] Given the fact that the identification of the parameters does not rely on the selected instruments (as discussed in Section 3.5) I do not interpret the results as local average treatment effects (LATE). On the contrary, I exploit the structure provided by the bivariate probit model and the embedded assumption of homogenous effects to estimate the average causal effects by simulating the impacts of SNAP participation on the probability of experiencing material hardships for both SNAP participants and non-participants.

$$E_X\left[E\left[y_i|X_i, SNAP_i=1\right]-E\left[y_i|X_i, SNAP_i=0\right]\right]=\frac{1}{N}\sum_{i=1}^{N}\left(\Phi\left(X_i\gamma+\delta\right)-\Phi\left(X_i\gamma\right)\right) \tag{3.8}$$

Alternatively, I estimated the average causal effect of SNAP participation by the percentage change in the probability of material hardship, given by the following formula:[14]

$$E_X\left[\frac{E\left[y_i|X_i, SNAP_i=1\right]}{E\left[y_i|X_i, SNAP_i=0\right]}-1\right]*100=\frac{1}{N}\sum_{i=1}^{N}\left(\frac{\Phi\left(X_i\gamma+\delta\right)}{\Phi\left(X_i\gamma\right)}-1\right)*100 \tag{3.9}$$

3.5 Results

Table 3.1 presents weighted summary statistics. Column 1 reports means for the households with incomes above 150% of poverty who are excluded from the multivariate analyses. The next three columns are restricted to households below 150% of poverty, divided into 4,948 observations for low-income households not reporting SNAP (column 3) and 3,079 observations for those reporting receipt of SNAP benefits (column 4). Only 13.4% of households with incomes above 150% of poverty lived in households that reported difficulties meeting essential expenses, and only 6.2% reported food insecurity. Among households at or below 150% of poverty, 29.7% of those not receiving SNAP and 48.7% of SNAP recipients reported trouble meeting their essential expenses. Similarly, just over a third of low-income SNAP households reported that they were food insecure, compared to 21.4% of non-SNAP households.

This positive association between reported SNAP participation and measures of material hardship are likely the result of the selection process of what households decide to participate in SNAP. Thus, simple probit regressions that do not model the participation decision will show a positive and statistically significant effect of SNAP participation on the probability of experiencing a material hardship. This is shown in Table 3.2 for two key

[14] Standard errors for average causal effects were calculated using 250 bootstrap replications (within state).

outcomes: food insecurity and trouble paying household essential expenses. Estimates are reported as coefficients of the latent linear indexes. Using the estimated coefficients I find that SNAP participation is actually associated with an increase of 8 percentage points in the risk of experiencing food insecurity, and with an increase of 13 percentage points in the risk of having trouble meeting essential expenses.

Table 3.3 reports the estimation results of the coefficients of the latent linear indexes in the bivariate probit models described in equations (3.1) and (3.2), which model jointly the participation decision and the probability of experiencing hardships. Columns 1 and 3 of Table 3.3 also report on the correlation coefficient between the errors components in the SNAP participation equation and in the material hardship equations. As expected, the correlation coefficient is positive ($\rho_{\varepsilon\nu} > 0$), large, and statistically significant in both models. This means that, after controlling for observed characteristics, there are unobserved factors driving both SNAP participation and material hardship, so that households that are more likely to report SNAP are also more likely to report experiencing food insecurity or difficulty meeting essential household expenses. Once I take into account this selectivity into the program, I find that SNAP has now a statistically significant negative effect on the latent indexes for both types of hardships.

Table 3.3 also reports that, after controlling for other factors, each additional child in a household is associated with a higher probability of SNAP participation. Female-headed households are much more likely to participate than those headed by a married couple. Households in which the reference person is Black or Asian or Pacific Islanders are more likely to participate than those in which the reference person is white, and households in which the reference person is a US citizen are more likely to participant than those with a non-citizen reference person. Increased education is associated with a decreased probability of SNAP participation, and households with 1 or more full-time workers are less likely to participate than families without. My instruments are also strong predictors of SNAP participation.[15] As a larger proportion of a state's SNAP caseloads are recertified in three months or less, the probability of participation decreases. Use of biometrics is also associated with a reduction in the probability of participation.

Regarding the probability of hardships, columns 2 and 4 report that additional children are associated with both increased food insecurity and non-food material hardship. Female-headed households are also more likely to experience both outcomes than households headed by a married couple. Higher levels of education and the presence of full-time workers are both associated with a lower risk of food insecurity and trouble meeting essential expenses. Households in which the reference person is black are more likely to experience both outcomes than households in which the reference person is white. Households in which the reference person is of Hispanic origin are more likely to be food insecure but not more likely to experience non-food material hardship than families in which the reference person is non-Hispanic.

The coefficients from Table 3.3 are use to estimate the average causal effects of SNAP participation on the probability of reporting (1) food insecurity and (2) difficulty meeting essential household expenses. These estimates are presented in Table 3.4, along with new estimated effects for three sub-categories of (2): (3) falling behind on rent or mortgage; (4) falling behind on utility bills and (5) medical hardship. All estimates are derived from bivariate probit models as the one discussed above. I present estimates of the average causal effects in percentage points in column 1 (using the formula in equation (3.8)) and in percentage changes in column 2 (using the formula in equation (3.9)).

SNAP participation results in a statistically significant 13.0 average percentage point reduction in the risk of being food insecure, which is equivalent to an average decrease of 41.7% in its incidence. This effect size is quite close to what is reported by Ratcliffe et al. (2011), who find that SNAP reduces food insecurity among households (not restricted to households with children) by 16.2 percentage points, even though they use a different set

[15] The Chi-square statistics for the null hypothesis that the excluded instruments coefficients are zero are 53.80 (p-value 0.0000) for column 1 and 30.62 (p-value of 0.0000) for column 3.

of instruments (a point I return to later).

I also find that SNAP is associated with a statistically significant 28.8 average percentage point reduction in the risk that households will have trouble meeting their essential expenses, equivalent to a 60.1% reduction in the incidence of non-food material hardship. SNAP participation leads to a statistically significant decrease of 7.4 percentage point (or 35.7%) in the risk that households fall behind on their rent or mortgage, and a 15.7 percentage point (46.8%) decrease in the risk of falling behind on household utility bills. Finally, SNAP is associated with a decrease of 8.5 percentage points in medical hardship (a reduction of 47.3%).[16]

These results from columns 1 and 2 appear to provide relatively robust evidence from an instrumental variable model that SNAP not only reduces the food insecurity of recipient households, but also has a statistically significant and substantial negative effect on non-food material hardships. The standard metrics of strength for my instruments suggest that the instruments are performing well. Importantly, though, columns 3 and 4 report point estimates from a simple test to assess the source of my identification of these effects. These columns report on results from bivariate probit models that are identical to those reported in columns 1 (and 2) in every way, except that my instruments (recertification periods and biometric requirements) are omitted. The resulting point estimates for the impact of SNAP participation on material hardship are virtually identical to the models with the instruments: a 13.9 percentage point decrease in food insecurity, a 33.9 percentage point decrease in trouble meeting essential expenses, a 9.4 percentage point decrease in the risk of falling behind on rent/mortgage, a 19.7 percentage point decrease in the risk of falling behind on utility bills, and a 9.2 percentage point decrease in the probability of not seeking medical care.

Moreover, when I estimated a standard 2SLS regression, in no case do I obtain a statis-

[16]It is worth noting that the point estimates for the marginal effect of SNAP coverage in percentage points for rent + utilities + medical hardship add to approximately the marginal effect in percentage points on difficulty meeting essential expenses. These three categories make up the prompt given to respondents in the broader question, which suggests consistency across respondent reporting.

tically significant, negative point estimate. In two cases the point estimates for the effect on food insecurity and problems meeting essential expenses are negative, but neither are statistically significant. Taken together, these results suggest that identification of my estimates does not rely on the instruments, but rather is coming from the structural form of the bivariate probit model.

According to Wilde (2000), one exogenous regressor with enough variation should suffice to identify δ in equation (2). Table 3.5 tests the robustness of my results to the sequential introduction of model covariates that are arguably exogenous. In the case of food insecurity, a negative and statistically significant effect of SNAP is achieved after controlling only for the number of children in the household. Note that the estimated effects are very stable to the introduction of additional covariates. In the case of problems meeting essential expenses, it takes adding just two exogenous controls (number of children and household structure) to obtain a negative and statistically significant effect. Similarly, adding extra controls does not change the estimated effects significantly. Therefore, Table 3.5 provides suggestive evidence that the estimated effects would also be robust to the exclusion of potentially unobserved factors.

In Table 3.6, I report on a series of additional sensitivity tests. I began by trying three alternative constructions of my observed SNAP receipt variable. I define SNAP receipt to be equal to one if and only if: 1) receipt in all reference months of the wave, 2) receipt in any reference month, and finally 3) receipt in the first reference month of the wave. In all three cases, the point estimates of the causal effects on food insecurity and non-food material hardship remain statistically significant. Requiring participation in any reference month or the first reference month reduces the size of the point estimates somewhat.

I also restricted the sample at two alternative income thresholds. My estimates at the 175% threshold are highly significant. At the more-restrictive 125% threshold sample, the point estimates are smaller and the food insecurity outcome becomes insignificant, most likely due to the loss of statistical power because of the smaller sample size. Some studies on SNAP and food insecurity control for income (Yen et al., 2008). Although this is an endogenous variable, I do this in panel C of Table 3.6 (using dummies for household income falling within 0-50%, 51-100%, and 101-150% of poverty level) and my results remain robust. I also ran a specification that drops all SIPP observations with imputed values. Finally, I ran models adding household-level weights. In all cases, my results remain robust.

3.6 Discussion

Because SNAP participation may allow households to reallocate resources otherwise directed toward food purchases, SNAP has a strong impact on many aspects of household economic well-being.. The prominence of SNAP among means-tested programs suggests that it should be evaluated using a broader set of material hardship outcomes than food insecurity and other food-related outcomes. To my knowledge, this study is the first to use a bivariate probit approach to estimate the effect of SNAP benefits on non-food measures of material hardship. My estimates suggest that SNAP is having a substantial positive effect on the non-food material well-being of participant households.

Under-reporting of benefits receipt in the SIPP remains a limitation, even though the SIPP does relatively well in terms of reporting rates (Meyer et al., 2009). Unfortunately, there is currently no source of nationally-representative data linking the demographic characteristics of individuals with administrative data on SNAP participation. Thus, the current study would be impossible with any existing source of administrative data. There are now a number of studies using different data and different methods that offer evidence that SNAP reduces food insecurity. I find that my estimates of the effects of SNAP of food insecurity are similar to those reported by Ratcliffe et al. (2011). It should be noted, though, that my point estimates are virtually identical in models with and without instruments, even though my instruments meet standard metrics of strength. Thus, I am confident that identification of my estimates is coming from the structure of the bivariate probit. The same may be true in the case of Ratcliffe et al.'s findings on food insecurity as well, and may be true of other papers in the SNAP and food insecurity literature—and even other literatures—that use instrumental variables in non-linear estimation frameworks.

While my own estimates are not evidence from a "quasi" natural experiment, they remain suggestive that SNAP participation significantly reduces not just food insecurity, but also non-food material hardship. My estimates suggest that households spread their SNAP benefit over food and non-food essential expenses, and that SNAP is having a substantively large and broad impact on the material well-being of recipient households. This is an important finding, largely because of the major changes to means-tested income maintenance programs since the 1990s. In effect, SNAP is acting like a negative income tax, providing a base level of support to recipient households that is not (typically) conditioned on labor force participation. Because it is playing this important role in the US, future studies of SNAP should analyze its effects on other measures of material hardships, even in the absence of adequate instruments.

Table 3.1: SIPP sample means (households with children)

	>150% poverty	<=150% of poverty		
		All	Non-SNAP	SNAP
	(1)	(2)	(3)	(4)
Observations	24,347	8,027	4,948	3,079
MATERIAL HARDSHIP CHARACTERISTICS				
Food Hardship				
Food Insecurity in past four months	0.062	0.261	0.214	0.345
Non-Food Hardship				
Problem meeting essential expenses	0.134	0.365	0.297	0.487
Did not pay full rent	0.053	0.177	0.142	0.240
Did not pay full gas, oil, or electricity bills	0.095	0.277	0.214	0.389
Did not go to the doctor because of cost	0.052	0.139	0.133	0.150
HOUSEHOLD CHARACTERISTICS				
SNAP Participation	0.026	0.360	0.000	1.000
Household Income as % Poverty	4.244	0.848	0.944	0.678
Number of children	1.821	2.284	2.182	2.464
Household structure				
Headed by husband/wife	0.773	0.467	0.584	0.260
Male Headed Family	0.066	0.068	0.071	0.061
Female Headed Family	0.160	0.465	0.345	0.679
Maximum education Level				
Less than High School	0.031	0.203	0.164	0.273
High School	0.185	0.352	0.332	0.387
Some college	0.380	0.339	0.358	0.304
BA degree or above	0.404	0.106	0.146	0.036
1+ Full time workers in household	0.891	0.554	0.665	0.355
Live in a metropolitan area	0.811	0.762	0.768	0.752
State-month unemployment rate	5.196	5.343	5.318	5.387
REFERENCE PERSON CHARACTERISTICS				
Male	0.537	0.343	0.424	0.197
Female	0.463	0.657	0.576	0.803
Age	40.533	37.480	38.203	36.192
Race				
White	0.829	0.678	0.741	0.567
Black	0.111	0.257	0.194	0.371
American Indian	0.020	0.024	0.025	0.022
Asian or Pacific Islander	0.039	0.040	0.040	0.040
Hispanic Origin	0.125	0.260	0.281	0.223
US citizen	0.928	0.836	0.807	0.888

Source: Author's analyses of a pooled sample from the 1996-2004 panels of the SIPP

Notes: Means are weighted. Observations belong to the fourth reference month only. Households must have a positive number of children. The household reference person must be 19 or older. I used the following waves: 1996w8, 2001w8, 2004w5. These are the waves in which adult well-being topical modules were collected.

Table 3.2: Estimation results of Probit models (latent index coefficients)

	Food Insecurity (1)	Problem meeting essential expenses (2)
SNAP Participation	0.248***	0.355***
	[0.044]	[0.047]
Household characteristics		
Number of children	0.026*	0.025**
	[0.015]	[0.012]
Married couple Headed Family	—	—
Male Headed Family	0.240***	0.179**
	[0.066]	[0.075]
Female Headed Family	0.192***	0.078**
	[0.046]	[0.040]
Less than High school	—	—
High School Diploma	-0.130***	-0.030
	[0.036]	[0.034]
Some college	-0.145***	0.115**
	[0.042]	[0.055]
BA degree or Advanced degree	-0.393***	-0.247***
	[0.065]	[0.051]
1+ full time workers	-0.222***	-0.211***
	[0.038]	[0.032]
Lives in a metropolitan area	0.058	-0.016
	[0.050]	[0.039]
State-month unemployment rate	0.067**	0.025
	[0.026]	[0.026]
Reference person characteristics		
Female	0.110**	0.097**
	[0.046]	[0.044]
Age	0.026***	0.026***
	[0.006]	[0.008]
Age Squared	-0.000***	-0.000***
	[0.000]	[0.000]
White	—	—
Black	0.025	0.056
	[0.039]	[0.049]
American Indian	-0.007	-0.031
	[0.140]	[0.093]
Asian or Pacific Islander	0.017	-0.165
	[0.082]	[0.116]
Hispanic Origin	0.147***	-0.046
	[0.055]	[0.053]
US citizen	-0.031	0.116***
	[0.055]	[0.041]
Observations	8,027	8,027

Source: Author's analyses of a pooled sample from the 1996-2004 panels of the SIPP
Notes: *** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance ant 10% level. All estimations include state dummies, year dummies and month dummies. Standard errors (in brackets) are clustered by state.

Table 3.3: Estimation results of Bivariate Probit models (latent index coefficients)

	Model 1		Model 2	
	SNAP Participation	Food Insecurity	SNAP Participation	Problem meeting essential expenses
	(1)	(2)	(3)	(4)
SNAP Participation	—	-0.427**	—	-0.874***
		[0.168]		[0.248]
Household characteristics				
Number of children	0.169***	0.063***	0.168***	0.092***
	[0.014]	[0.019]	[0.014]	[0.018]
Married couple Headed Family	—	—	—	—
Male Headed Family	0.315***	0.295***	0.317***	0.273***
	[0.061]	[0.063]	[0.057]	[0.075]
Female Headed Family	0.579***	0.317***	0.589***	0.317***
	[0.035]	[0.063]	[0.037]	[0.073]
Less than High school	—	—	—	—
High School Diploma	-0.204***	-0.170***	-0.197***	-0.113***
	[0.037]	[0.033]	[0.039]	[0.036]
Some college	-0.392***	-0.225***	-0.389***	-0.070
	[0.046]	[0.038]	[0.047]	[0.072]
BA degree or Advanced degree	-0.853***	-0.548***	-0.844***	-0.534***
	[0.070]	[0.073]	[0.074]	[0.078]
1+ full time workers	-0.652***	-0.362***	-0.655***	-0.455***
	[0.031]	[0.059]	[0.032]	[0.054]
Lives in a metropolitan area	-0.069	0.038	-0.077*	-0.047
	[0.047]	[0.049]	[0.047]	[0.039]
State-month unemployment rate	0.052	0.085***	0.064*	0.060*
	[0.038]	[0.026]	[0.038]	[0.032]
Reference person characteristics				
Female	0.129**	0.131***	0.113**	0.129***
	[0.052]	[0.048]	[0.050]	[0.042]
Age	-0.025***	0.020***	-0.025***	0.010
	[0.006]	[0.006]	[0.006]	[0.009]
Age Squared	0.000**	-0.000***	0.000**	-0.000**
	[0.000]	[0.000]	[0.000]	[0.000]
White	—	—	—	—
Black	0.333***	0.099**	0.331***	0.186***
	[0.038]	[0.048]	[0.037]	[0.047]
American Indian	0.165	0.026	0.179	0.036
	[0.141]	[0.132]	[0.144]	[0.099]
Asian or Pacific Islander	0.296***	0.084	0.295***	-0.012
	[0.088]	[0.084]	[0.092]	[0.113]
Hispanic Origin	0.063	0.152**	0.079	-0.015
	[0.086]	[0.060]	[0.091]	[0.038]
US citizen	0.214**	0.016	0.209**	0.184***
	[0.083]	[0.056]	[0.086]	[0.054]

State Policies				
Biometrics	-0.350***		-0.283***	
	[0.058]		[0.055]	
Short period recertification	-0.186**		-0.156*	
	[0.093]		[0.088]	
Correlation of errors terms	0.403***		0.726***	
	[0.110]		[0.148]	
Observations	8,027	8,027	8,027	8,027

Source: Author's analyses of a pooled sample from the 1996-2004 panels of the SIPP

Notes:

*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance ant 10% level. All estimations include state dummies, year dummies and month dummies. Standard errors (in brackets) are clustered by state.

Table 3.4: Average causal effect of SNAP coverage on several measures of material hardship

	Bivariate Normal Results				IV approach (2SLS)		
	With Instruments		Without Instruments		Using recertification period	Using Biometrics	Both Instruments
	Percentage points	Percentage change	Percentage points	Percentage change	All estimates are in percentage points		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Food Hardship							
(1) Food Insecurity	-0.130**	-0.417***	-0.139***	-0.437***	0.626	-0.196	0.138
	[0.051]	[0.140]	[0.045]	[0.115]	[0.399]	[0.144]	[0.202]
Non-Food Hardship							
(2) Problem meeting essential expenses	-0.288***	-0.601***	-0.339***	-0.668***	0.298	-0.027	0.105
	[0.081]	[0.132]	[0.056]	[0.082]	[0.285]	[0.153]	[0.130]
(3) Did not pay full rent	-0.074**	-0.357***	-0.094***	-0.430***	0.302	0.236**	0.259**
	[0.030]	[0.121]	[0.029]	[0.100]	[0.249]	[0.114]	[0.130]
(4) Did not pay full gas/oil/electricity bills	-0.157***	-0.468***	-0.197***	-0.549***	0.434	0.042	0.206
	[0.061]	[0.146]	[0.057]	[0.121]	[0.335]	[0.119]	[0.168]
(5) Did not go to the doctor because of cost	-0.085**	-0.473**	-0.092**	-0.502***	-0.048	-0.196	-0.136
	[0.041]	[0.193]	[0.040]	[0.180]	[0.292]	[0.153]	[0.168]

Source: Author's analyses of a pooled sample from the 1996-2004 panels of the SIPP

Notes:

*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance ant 10% level.

All estimations include state dummies, year dummies and month dummies. Standard errors (in brackets) are clustered by state. Standard errors are calculated from 250 bootstrap draws within each state.

Table 3.5: Sensitivity of SNAP effects to inclusion of different controls (in percentage points)

	Model specification									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
I. Food Insecurity										
SNAP Participation	0.133***	-0.166	-0.152***	-0.176***	-0.154***	-0.159***	-0.130***	-0.133***	-0.135***	-0.139***
	[0.010]	[0.130]	[0.033]	[0.036]	[0.042]	[0.039]	[0.041]	[0.041]	[0.042]	[0.044]
Errors correlation (ρ)	0.000	0.545*	0.467***	0.502***	0.432***	0.446***	0.390***	0.397***	0.400***	0.419***
	[0.063]	[0.260]	[0.081]	[0.092]	[0.111]	[0.106]	[0.106]	[0.105]	[0.101]	[0.101]
II. Problem meeting essential expenses										
SNAP Participation	0.185***	0.484*	-0.289***	-0.301***	-0.321***	-0.312***	-0.308***	-0.324***	-0.329***	-0.339***
	[0.011]	[0.285]	[0.062]	[0.063]	[0.047]	[0.048]	[0.066]	[0.062]	[0.057]	[0.062]
Errors correlation (ρ)										
	0.000	-0.549	0.735***	0.761***	0.790***	0.771***	0.760***	0.796***	0.809***	0.841
	-	[0.757]	[0.096]	[0.089]	[0.098]	[0.094]	[\0.118]	[0.113]	[0.104]	[0.100]
Observations	8,027	8,027	8,027	8,027	8,027	8,027	8,027	8,027	8,027	8,027
Number of children		X	X	X	X	X	X	X	X	X
Household structure			X	X	X	X	X	X	X	X
Maximum education Level				X	X	X	X	X	X	X
1+ Full time workers in family					X	X	X	X	X	X
Reference person sex and age						X	X	X	X	X
Race, ethnicity and citizenship							X	X	X	X
Urban/Rural area								X	X	X
State unemployment rate									X	X
State, year and month dummies										X

Notes:

*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance ant 10% level.

Standard errors are calculated from 250 bootstrap draws within each state.

Table 3.6: Sensitivity analysis of average causal effects of SNAP coverage on material hardships

	Food Insecurity (Percentage points)	Problem meeting essential expenses (Percentage points)
A. Alternative definitions of SNAP participation		
= 1 if participation in all reference months, 0 otherwise	-0.161***	-0.315***
	[0.051]	[0.072]
= 1 if participation in any reference month, 0 otherwise	-0.092*	-0.211**
	[0.054]	[0.088]
= 1 if participation in first reference month, 0 otherwise	-0.122**	-0.276***
	[0.057]	[0.083]
B. Alternative samples by Income		
175% of Poverty	-0.163***	-0.309***
	[0.037]	[0.058]
125% of Poverty	-0.049	-0.216**
	[0.056]	[0.107]
C. Other sensitivity tests		
Controlling for family income	-0.130***	-0.229**
	[0.047]	[0.102]
Dropping imputed values	-0.192***	-0.373***
	[0.043]	[0.058]
Weighted regressions	-0.152***	-0.280***
	[0.053]	[0.092]

Source: Author's analyses of a pooled sample from the 1996-2004 panels of the SIPP

Notes:

*** denotes statistically significance at 1% level; ** denotes statistically significance at 5% level; * denotes statistically significance ant 10% level.

Standard errors are calculated from 250 bootstrap draws within each state. All estimations include state dummies, year dummies and month dummies. All measures of SNAP participation in Panel A were instrumented with states' policies in the first reference month.

3.7 Appendix: Food security in the SIPP

I defined a household as being food insecure if they report at least two of the following, in reference to the previous 4 months (Nord, 2006):

- The food the household bought didn't last and they didn't have money to get more (answers "often" or "sometimes").
- The household couldn't afford to eat balanced meals (answers "often" or "sometimes").
- The adults in the household ever cut the size of their meals or skipped meals because there wasn't enough money for food (answer "yes").
- The adults in the household ever ate less than they felt they should because there wasn't enough money to buy food (answer "yes").
- The adults in the household ever did not eat for a whole day because there wasn't enough money for food (answer "yes").

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