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Partial and general equilibrium effects of unemployment insurance

Mathias von Buxhoeveden

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Postal address: P O Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

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Partial and General Equilibrium Effects of Unemployment Insurance

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Identification, Estimation and Inference

UPPSALA
UNIVERSITET

Department of Economics, Uppsala University

Visiting address: Kyrkogårdsgatan 10, Uppsala, Sweden
Postal address: Box 513, SE-751 20 Uppsala, Sweden
Telephone: +46 18 471 00 00
Telefax: +46 18 471 14 78
Internet: http://www.nek.uu.se/

ECONOMICS AT UPPSALA UNIVERSITY

The Department of Economics at Uppsala University has a long history. The first chair in Economics in the Nordic countries was instituted at Uppsala University in 1741.

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* Public economics
* Macroeconomics
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Abstract
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Essay I: Wage setting models typically posit a tight relationship between the generosity of unemployment insurance (UI) and equilibrium wages. This paper estimates the effect of UI on workers' wages. I build on a unique feature of the unemployment policy in Sweden, where workers can opt to buy supplement UI coverage above a minimum mandated level. In January 2007, the government sharply increased the price of UI, and the share of workers with supplement coverage fell from 90% to 80%. I exploit variation in the price of UI across industries to measure the effect of industry level UI-coverage on wages. My estimates suggest that a 10 percentage point reduction in the share of workers covered by supplement UI reduce wages by 5%. Since I rely on variation in UI-coverage at the industry level, these estimates contain wage adjustments from collective and individual level bargaining. Finally, I use the estimated UI-wage effect to derive bounds on worker bargaining power in a simple DMP model and find that it can be at most 0.12. This evidence supports wage setting mechanisms that tie wages to the generosity of UI.
Essay II: This paper estimates the effects of unemployment insurance (UI) benefits on job finding rates and entry level wages for unemployed high school leavers. Up to year 2007, Swedish high school-students who became unemployed shortly after graduation were entitled to UI-benefits once they became 20 years of age. Therefore, the start of an unemployment spell relative to the 20:th birthday creates potentially exogenous variation in time to treatment. I exploit this to estimate the effect of UI benefits on unemployment duration and entry level wages. The results show that there is a large and statistically significant negative effect of UI benefits on the employment hazard. There are no detectable effects on entry level wages. This would suggest that unemployment benefits induce high school leavers to postpone labor market entry but does not seem to effect job match quality.
Essay III: Difference-in-Differences (DID) is a quasi-experimental method to evaluate the effect of a treatment. In the basic version, two groups are observed at two dates. The treatment group becomes treated in the second period. The effect of the intervention is estimated by comparing the change in the outcome experienced by the treatment group to the corresponding change in the control group. However, assessing the impact of an intervention is often complicated by the well-known problem of sample selection. In randomized experiments, one popular method to address this is to implement Lee (2009) bounds. This paper extends Lee (2009) bounds to the DID design. Identification results, estimators and a simple bootstrap procedure for computing standard errors are presented.

Keywords: Labor Economics, Unemployment Insurance, Wage Level and Structure, Job Search, Duration Analysis, Econometric and Statistical Methods, Selection Models

Mathias von Buxhoeveden, Department of Economics, Box 513, Uppsala University, SE-75120 Uppsala, Sweden.

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Dedicated to my father

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The final 2 years of my time as a PhD Student was spent at the Institute for the Evaluation of Labour Market and Education Policy (IFAU). This was a great experience. IFAU is filled with excellent researchers, and my work has benefited a lot from your comments and suggestions. I have also enjoyed all of the conversations unrelated to economics, especially those pertaining to Adrians prospects as a professional boxer.

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Finally, I would like to thank my family. Mom, thank you for helping me remember that there is more to life than work. Dad, an appropriate thank you is beyond the scope of this text, so I decided to dedicate the thesis to you instead. Helene, thank you for taking an interest in what I do, it means a lot. Arto, I sincerely believe that our discussions helped spark my interest in intellectual pursuits. Lastly, I would like to thank my partner in life. Sofie, your love and support means the world to me. Also, thank you for helping me find my passport. This was really useful for getting to the ASSA meetings.

Uppsala, April 2019

Mathias von Buxhoeveden

Contents

Introduction

About 72 countries worldwide, including all of the OECD countries, have some form of unemployment insurance (UI), designed to provide financial support to displaced workers. These programs take up a substantial share of government expenditures, and the effects of UI-policy have been a recurrent topic of policy debate.

According to one view, UI-benefits makes unemployment more attractive to the jobless, and this reduces incentives for the unemployed to look for work. Therefore, decreasing the generosity of UI can lead to substantial gains in employment. Others have argued that these effects are modest, and emphasised that UI increases the time and resources available to the unemployed to search for a better job. Clearly, empirical evidence is important for distinguishing between these claims, and for the design of UI-policy. The purpose of this thesis is to contribute with new evidence on the effects of UI-policies, and to develop new econometric methods to study these effects.

Economic theory predicts that UI-policies affect the labor market along two margins. First, the generosity of UI affects jobs search decisions of unemployed individuals. Second, UI affects the wage-structure and subsequent job creation decisions of firms. The following stylized decomposition, which I borrowed from Hagedorn et al. (2013), helps illustrate the two margins:

$$\text{Job finding rate}_{it} = \underbrace{s_{it}}_{\text{search intensity}} \times \underbrace{f(\theta_t).}_{\text{finding rate per unit of s}} \tag{1}$$

That is, the likelihood that jobseeker i finds a job at time t depends on search-intensity, s_{it}, and aggregated economic conditions, $f(\theta_t)$.[1] To take an extreme example, if there are no job vacancies created by employers, $f(\theta_t) = 0$, and no amount of search effort can yield a positive probability of finding a job.

Changes in UI benefit policies can affect the search intensity of unemployed individuals, s_{it}, and the wage-structure and subsequent job creation decisions of firms, $f(\theta_t)$. A complete assessment of the labor market implications of UI-policies has to account for the effects on s_{it} and $f(\theta_t)$.

Motivated by this, Chapter 1 estimates the effect of UI on the wage-structure. This is an important general equilibrium effect, since changes in the wage-structure have implications for the job creation decisions of firms. Chapter 2 estimates the impact of UI on job search decisions. Chapter 3 develops new

[1] θ corresponds to labor market tightness, i.e the number of job vacancies divided by the number of unemployed workers.

econometric methods that can deal with methodological problems that often emerge in settings such as those in Chapter 2. I will now proceed and summarize the work and findings of this thesis. The final part of the introduction puts the main results together, discuss policy-implications and suggests topics for future research.

Unemployment Insurance and Wage Formation

The first chapter of this thesis measures the sensitivity of wages to changes in UI. The identifying variation is derived from a unique feature of the Swedish UI system. All workers are entitled to a minimum level of UI but can opt to buy supplemental coverage through UI-funds. In January 2007, the newly elected right-wing government sharply increased the price of supplement UI, and this lead to a significant decline in the take-up rate of UI. In 2007, approximately 380,000 workers opted out of the UI system. Importantly, the sharp decline in UI-coverage coincided with a period when 85% of wage-agreements expired and were subject to renegotiations.

I focus on the impact of industry level UI-coverage on equilibrium wages. This is because Sweden, like other northern European countries, has very strong unions that play a major role in wage determination. Furthermore, unions are tied to industries, which imply that variation in UI-coverage at the industry level will capture wage adjustments that occur through collective *and* individual-level bargaining.

To estimate the effect of industry level UI-coverage on wages, I exploit an institutional feature of the premia hike. The reform introduced an additional fee that tied the premium in each UI fund to the average unemployment rate in that fund. Furthermore, any given UI fund will typically provide insurance to workers from several different industries. It follows that the premia for supplement UI workers in any given industry face is partly determined by the unemployment rate in other industries that buy UI from the same fund. I use this part of the premia as an instrument for changes in industry level UI-coverage.

I begin the analysis by simply relating wage growth to changes in industry level UI-coverage using a difference-in-difference (DID) approach. I find that wage growth decline in a robust and significant manner in industries where a larger share of workers opt out of UI. Quantitatively, the results suggest that a 10 percentage point reduction in industry level UI-coverage reduce wages by 1 percent.

To bolster confidence in the results, I exploit the entire pre-treatment period (2002-2006) and conduct a series of placebo exercises which verify that there were no significant differences in wage growth prior to the reform. Although encouraging, this analysis is not completely satisfactory since the choice to opt out of UI is most likely endogenous. In particular, the choice to leave UI should be related to factors such as perceived displacement risk.

To address this, I implement the IV-strategy. Here, I instrument changes in industry level UI-coverage with the part of the UI premia that stems from other industries unemployment. The point estimates suggest that a 10 percentage point decrease in industry level UI-coverage reduce wages by 5 percent.

The differential decline in industry level UI-coverage steams from additional, plausibly exogenous, sources. For instance, workers who hold part-time employment are particularly sensitive to changes in the price of UI (IAF, 2007, 2008). These workers are not spread out uniformly across industries. Therefore, the general increase in the price of UI will also have different implications for the-up rate of UI across industries. In a second step, I exploit this using an IV approach. Here, I instrument changes in industry level UI-coverage with the share of workers that held part-time employment the year prior to the reform. Quantitatively, the point estimates are very similar to the IV-strategy that relies on the UI-premia differentiation, but there are large gains in precision.

Altogether, these results support wage-setting models that tie wages to the generosity of UI.

Unemployment Insurance and Youth Labor Market Entry

Chapter 2 estimates the impact of UI-benefits on job finding rates and entry level wages for unemployed high school leavers. My empirical strategy is to exploit age-discontinuities in the Swedish UI-system. Between 2002 and 2006, all unemployed high school leavers became entitled to UI once they turned 20 years. Moreover, Swedish high school student typically graduate in spring the year they turn 19 years. Therefore, the start of an unemployment spell relative to the 20:th birthday creates plausibly exogenous variation in the duration until they become eligible for UI.

The experiment I have in mind is as follow. Consider two groups of high school students that graduates the year they turn 19 years. They are unable to find employment and register as unemployed in October. Now, assume that the first group are born in January and the second in February. This imply that no one can claim UI between October - December (since they are all 19 years of age at the time). However, in January, the first group become 20 years of age and are eligible for UI. My empirical strategy is to compare the employment hazards in January, when the first group can claim UI but the second one cannot.

I extend this analysis to a larger sample, and match each group to a comparison cohort that become unemployed at the same time, but are born one month later and therefore have to wait an additional month until they become eligible for UI. Again, the idea is to compare the employment hazards in the period when the first group can claim UI but the comparison cohort cannot.

The analysis builds on the econometric framework pioneered by Van den Berg et al. (2010). They show that such comparisons identify a causal effect of the treatment on the employment hazard under two conditions. First, the treatment and control group have to be comparable when they enter unemployment. Second, the dynamic selection has to be identical until treatment is imposed. This is achieved by imposing the "no anticipation" assumption. Here, this means that future UI-benefit eligibility does not affect current job search behaviour. This assumption would be violated if, for instance, unemployed high school leavers adjust their job search behaviour in anticipation of future UI-benefit payments.

I begin the analysis by validating the identifying assumptions through a series of falsification tests. First, I implement balancing tests and verify that the distribution of important covariates, such as grades, are balanced across treatment and control groups at the start of the unemployment spells. To validate the "no anticipation" assumption, I implement a series of placebo checks that test for differences in the employment hazards in the pre-treatment period, when neither group can claim UI. Overall, 5.3% of the placebo tests are significant at the 5% level, which is expected by chance.

I proceed and estimate the effect of UI benefits on the employment hazard. Variation in time to treatment allows me to explore how the effect of UI benefits vary depending on the elapsed duration that payments are made. All of the point estimates suggest that UI-benefits reduce job finding rates, but the effect does not vary depending on how far into the unemployment spell payments are made.

To gain precision, I pool all of the available treatment-control group comparisons and estimate them jointly. The results are strongly significant and indicate that becoming eligible for UI benefits reduce monthly job finding rates by 1 percentage point. The baseline hazard is roughly 8%. Hence, the treatment effect translates into a 12.5% reduction in the conditional probability of leaving unemployment.

In the final part of the paper, I leverage the same treatment-control group comparisons and estimate the effect of UI-benefits on entry level wages. There are no detectable effects of UI on entry level wages. Altogether, these results suggest that UI-benefits induce high school leavers to postpone labor market entry, but does not seem to effect job match quality.

Partial Identification in Difference-in-Difference Models with Missing Outcome Data

Chapter 3 develops new econometric methods that can deal with methodological problems that often emerge in settings such as those in Chapter 2. When studying the effects of UI on the duration of unemployment, an important question is *how* changes in UI-policy affect individual-level job search behaviour. In the classic search model by Mortensen (1977), UI affects the duration of unemployment by changing the lowest wage-offer that an unemployed worker is willing to accept. By contrast, Card et al. (2007) considers a search model where UI changes search-intensity, and there is no effect on job acceptance decisions. To distinguish between competing models of job search behaviour, it is crucial to directly measure the effect of UI on post-unemployment wages.

The methodological challenge is that wages are only observed for those who found a job. This creates a selection problem since changes in UI can affect the likelihood that an unemployed workers finds a job in the first place.

There has been significant methodological progress on how to deal with the selection problem in the context of a randomized experiment. In particular, Lee (2009) develops an attractive method to bound average causal effects with non-random selection, the so called "Lee Bounds". This approach is, however, only applicable to a randomized experiment, which is problematic since most public policies are not implemented with an experimental design.

Difference-in-differences (DID) is a popular method to estimate causal effects in the absence of experimental data. However, there are currently no effective methods to deal with selection bias in a DID setting.

Motivated by this, Chapter 3 extends the "Lee bounds" to the DID setting. Identification results, estimators and a simple bootstrap procedure for computing the standard errors are presented.

Concluding Remarks

I started the introduction with a stylized decomposition of the job finding rates of unemployed job-seekers. The main take-away is that job finding probabilities depends on search intensity and aggregated macroeconomic conditions. Economic theory suggest that UI-policies have implications for both.

In Chapter 2, I studied the effects of UI on individual-level job search behaviour. The main result is that UI-benefits raises the average amount of time people spend out of work. Is this sufficient to infer a positive effect of UI on aggregated unemployment?

Not if changes in UI-policies also have implications for aggregated macroeconomic conditions. The results in Chapter 1 suggest that it does. In particular, I found evidence of a positive effect of UI on wages. Furthermore, economic theory predicts that an increase in wages lowers the profits firms receive from filled jobs, and that this depresses vacancy creation. In terms of the stylized decomposition, this translates into worse aggregated economic conditions (a lower $f(\theta_t)$).

Does this imply that the impact of UI on individual-level job search behaviour underestimates the effect on aggregated unemployment? Not necessarily. Lalive et al. (2015) show that changes in UI-policy have additional implications for aggregated economic conditions. Furthermore, their results suggest that the effects on job-search behaviour *overestimates* the impact on aggregated unemployment.

Altogether, the evidence suggests that UI-policies have implications for search-intensity *and* macroeconomic conditions. To asses the impact of UI on aggregated unemployment, one must account for all of this. These are promising topics for future research.

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1. Unemployment Insurance and Wage Formation

1.1 Introduction

The Great Recession displaced 25 million workers around the world. In the United States, unemployment insurance (UI) benefit duration was extended from the usual 26 weeks to as long as 99 weeks. The policy response was controversial. Barro (2010) raised concerns that UI discourages job search, and that this could have contributed to the slow recovery. Others emphasized the potential stimulus effect of UI benefits, (Summers, 2010).

Despite a long literature on the impact of UI on individual-level job search behaviour,[1] there is limited evidence on the macroeconomic effects of UI-policies. The reason for this is simple. Economic theory does not provide a one-to-one mapping between the microeconomic effect of UI and aggregated unemployment. For instance, large microeconomic effects could be consistent with small macroeconomic effects, if benefit extensions decrease job finding rates among UI recipients, but increases that of non-recipients (Lalive et al., 2015; Levine, 1993). Alternatively, in the standard DMP model (Mortensen and Pissarides, 1994) with exogenous search effort, a benefits extension improves workers outside option, puts upward pressure on wages and this depresses vacancy creation. Exogenous search effort implies a zero microeconomic effect of UI, but the decline in vacancy creation leads to a rise in aggregated unemployment.

Therefore, the UI-wage-pressure channel is crucial for a complete evaluation of the macroeconomic effects of UI, and the sensitivity of wages to changes in UI has important implications for the design of UI-policy and for economists' understanding of wage formation.[2]

In this paper, I estimate the effect of industry level UI-coverage on wages. I focus on UI-coverage at the industry level for institutional reasons. Sweden, like other northern European countries, has very strong unions that play a major role in wage determination. Furthermore, unions are tied to industries,

[1] See for example Caliendo et al. (2013); Card et al. (2007); Card and Levine (2000); Carling et al. (2001); Van Ours and Vodopivec (2008); Nekoei and Weber (2015); Lalive (2007)

[2] Conventional wage-setting protocols typically posit a tight relationship between the generosity of UI and equilibrium wages. See for instance the literature on "efficiency wages" (Krueger and Summers, 1988; Weiss, 2014; Katz, 1986; Akerlof and Yellen, 1990; Shapiro and Stiglitz, 1984) and union bargaining models (Calmfors and Driffill, 1988; MaCurdy and Pencavel, 1986; McDonald and Solow, 1981; Nickell, 1982; Ulph, 1982; Brown and Ashenfelter, 1986)

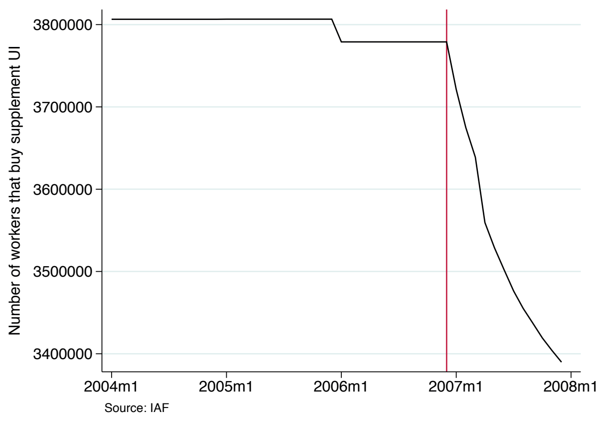

Figure 1.1. Number of workers with supplement UI over time

which imply that variation in UI-coverage at the industry level will capture wage adjustments that occur through collective *and* individual-level bargaining.

The identifying variation is derived from a unique feature of the Swedish UI system. *All* workers are entitled to a minimum level of UI but can opt to buy supplemental coverage through UI-funds. In January 2007, the newly elected right-wing government sharply increased the price of supplement UI. As visible in Figure 1.1, this lead to a significant decline in the take-up rate of supplement UI. In 2007, approximately 380,000 workers opted out of the UI system. Importantly, the sharp decline in UI-coverage coincided with a period when 85% of wage-agreements expired and were subject to renegotiations.

To estimate the effect of industry level UI-coverage on wages, I exploit an institutional feature of the premia hike. In addition to a general increase, the reform introduced a fee that tied the premium in each UI fund to the average unemployment rate in that fund. Furthermore, any given UI fund will typically provide insurance to workers from several different industries. Therefore, the premia for supplement UI workers in any given industry face is partly determined by the unemployment rate in other industries that buy UI from the same fund. I use this part of the premia increase as an instrument for changes in industry level UI-coverage.

Table 1.1 illustrates the approach with two industries. Both had unemployment rates of 2 % in 2006. Still, the yearly premium for supplement UI was 60 USD higher for workers in the saw milling industry. This is because they share UI funds with industries that have higher unemployment rates. Furthermore, the additional cost of UI was associated with a sharper decline in UI-coverage and weaker growth in wages.[3] My instrumental variable approach amounts to asking whether this pattern generalizes to a larger sample.

Industry	Unemployment rate, 2006	UI premium, 2007
Saw milling	2%	540 USD
Electricity components	2%	480 USD

Table 1.1. *IV example*

I begin the analysis by simply relating wage growth to changes in industry level UI-coverage using a difference-in-difference (DID) approach. I find that wage growth decline significantly in industries where a larger share of workers opt out of UI. Quantitatively, the results suggest that a 10 percentage point reduction in industry level UI-coverage reduce wages by 1 percent.

To bolster confidence in the results, I exploit the entire pre-treatment period (2002-2006) and conduct a series of placebo exercises which verify that there were no significant differences in wage growth prior to the reform. Although encouraging, this analysis is not completely satisfactory since the choice to opt out of UI is most likely endogenous. In particular, the choice to leave UI should be related to factors such as perceived displacement risk.

To address this concern, I instrument changes in industry level UI-coverage with the part of the UI premia that stems from other industries unemployment. The point estimates suggest that a 10 percentage point decrease in industry level UI-coverage reduce wages by 5 percent.

The differential decline in industry level UI-coverage steams from additional, plausibly exogenous, sources. For instance, workers who hold part-time employment are particularly sensitive to changes in the price of UI (IAF, 2007, 2008). These workers are not spread out uniformly across industries. Therefore, the general increase in the price of UI will also have different implications for the-up rate of UI across industries. In a second step, I exploit this using an IV approach. Here, I instrument changes in industry level UI-coverage with the share of workers that held part-time employment the year prior to the reform. Quantitatively, the point estimates are very similar to the IV-strategy that relies on the UI-premia differentiation, but there are large gains in precision.

In the final part of the paper, I augment the empirical analysis by interpreting the results through the lens of a simple DMP model (Diamond, 1982; Mortensen and Pissarides, 1994). Here, I use the estimated UI wage effect to derive informative bounds on worker bargaining power. My approach starts from the observation that shifts in the UI benefit level pass through into wages

[3]UI coverage in the saw milling and electrical component industry fell by 8 and 4 percentage points respectively. Furthermore, monthly wages in the electricity component industry rose by an additional percent between 2006 and 2007.

by one minus workers bargaining power, such that wages are more sensitive to changes in UI if workers have low bargaining power. The initial increase in wages is partly offset by a reduction in labor market tightness since an increase in UI benefits puts upward pressure on wages, reduce vacancy creation and depresses workers outside option.[4] The quantitative magnitude of this effect is unknown but theory restricts the sign to be non-positive. I derive bounds on worker bargaining power by considering the logical extremes where this effect is either zero or arbitrarily large. Using this strategy, I find that worker bargaining power can be at most 0.12.

Previous research on the general equilibrium effects of UI has followed two lines of inquiry. There is a large literature based on the estimation of structural models derived from Mortensen and Pissarides (1994)[5]. There is also a new line of research that employs microeconometric methods to estimate the macroeconomic effects of UI. Almost all of the available evidence relies on variation in the potential duration of UI across states in the U.S, following the Great Recession. For instance, Hagedorn et al. (2013) employ a border discontinuity design, and compare macroeconomic outcomes in neighbouring counties, separated by a state border.[6] They find that benefit extensions raise wages, lead to a contraction in vacancy creation and a rise in unemployment. However, recent work by Marinescu (2017) and Chodorow-Reich and Karabarbounis (2016) challenge these findings by documenting small effects of UI on wages and aggregated unemployment.

It is worth pointing out that changes in UI benefit entitlement may not get passed into wages in the U.S. for institutional reasons. First, conditional on separation, the take-up rate of UI is low compared to other OECD countries. Hence, UI is not a part of the non-employment scenario for many workers. Second, those who quit their jobs without a valid reason are not eligible for UI, which imply that more generous UI benefit does not necessarily shift workers threat point in wage bargaining. Theoretically, both of these mechanisms could explain a low elasticity of wages with respect to UI, even if non-employment is the relevant outside option in wage bargaining.

However, Schoefer et al. (2018) study the effect of UI on wages using four reforms of the Austrian UI system. Workers in Austria are always eligible for UI if they quit, and the take-up rate of UI conditional on separation is high. Hence, UI should be a part of the non-employment scenario for most workers. They still find that wages are insensitive to changes in UI. Schoefer et al. (2018) argues that this presents a puzzle to conventional wage setting models, and that non-employment may not constitute the relevant threat point in wage bargaining.

[4] Labor market tightness is defined as the number of vacancy's divided by the number of unemployed workers.

[5] See for example Millard and Mortensen (1997); Shi and Wen (1999); Krause and Uhlig (2012).

[6] This paper is controversial, and the credibility of their research design has been challenged by Amaral et al. (2014); Boone et al. (2016); Dieterle et al. (2016); Hall (2013).

A possible explanation for these results is that union-affiliation does not map one-to-one with any particular part of the earnings distribution. This implies that the treatment does not vary at a level that can capture wage-adjustments that occur through collective bargaining. I rely on variation in UI-entitlement at the industry-level, where collective bargaining occurs, which could potentially explain why I find that wages are sensitive to changes in UI.

The rest of this paper is organized as follows. In the next section, I introduce a simple DMP model, which clarifies the economic mechanisms at work and facilitates interpretation of the empirical results. Section 1.3 describes the institutional context and the UI premia increase that took place in 2007. Section 1.4 presents the data sources and perform some descriptive analysis. After this initial analysis, section 1.5 proceeds and exploit the premia increase to estimate the impact of UI benefits on wages using a difference-in-difference *and* an IV strategy. Section 1.6 interprets the empirical results through the lens of my theoretical framework. Section 1.7 concludes.

1.2 Theoretical Framework

This section introduces a simple version of the Diamond-Mortensen-Pissarides (DMP) model, and discuss some theoretical predictions of the UI wage effect. This is an equilibrium search model were wages are determined through Nash bargaining between firms and workers.

The transmission of UI policy into equilibrium wages is determined by worker bargaining power (γ) and the responsiveness of labor market tightness, (θ), to changes in UI policy, where θ is defined as the number of vacancies divided by the number of unemployed workers. In section 1.6, I will use my empirical results to derive informative bounds on γ.

My point of departure is an economy populated by infinitely lived workers who are either employed or unemployed. Each firm employ at most one worker, and employed workers produce an instantaneous flow of output, y. The job separation rate, σ, is taken as exogenous. The worker value functions can be written as:

$$rW = w + \sigma(U - W) \tag{1.1}$$

$$rU = b + \lambda_u(\theta)(W - U). \tag{1.2}$$

Where rW and rU corresponds to the flow value workers obtain in employment and unemployment respectively. The value of employment consists of two parts: (i) the wage, w and (ii) the job separation rate times the loss from becoming unemployed relative to staying employed.

Similarly, the value of unemployment consists of (i) the current flow of UI benefits, b, and (ii) the job finding rate, λ_u, times the gain from finding employment over staying unemployed.

The firm value functions are given by:

$$rJ = y - w + \sigma(V - J) \tag{1.3}$$

$$rV = -c + \lambda_v(\theta)(J - V). \tag{1.4}$$

Where, rJ and rV are the flow values of a filled and an unfilled vacancy respectively. Similarly, the value of a filled vacancy consists of (i) the flow of output minus the wage and (ii) the job separation rate times the loss of an unfilled relative to a filled vacancy. The value of an unfilled vacancy consist of (i) the vacancy positing cost, c, and (ii) the job-filing rate times the gain from a filled relative to an unfilled vacancy.

The standard assumption in the DMP model is that wages are determined through Nash bargaining. Formally, when workers and firms meet, they bargain over wages such that:

$$w = \arg\max_{w}(W(w) - U)^{\gamma} \times (J(w) - V)^{1-\gamma}. \tag{1.5}$$

Hence, workers and firm bargain over the wage, with bargaining weights γ, (1 - γ). The first order condition from equation (1.5) combined with free entry ($V = 0$), produce the wage curve:

$$w = (1-\gamma)b + \gamma(y + c\theta). \tag{1.6}$$

Where w is the equilibrium wage. Hence, the model predicts a simple linear relationship between a change in the benefit level (db) and the change in the equilibrium wage (dw):

$$dw = (1-\gamma)db + \gamma c\frac{\partial\theta}{\partial b}db. \tag{1.7}$$

Equation 1.7 can be understood as follows. An increase in the UI benefit level (b) improves workers outside option, and this exerts upward pressure on equilibrium wages. The transmission of changes in the outside option into equilibrium wages is determined by the term $(1-\gamma)$.

Moreover, when wages increase, firms receive smaller profits from filled jobs. This depresses vacancy creation and reduce the value of unemployment, since finding new job becomes more difficult. Hence, workers outside option become less attractive and this counteracts some of the initial increase in equilibrium wages. This is reflected in the second term in equation 1.7, which is negative since $\frac{\partial\theta}{\partial b} \leq 0$. Thus, the net effect of UI on wages cannot be theoretically determined.

1.3 Institutional Context

This section review the institutional details of unemployment insurance and wage setting in Sweden, and the reform that I study.

1.3.1 Unemployment Insurance

The UI system in Sweden consists of two parts. The first part is mandated and provide basic coverage to *all* workers. The benefit level under the basic plan is low and unrelated to pre-displacement earnings. Between 2005 and 2007, unemployed workers covered by the basic plan received a daily allowance of 320 SEK ($\approx$ 35 USD).[7] To get a sense of the magnitude, this corresponds to a replacement rate of approximately 20% for the median wage earner.

The second part of the UI system is voluntary. In particular, workers can opt for comprehensive UI coverage by paying a monthly premium to UI funds. Workers are free to opt in and out of comprehensive UI at any time, but need to have paid the premium for at least 12 consecutive months to be eligible for comprehensive UI. Moreover, to qualify for either insurance plan, workers have to fulfil a labor market attachment criterion. During the past 12 months prior to displacement, workers need to have had at least a part-time job for 6 months.

Comprehensive UI replaces 80% of pre-unemployment earnings up to a cap. In 2007, the daily allowance was capped at 680 SEK ($\approx$ 75 USD). Moreover, the ceiling is fairly low, and approximately 70% of *unemployed* workers have pre-unemployment earnings above the ceiling. Still, the UI benefit level for most workers is more than twice as high if they are covered by comprehensive UI. The benefit level is the only difference between the insurance plans.[8] Benefit duration was capped at 300 days in 2007.

The voluntary part of the UI system is administrated through several UI funds. In 2007, there were 36 UI funds, tied to different industries/occupations.[9] For instance, there is one UI fund restricted to those who are employed in the teaching profession.

The government heavily subsidizes the UI funds, and more than 90% of their expenditures on UI are covered by subsidizes from the state. Furthermore, the generosity of UI and the monthly insurance premium are entirely determined by government policy.

1.3.2 2007 Reform

Following the September 2006 general election, the Social democratic government was ousted and replaced by the right-wing coalition. The Swedish

[7]Benefits are paid out 5 days per week, which means that 320 SEK per day translated into a monthly income of 6400 SEK ($\approx$ 700 USD).

[8]The replacement rate under comprehensive UI is reduced from 80% to 70% after 200 days of unemployment. However, this will only affect the UI benefit level for workers who have pre-displacement *below* the cap. The benefit ceiling is however binding for most workers so I will ignore this feature of the UI system.

[9]There is also one UI fund called "Alfa kassan" that provide coverage to *all* workers, regardless of which industry they are employed in.

parliament subsequently decided to sharply increase the premium for UI fund membership, a decision taken on December 21, 2006.

This was achieved by introducing an additional fee that the UI funds had to pay to the government each month. The fee was determined through a formula written into UI law. In addition to a general increase, UI funds with higher expenditures on benefits had to pay a larger fee to the government each month. The motivation behind the reform was partly to make UI actuarially fair, and to incentivise the UI funds to tighten monitoring of benefit recipients. The reform was implemented on January 1, 2007, and all UI funds immediately decided to sharply increase their membership premiums.

As visible in Figure 1.2, the average monthly premia rose from around 100 SEK to 320 SEK. The surge in pricing also lead to a sharp decline in the take up rate of supplement UI. In 2007, the share of workers covered by comprehensive UI was reduced from 90% to 80%.

Figure 1.2. Monthly UI fund premium and the take-up rate of supplement UI

1.3.3 Wage Setting

Sweden is heavily unionized and almost all workers (91%) are covered by collective bargaining agreements. These take place at the sectoral (or occupational) level and typically impose lower bounds on workers' wages. Bargaining occurs at three levels. First, unions and employer organizations set the frame for wage formation through central agreements. Once negotiations at the central level are complete, bargaining at the local (establishment) level occurs. Here, the local union and firm representatives curtail the central agreement to the establishment level. Finally, wages at the individual level are set in negations between the manager and the worker.

In 2007, an unusually large number of central agreements expired and were subject to renegotiations (Medlingsinstitutet, 2007). This was the largest round of revisions since 1993, and the majority of unions and employer organizations signed new agreements. Negotiations at the central level were initiated in spring 2007.

The impact of the premia hike on the take-up rate of supplement UI was immediate. By February 2007, more than 100,000 workers had already opted out of supplement UI. Hence, the sharp decline in the take-up rate of supplement UI coincided with a period when wages were fairly flexible.

Moreover, the wage data used in this paper is collected by Statistics Sweden during the fall each year. The timing of measurement is explicitly motivated by the desire to include new wage-agreements. Hence, there should be plenty of time for any potential wage adjustments to occur before the data was collected.

1.4 Data Sources and Descriptive Statistics

In this section, I introduce the data sources used in the empirical analysis, describe the construction of the cost of comprehensive UI variable and provide basic descriptive statistics.

1.4.1 Data

This paper uses data from several Swedish administrative registries. The first registry contain UI fund membership information for the entire population (ages 16 - 64) between 2005 - 2009. This data was compiled by Statistics Sweden using the UI funds membership registries. The dataset contains indicators for if the individuals are buying supplement UI as of December each year.

The UI fund membership data are combined with survey data on wages and industry of occupation from Statistics Sweden's wage statistics. The survey is conducted annually during the fall and covers all workers in the public sector and larger private firms, as well as a random sample of workers in small firms (altogether, approximately 50% of private sector workers are covered). Moreover, wages are recorded as monthly full-time equivalent wages and are not derived from some measure of earnings and hours worked.

The industry classification codes were used to divide the labor market into industries. The codes consist of 5 digits and indicates at an increasingly detailed level (2 digits being the least and 5 the most detailed) the type of industry in which an individual is employed. I used the first 3 digits to divide the labor market into industries.

My empirical analysis focus on the UI-premia hike that was implemented in 2007. The industry classification codes used by Statistics Sweden at the time were introduced in 2002, which therefore defines the starting point for my sample. To avoid potentially confounding effects of the Great Recession, I end the sample in 2007. The sample is further restricted to one employment spell per worker each year. If an individual had several employment spells in a given year, I restrict attention to the spell with the highest wage. Furthermore, some industries are very small and are not observed in the wage statistics every year. I exclude these and restrict the sample to industries which employed at least 500 workers in 2002, and were observed every year between 2002 and 2007.

The instrumental variable approach exploits the differential increase in the premia for supplement UI. Unfortunately, the UI-fund membership data only reveals if an individual buys supplement UI, but from *which* UI fund. To link industries to UI-funds, I add data on unemployment from the Swedish public employment office. It covers the universe of unemployment spells between 1990 and 2015. This data is combined with the registry ASTAT, which contains detailed information on the universe of UI benefit spells from 1999 onwards. Importantly, ASTAT ties *all* payments to UI funds.

To link industries to UI funds, I combine the wage survey from 2005 with unemployment outcomes from 2006.[10] Specifically, I link workers who became displaced in 2006 to the industries they were employed in 2005. I then use the empirical distribution of payments from different UI funds to estimate the share of workers in an industry that buy insurance from a particular fund.[11]

Information about the monthly premias each UI fund charge from 2004 onwards are publicly available on the IAFs web page. I downloaded these and computed the average monthly premia in 2007. These were then combined with the estimates of the share of workers in each industry that buy supplement UI from a particular fund to construct an estimate of the cost of insurance for each industry in 2007.[12]

I further define industry level unemployment in year t as the share of workers employed in the industry in year t - 1, who were unemployed in year t. I complement this data with an additional population wide registry Louise, which contains information about earnings and background characteristics such as educational attainment.

Before turning to the main empirical analysis, I briefly analyse the characteristics of those who left supplementary UI as a response to the premia increase. Figure 1.6 and 1.7 in the appendix show that workers above 60 years of age and those who held part-time employment opt out of UI to a much greater extent. There are several reasons for this. Older workers have typically

[10] I define an unemployment spell as en episode that starts with "open" unemployment, during which the individual claim income related UI benefits.

[11] Let n_j denote the number of workers who were employed in industry j in 2005, and who became unemployed in 2006. Moreover, let f_j denote the number of unemployed workers from industry j, who claimed UI benefits from fund f. I then compute the fraction $x_{jf} = \frac{f_j}{n_j}$, and assume that x_{jf} represents the share of workers from industry j who buys insurance from fund f.

[12] Let p_f denote the average monthly premia charged by UI fund f in 2007. Let c_j denote the cost of insurance for industry j in 2007. We then have that $c_j = \sum_{f=1}^{36} x_{jf} * p_f$. Recall that there were 36 UI funds in January, 2007.

attained high job tenure and are subsequently unlikely to be displaced. In addition, UI fund memberships are automatically dissolved once the workers turn 65 and become eligible for retirement benefits. Hence, it is unlikely that older workers will ever claim UI benefits.

Recall from section 3 that workers have to fulfill a labor market attachment criterion to qualify for any type of UI. Part-time workers are at the boundary for fulfilling this criterion. Hence, if they work slightly less for a few months and then become displaced, they will fail to fulfil the labor market attachment criterion and will not qualify for supplement UI, even if they are members of a UI fund. In addition, part-time workers have low earnings. Any given UI-premia increase will therefore constitute a large share of their total income. For these two reasons, one would expected part-time workers to be more sensitive to changes in the price of UI.

Furthermore, Landais et al. (2017) use the same reform and document that displacement risk is a strong predictor of UI choices. In particular, workers with a higher risk of unemployment are more likely to stay in the UI system after the premia hike.

Altogether, this suggest that UI-coverage should decline more in industries that employ a greater share of workers close to retirement, have low displacement risk and employ a large share of part-time workers.[13]

Appendix Table 1.5 shows the cross-industry relationship between the decline in UI-coverage between 2006-2007 and a variety of industry-level characteristics.[14] There is a strong positive correlation between the decline in UI-coverage and the price of supplement UI in 2007, the share of workers that are close to retirement and the share of part-time workers. In particular, the share of part-time employment seams to be a much stronger predictor of the decline in UI-coverage compared to the share of workers that are above 60 years of age.

Table 1.2 collects some additional descriptive statistics of the available sample.[15] The table reports the mean and standard deviation of selected covariates for the three groups in 2005, 2006 and 2007. I focus on this period because the UI-fund-membership data starts in 2005.

Industries with a larger decline in UI-coverage employ slightly older workers who earn relatively low wages. UI coverage prior to the reform was also

[13]Following the sharp decline in UI-coverage, the government mandated the Swedish Unemployment Insurance Board (IAF) to investigate the effect of the premia increase. These reports (IAF, 2007, 2008) also show that those who opt out were primarily older workers, individual that work part-time and those who have low unemployment risk. In addition, there was a group of workers that simply thought UI became to expensive after the reform.

[14]The wage data is collected by Statistics Sweden through 5 different surveys. I always control for the share of observation in each industry that comes from each survey.

[15]The industries have been divided into three groups with a large, medium and small reduction in UI-coverage between 2006 - 2007, corresponding to below 3, between and 3 and 5.4 and above 5.4 percentage points. The threshold were set so that 33% of the industries experienced a change in UI-coverage that were classified as "small", "medium" and "large" respectively.

lower in those industries. However, educational attainment does not vary systemically across industries with small, medium and large changes in UI-coverage. Also, note that UI-coverage is almost unaffected in industries with low reductions, while UI-coverage decline by more than 10% in the most affected industries.

	2005				2006				2007			
	All	Large	Medium	Small	All	Large	Medium	Small	All	Large	Medium	Small
Share who buy UI insurance	0.905	0.885	0.922	0.956	0.895	0.875	0.912	0.950	0.827	0.788	0.864	0.927
	(0.293)	(0.319)	(0.269)	(0.205)	(0.306)	(0.331)	(0.283)	(0.219)	(0.378)	(0.409)	(0.343)	(0.259)
Age	43.16	43.38	43.11	42.25	43.18	43.40	43.16	42.25	43.23	43.34	43.40	42.33
	(11.88)	(12.16)	(11.58)	(11.23)	(12.01)	(12.30)	(11.71)	(11.31)	(12.08)	(12.42)	(11.66)	(11.33)
Log monthly wage	10.05	10.03	10.06	10.16	10.08	10.06	10.09	10.19	10.11	10.08	10.13	10.22
	(0.298)	(0.274)	(0.327)	(0.312)	(0.300)	(0.276)	(0.330)	(0.310)	(0.305)	(0.280)	(0.338)	(0.306)
Years of schooling	13.18	13.27	13.34	12.41	13.24	13.34	13.38	12.48	13.30	13.39	13.46	12.54
	(2.813)	(2.851)	(2.758)	(2.637)	(2.803)	(2.840)	(2.747)	(2.631)	(2.794)	(2.826)	(2.747)	(2.623)

Mean and standard deviation of selected covariates.

Table 1.2. *Characteristics of industries with small, medium and large reduction in UI-coverage.*

1.5 The Impact of UI Benefits on Wages

In this section, I present the main empirical results on the impact of industry-level UI coverage on equilibrium wages. I begin by presenting a transparent difference-in-differences design, that exploits the differential impact of the premia hike in 2007 on the take-up rate of supplement UI. I complement the analysis with a series of placebo exercises to test the "common trends" assumption in the pre-reform period. I argue that any potential bias associated with the difference-in-difference design should lead to an underestimation of the true UI wage effect. The final subsection attempts address the downward bias using an IV-approach.

1.5.1 Difference-in-Difference Estimates

Figure 1.3 presents plots of the change in wages against the reduction in industry level UI-coverage. The industries have been grouped into 17 equally sized bins, based on the decline in industry level UI-coverage following the 2007 premia reform.

There is a clear negative association between wage growth and the reduction in industry level UI-coverage. Furthermore, the right-hand side panel in Figure 1.3 plots the growth in wages between 2005 - 2006 against the change in UI-coverage between 2006 - 2007. There are no signs of differential trends prior to the reform.

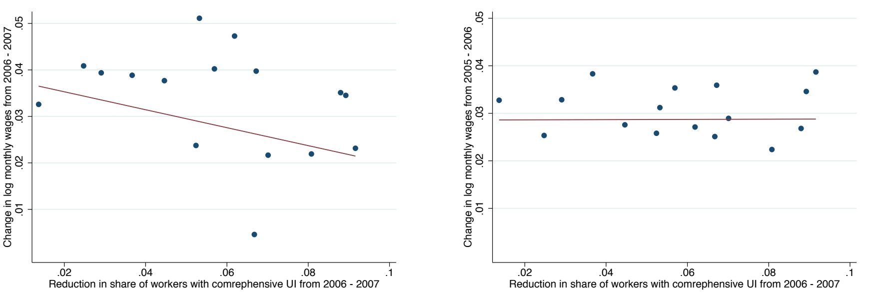

Figure 1.3. Changes in UI-coverage and wage growth

Although suggestive, the results in Figure 1.3 do not entail any formal tests for statistical significance. To investigate this further, I pool data from 2002 - 2007 and estimate the following regression model:

$$y_{ist} = \delta_s + \alpha_1 t_{2002} + ... + \alpha_5 t_{2007} + X'_{ist}\beta_t + \phi_1 t_{2002} d_s + ... + \phi_5 t_{2007} d_s + \varepsilon_{ist} \tag{1.8}$$

Where y_{ist} is the log monthly wage of workers i, employed in industry s in year t, δ_s is an industry-level fixed effect, $t_{2002}, ..., t_{2007}$ are year fixed effects, X_{ist} denotes individual-level controls for age, gender, and education, interacted with the year fixed effects. d_s is the reduction in the share of workers covered by supplement UI in industry s between 2006 - 2007.[16] The coefficients of interest are $\phi_1, ..., \phi_5$. These interaction terms measure whether industries with a larger decline in UI-coverage between 2006 and 2007 experienced weaker wage growth, and if there is any evidence of diverging wage trends in the pre-reform period. I omit year 2006 so that the interaction terms measures differences in wage growth relative to the year prior to the reform.

Figure 1.4 plots the point estimates together with 95% confidence intervals from the baseline specification without covariates. Standard errors are clustered at the industry level.

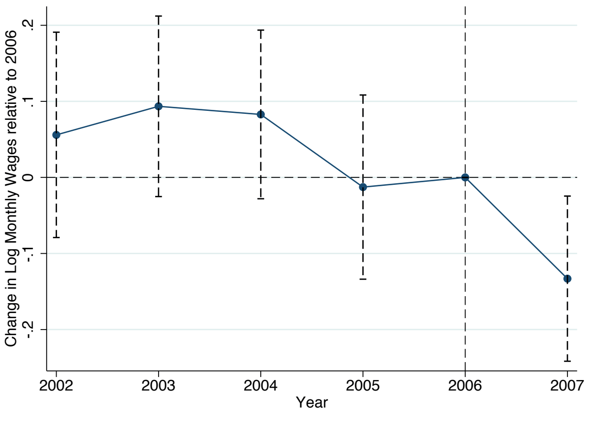

Figure 1.4. Difference-in-Differences estimates

The figure shows that wage growth decline significantly in industries that experienced a sharper reduction in UI-coverage. Moreover, there is no evidence of diverging trends in the pre-treatment period. Importantly, Figure 1.4 shows that, if anything, industries that experienced a sharper decline in UI-coverage had *stronger* wage growth prior to the reform.

Note that the pre-treatment years include a period of increasing (2002 - 2005) and decreasing (2005 - 2006) unemployment (see appendix Figure 1.8). Still, there were no significant differences in wage growth at any point during the pre-treatment period. This suggest that aggregated macroeconomic conditions exert the same effect on wage growth across industries. Moreover, the unemployment rate was smoothly decreasing when the UI-premia reform was implemented. Hence, it is unlikely that the difference-in-difference estimates are confounded by changes in macroeconomic conditions.

[16] $d_s = \frac{I_{s,2006}}{N_{s,2006}} - \frac{I_{s,2007}}{N_{s,2007}}$, where $I_{s,2006}$ and $I_{s,2007}$ are the number of workers in industry s covered by supplement UI in 2006 and 2007 respectively. $N_{s,2006}$ and $N_{s,2007}$ are the total number of workers employed in industry s in 2006 and 2007 respectively.

Table 1.3 presents the results from specification (1.8). The first column represents the most parsimonious specification without controls. In column 2, I add individual-level controls for age, gender and education. Column (3) and (4) show that the results are robust to excluding workers above 60 years of age. This is important since those who are above 60 opt out of UI to a much greater extent. Moreover, as workers start to approach retirement, labor supply and subsequent wages could potentially start to stagnate. Column (3) and (4) show that this does not drive the results. Quantitatively, the results suggest that a 10 percentage point reduction in industry level UI-coverage reduce wages by 1.3 percent.

Although encouraging, this analysis is not completely satisfactory since opting out of UI *is* a choice. Moreover, this decision should clearly depend on factors such as perceived displacement risk. Lay-off risks should therefore, on average, be lower in industries where a greater share of workers opted out of comprehensive UI. Moreover, the unemployment risk will typically be lower in sectors of the economy that are booming. Hence, industries where a greater share of workers opted out of supplement UI should, if anything, have experienced *stronger* wage growth in the absence of the reform. Moreover, the placebo checks supports this conjunction since industries most effected by the reform experienced slightly stronger, but not statistically significant, growth in wages during the pre-treatment period. Altogether this suggests that the difference-in-difference estimates should be interpreted as a lower bound on the true UI-wage effect.

VARIABLES	(1)	(2)	(3)	(4)
Reduction in share with UI*2002	0.056	0.024	0.068	0.023
	(0.069)	(0.053)	(0.071)	(0.054)
Reduction in share with UI*2003	0.093	0.065	0.105	0.066
	(0.061)	(0.065)	(0.064)	(0.068)
Reduction in share with UI*2004	0.083	0.053	0.096	0.058
	(0.057)	(0.055)	(0.058)	(0.058)
Reduction in share with UI*2005	-0.013	-0.019	-0.008	-0.017
	(0.062)	(0.060)	(0.063)	(0.062)
Reduction in share with UI*2007	-0.133**	-0.078*	-0.140**	-0.080*
	(0.055)	(0.046)	(0.056)	(0.046)
Observations	13,198,919	13,147,594	12,381,284	12,341,938
Clustered standard errors	✓	✓	✓	✓
Controls		✓		✓
Excludes workers above 60			✓	✓

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: Standard errors (in parentheses) accounts for clustering at the industry level. The vector of controls include gender, age and educational dummies (primary school, high school and more than high school).

Table 1.3. *Difference-in-difference estimates of the impact of UI-coverage on wages*

1.5.2 Instrumental Variable Estimates

To break the potential endogeneity of UI-coverage in the DID design, I propose an instrumental variable approach. The monthly premia for supplement UI rose differentially across UI funds. In particular, the increase was dictated by a formula written into UI law, which mandated that UI funds with higher unemployment rates increased their premium further. However, the UI funds typically provide insurance to workers from several different industries. Hence, two industries with identical unemployment rates can experience differential premia hikes, if they share UI funds with other industries that have differential unemployment rates.

By controlling for industry level unemployment, I can thus isolate variation in the UI premia that is driven by the unemployment rate in *other* industries. This part of the premia should clearly affect UI-coverage, while the unemployment rates in other industries that buy UI from the same fund are unlikely to directly affect wage growth.

As explain in section 1.3, the UI premia increase was implemented by introducing an additional fee that the UI-funds had to pay to the government each month. This fee was determined through the following formula:

$$A = 240 + (K - G) * 0.0348 \tag{1.9}$$

where A is the fee per employed member (in SEK), K corresponds to the average monthly UI benefit payments per member from the period June - July the preceding year. G is simply the average of K across all (36) UI funds. Hence, the formula mandated that UI funds with higher expenditures on UI pay a larger fee to the government each month. Moreover, the UI funds increased their premiums by an amount that was close to identical to the monthly payments mandated by the formula (IAF, 2007).

This suggests that the price of UI in 2007 should be higher in industries with more unemployment. To confirm this, appendix Figure 1.9 plots the price of UI in 2007 against industry-level unemployment in 2006. There is a strong positive relationship between industry-level unemployment and the price of UI.

Appendix Figure 1.10 shows the distribution of prices for supplement UI across industries in 2007. The variation is fairly small, and most workers face monthly premias between 320 ($\approx$ 40 USD) and 360 SEK ($\approx$ 45 USD). Furthermore, the premia difference between any given industries never exceeds 100 SEK ($\approx$ 13 USD). It is worth pointing out that the reform increased monthly UI-premiums by 220 SEK ($\approx$ 27 USD) on average. This reduced the take-up rate of supplement UI by approximately 10 percentage points. Hence, the price elasticity of demand for UI is very high, which imply that small premia-differences could potentially be used in an IV-strategy.

There are many potential instruments for the reduction in industry level UI-coverage. The most obvious one is the premia for supplement UI in 2007, after controlling for industry-level unemployment in 2006. Alternatively, one could replace the actual price with a predicted price, based on the price of UI in 2006 and the formula in equation (1.9).

Finally, one could ignore the UI-premia differentiation, and simply leverage the distribution of price-sensitive groups across industries. Section 1.4 showed that workers who hold part-time employment are particularly sensitive to changes in the price of UI. An additional approach is therefore to instrument changes in UI-coverage with the share of workers that held part-time employment the year prior to the reform. This instrument will be stronger and have higher precision, which could be important since the UI-premia variation is fairly small. On the other hand, the exclusion restriction becomes harder to justify. I will therefore consider three instruments for the reduction in industry level UI-coverage:

- $\mathrm{IV}_{1,s}$: Price of UI in 2007 for workers in industry s
- $\mathrm{IV}_{2,s}$: Predicted price of UI in 2007 for workers in industry s
- $\mathrm{IV}_{3,s}$: Share of workers part-time employed in industry s in 2006

I construct the predicted price of UI in 2007 as follows:

$$\mathrm{IV}_{2,s} = \mathrm{P}_{2006,s} + \mathrm{F}_s \tag{1.10}$$

where $\mathrm{P}_{2006,s}$ is the price of UI workers in industry s faced in 2006, F_s is the price increase mandated by the formula in equation 1.9.[17] The government agency IAF was responsible for determining the fees that the UI-funds had to pay to the government each month. The fees were determined on a yearly basis and are publicly available. I use the fees computed by the IAF to construct F_s.

To implement the instrumental variable strategy, consider the following regression specification:

$$y_{ist} = \delta_s + \alpha \times d_{2007} + X_s' \times \beta + \phi \times d_{2007} \times d_s + \varepsilon_{ist} \tag{1.11}$$

Where y_{ist} is the log monthly wage of worker i, employed in industry s in year t, δ_s is an industry-level fixed effect, d_{2007} is a dummy variable for 2007, X_s is a vector of industry-level controls, such as the unemployment rate. Importantly, all covariates in X_s are measured in 2006. The variable d_s correspond to the reduction in the share of workers covered by supplement UI in industry s between 2006 - 2007.[18] The potentially endogenous variable of interest is

[17] As usual, I weight the mandated price increase with the estimates of the share of workers in any given industry that buy UI from a particular fund.

[18] $d_s = \frac{I_{s,2006}}{N_{s,2006}} - \frac{I_{s,2007}}{N_{s,2007}}$, where $I_{s,2006}$ and $I_{s,2007}$ are the number of workers in industry s covered by supplement UI in 2006 and 2007 respectively. $N_{s,2006}$ and $N_{s,2007}$ are the total number of workers employed in industry s in 2006 and 2007 respectively.

$d_{2007} \times d_s$. This interaction term measure whether industries with a sharper reduction in UI-coverage experienced weaker wage growth between 2006 and 2007.

I pool data from 2006-2007 and estimate equation (1.11) with two stage least squares (2SLS), using one of my three instruments at a time. The 2SLS estimates of the impact of industry level UI-coverage on wages are shown together with 95% confidence intervals in Figure 1.5. First stage F-statistics are reported together with the point estimates. Standard errors are clustered at the industry level.

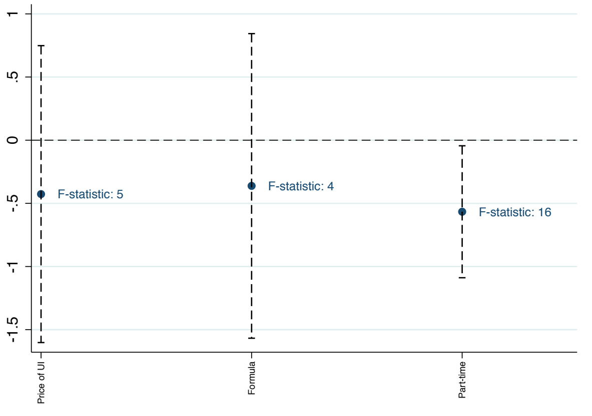

Figure 1.5. IV estimates of the impact of UI-coverage on wages

Two things are immediately clear from Figure 1.5. First, the estimated UI-wage effects are unilaterally larger than those obtained from the DID strategy. Indeed, the IV estimates oscillates around -0.5. By contrast, the DID estimates was about -0.1. This supports the conjecture that DID estimates should be interpreted as a lower bound on the true UI-wage effect.

Second, the specification that exploit the actual and predicted price of UI are similar. However, these estimates are not statistically significant. This is clearly the result of a fairly weak first stage relationship between the instruments and industry level UI-coverage. From Figure 1.5, it is clear that this results in low precision. Instrumenting changes in UI-coverage with the share of workers that held part-time employment in 2006 substantially strengthens the first stage relationship and provides large gains in precision. Furthermore, the UI-wage effect is statistically significant at conventional levels in these specifications.

Table 1.4 presents the results from specification (1.11). It is interesting to note that the point estimates do not change much when the distribution of part-time employment is used as an instrument. Qualitatively, the only difference is that precision improves and that a zero effect can be ruled out at the 5% level.

This is reassuring since the exclusion restriction is easier to justify in specification that only leverages UI-premia variation (controlling for industry level unemployment). The fact that implied effect sizes are qualitatively the same across specifications lends additional credibility to the part-time employment instrument.

To further validate the use of part-time employment as an instrument, I pool data from 2002 - 2007 and estimate the following regression model:

$$y_{ist} = \delta_s + \alpha_1 t_{2002} + ... + \alpha_5 t_{2007} + \phi_1 t_{2002} z_s + ... + \phi_5 t_{2007} z_s + \varepsilon_{ist} \quad (1.12)$$

Where y_{ist} is the log monthly wage of worker i, employed in industry s in year t, δ_s is an industry level fixed effect, $t_{2002}, ..., t_{2007}$ are year fixed effects, z_s is the share of workers in industry s that held part-time employment in 2006. The coefficients of interest are $\phi_1, ..., \phi_5$. These interaction terms measure whether industries that had a greater share of workers with part-time employment in 2006 experienced weaker wage growth between 2006 - 2007, and if there is any evidence of diverging wage trends in the pre-reform period. I omit year 2006 so that the interaction terms measures differences in wage growth relative to the year prior to the reform.

Appendix Figure 1.11 plots the point estimates together with 95% confidence intervals. Standard errors are clustered at the industry level. There is no evidence of differential wage trends prior to the reform. However, wage growth decline significantly in industries with more part-time employment between 2006 and 2007.[19] These results further validates using the distribution of part-time employment as an instrument for changes in industry level UI-coverage. Altogether, the IV-strategy suggest that a 10 percentage point reduction in industry level UI-coverage reduce wages by about 5%.

[19]In specification 1.11, I always control for the share of workers employed by the county council, government and municipality, share of white and blue collar workers in the private sector and the share of workers above 60 years of age. If I include these controls in specification 1.12, one placebo estimate becomes positive and statically significant.

	(1)	(2)	(3)	(4)	(5)	(6)
Reduction in share with UI	-0.227	-0.427	-0.175	-0.362	-0.514**	-0.567**
	(0.534)	(0.600)	(0.547)	(0.615)	(0.249)	(0.266)
	First stage coefficients					
Price	0.007**	0.007**				
	(0.003)	(0.003)				
Predicted price			0.007**	0.006**		
			(0.003)	(0.003)		
Part time					0.010***	0.010***
					(0.003)	(0.003)
First stage F-statistic	5.86	4.85	5.13	4.17	16.77	15.74
Control for unemployment		✓		✓		✓

Note: Standard errors (in parentheses) accounts for clustering at the industry level. Each specification pool data from 2006 and 2007. I always control for the share of workers employed by the county council, government and municipality, share of white and blue collar workers in the private sector and the share of workers above 60 years of age. All controls are measured in 2006 and interacted with a 2007 dummy. The specifications that exploit the predicted and actual UI-premia as instruments control for the share of workers that work part time or less. The excluded instruments are always standardized and interacted with a 2007 dummy.

Table 1.4. *IV estimates of the impact of UI coverage on wage formation*

1.5.3 Direct effect of UI premium

The UI premia reform was intended to make UI actuarially fair and provide incentives for UI funds to tighten monitoring of benefit recipients. However, the reform was also designed to incentivise unions with high unemployment rates to be more restrictive in wage negotiations. Specifically, if they bargained for higher wages and increased unemployment, this would hurt their employed members by driving up the premiums that they had to pay for supplement UI. This is clearly important since such an effect would immediately imply that the UI-premia instrument fails to fulfil the exclusion restriction.

However, several prominent economists pointed out that these potential incentives effects were trivial (Calmfors et al., 2011). They also stressed the fact that one UI fund typically provide UI coverage to *many* industries. The unemployment rate within each industry will therefore have a trivial effect on the overall unemployment rate in the UI fund. Hence, the impact of the unemployment rate within any particular industry on the UI premium will be small. In addition, bargaining at the establishment level is a crucial component of wage formation in Sweden and displacement rates at the firm level will obviously have an even smaller impact on the overall unemployment rate in the UI fund, and thus influence the UI premiums even less. I will therefore assume that any impact of the reform on wages operates through the reduction in supplement UI coverage, and that the introduction of the new premia did not have any direct effect on wages.

1.6 Interpretation and Relation to Existing Literature

In this section, I interpret my findings through the lens of the DMP model with Nash bargaining. Recall from section 1.2 that the equilibrium wage is given by:

$$w = (1-\gamma)b + \gamma(y + c\theta(b)) \tag{1.13}$$

where w is the equilibrium wage, γ represents worker bargaining power, b are unemployment benefits, y is the instantaneous flow of output produced by the worker, c is the vacancy positing cost and θ represent labor market tightness.[20]

In the previous section, I estimated the impact of industry-level UI coverage on wages using a DID and an IV approach. I argued that the difference-in-difference estimate should be interpreted as a lower bound on the true UI-wage effect. Hence, I choose the IV estimates as my preferred specification.

These results indicate that 10 percentage point reduction in industry-level UI coverage reduce wages by about 5 percent. If we are willing to extrapolate it, this estimate implies that if all workers are covered by the comprehensive

[20]Recall that labor market tightness is defined as the number of vacancy's, v, divided by the number of unemployed workers, u. Hence, $\theta = \frac{v}{u}$.

plan, wage are 50% higher compared to the scenario were no one buys supplement UI. Moreover, if we let b_1 and b_0 denote the UI benefits available to workers under the basic and comprehensive plan respectively, equation 1.13 imply that the associated equilibrium wages can be expressed as:

$$w_0 = (1-\gamma)b_0 + \gamma(y + c\theta(b_0)) \tag{1.14}$$

$$w_1 = (1-\gamma)b_1 + \gamma(y + c\theta(b_1)) \tag{1.15}$$

Where w_1 and w_0 are the equilibrium wages associated with the hypothetical scenarios where all or no workers are covered by the comprehensive plan. Now, subtract 1.14 from 1.15 and divide through by w_0 and we get:

$$0.5 = (1-\gamma)\frac{b_1 - b_0}{w_0} + \gamma\frac{c}{w_0}(\theta(b_1) - \theta(b_0)) \tag{1.16}$$

Where I used that my estimate of the UI wage effect imply that $\frac{w_1 - w_0}{w_0} = 0.5$. Furthermore, the DMP model predicts that $\theta(b_1) \leq \theta(b_0)$. This is simply because wages are either completely unresponsive to changes in UI ($\gamma = 1$), in which case $\theta(b_1) - \theta(b_0) = 0$. Otherwise, changes in UI exerts upward pressure on wages, and vacancy creation goes down. In this case, $\theta(b_1) - \theta(b_0) < 0$. There is however no scenario where $\theta(b_1) - \theta(b_0) > 0$. In addition, the range of estimated general equilibrium effects of UI (Chodorow-Reich and Karabarbounis, 2016; Hagedorn et al., 2013; Marinescu, 2017) imply either no effect, or large negative effects of UI on θ. Hence, it is safe to assume that $\theta(b_1) - \theta(b_0) \leq 0$.

Moreover, vacancy posting cost (c) and the counterfactual wage (w_0) are clearly positive. We can thus define $\alpha = \frac{c}{w_0}(\theta(b_1) - \theta(b_0)$, where $\alpha \leq 0$. Substitute into 1.16 and we obtain:

$$0.5 = (1-\gamma)\frac{b_1 - b_0}{w_0} + \alpha\gamma \tag{1.17}$$

UI benefit duration under the basic and comprehensive plan is capped at 300 days. Moreover, the daily allowance is paid out 5 days per week under both insurance plans. Hence, the only difference between basic and comprehensive UI is the daily benefit level.

Moreover, the vast majority of workers have pre-unemployment earnings that are significantly higher than the benefit ceiling. Recall from section 1.3 that approximately 70% of displaced workers have pre-unemployment earnings for which the benefit ceiling is binding. Moreover, displaced workers typically earn relatively low wages. The total share of workers (unemployed or employed) with earnings above the ceiling is presumably much higher. I will therefore make the simplifying assumption that all workers receive the maximum daily benefit level if they are covered by supplement UI.

Hence, workers receive daily benefits of 680 SEK ($\approx$ 75 USD) under the comprehensive plan and 320 SEK ($\approx$ 35 USD) under the basic plan. Under the simplifying assumptions discussed above, this is the only difference between the insurance plans. This imply that we can write $b_1 \approx 2.125 b_0$. Worker bargaining power can thus be expressed as:

$$\gamma = \frac{\frac{1.125 b_0}{w_0} - 0.5}{\frac{1.125 b_0}{w_0} - \alpha} \tag{1.18}$$

I further compute the average wage (25260 SEK) and the UI coverage rate (90%) for my sample in 2006. We can once again use the estimated UI wage effect to compute the counterfactual wage in 2006, had no worker been covered by the comprehensive plan. This is easily seen to be (1 - 0.45) $\times$ 25260 = 13893 SEK. Now, unemployed workers covered by basic UI receive 320 SEK per day. These are paid per "working day", which means that there are 5 days of benefits paid per week. Hence, basic UI correspond to a monthly income of 7040 SEK. Substitute $w_0 = 13893$ and use $1.125 b_0 = 7920$ and we get:

$$\gamma \approx \frac{0.071}{0.57 - \alpha} \tag{1.19}$$

Clearly, the upper bound on γ is obtained by setting $\alpha = 0$, in which case we have $\gamma = 0.12$. Furthermore, as α become arbitrarily large in absoulte terms, γ approaches zero. Hence, $\gamma \in (0, 0.12)$.

Schoefer et al. (2018) argue that estimated rent-sharing elasticity's imply that worker bargaining power can be at most 0.2. They further estimate the effect of non-employment values on wages, and argue that their estimates cannot be rationalized by bargaining models with unemployment as the outside option unless one is willing to assume that workers wield almost full bargaining power ($0.95 \leq \gamma$). They conclude that Nash bargaining with unemployment as the outside option for workers is an inappropriate model for wage setting, and that more promising models insulate wages from the value of non-employment.

My results challenge this conclusion. As shown above, my estimates imply an upper bound on worker bargaining power of 0.12. This is clearly lower than the upper bound target by Schoefer et al. (2018). Hence, my result suggests that wage setting protocols should *not* insulate wages from the value of non-employment. It is interesting to consider why my results are so different from the those in Schoefer et al. (2018).

My interpretation is that the results differ because we use variation in UI-benefit entitlement that occurs at different levels. As I mentioned in the introduction, Schoefer et al. (2018) rely on four reforms that changed UI-benefit entitlement for workers in particular parts of the earnings distribution. However, union-affiliation does not map one-to-one with any particular part of the earnings distribution, which imply that treatment does not vary at a level that can capture wage-adjustments that occur through collective bargaining. I rely on variation in UI-entitlement at the industry-level, where collective bargaining occurs, which could potentially explain why I reach different conclusions.

1.7 Conclusion

Conventional wage setting protocols in macroeconomics and labor economics typically posits a tight relationship between the generosity of UI and wages. In the standard DMP model, the response of unemployment to changes in UI-policy is driven by the UI-wage-pressure channel. Hence, the UI wage effect has important policy implications. There is however surprisingly little credible evidence on this issue, and the sensitive of wages to changes in UI is largely an unresolved issue.

In this paper, I find strong evidence that UI benefits exerts upward pressure on equilibrium wages. I exploit the sudden increase in the premium for supplement UI, following the election of the right-wing coalition. I studied the impact of UI-coverage on equilibrium wages using a difference-in-difference approach, and argued that any potential bias should lead to underestimation of the true UI wage effect. In a second step, I validated this conjecture using an IV approach. The results unilaterally suggest that industry level UI-coverage exerts upward pressure on equilibrium wages.

Finally, I used the estimated UI wage effect to derive information bounds on the worker bargaining power parameter in a simple DMP model. My results suggest that it can be at most 0.12. Altogether, this evidence supports wage setting mechanisms that tie wages to the generosity of UI.

1.8 Appendix

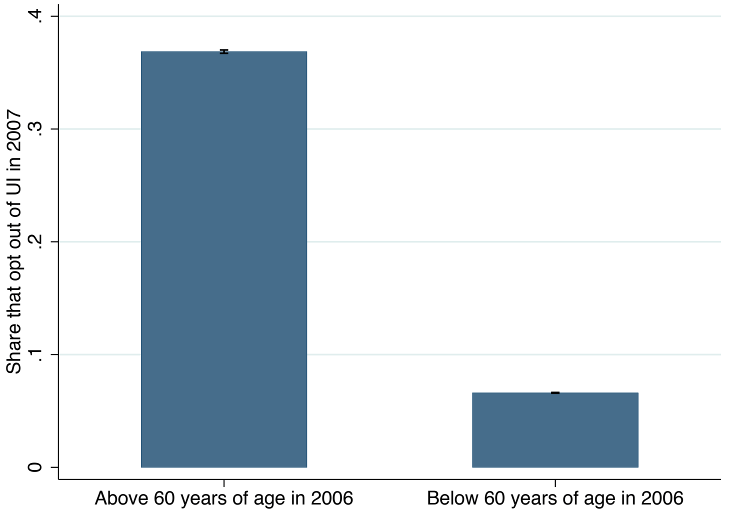

Figure 1.6. Share of workers above and below 60 years of age that opt out of UI in 2007

Note: The figure shows the share of workers that were insured in 2006 but uninsured in 2007 separately for workers that were above or below 60 years of age in 2006.

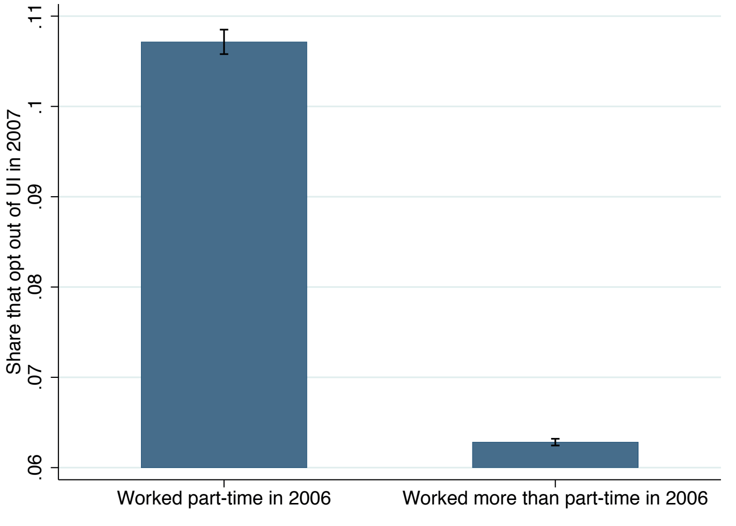

Figure 1.7. Share of part-time and full-time workers that opt out of UI in 2007

Note: Figure 1.7 shows the share of workers that were insured in 2006 but uninsured in 2007 separately for worked part-time or more than part-time in 2006. Part-time employment is defined as working part-time or less.

Determinants of the reduction in industry level UI coverage

	(1)	(2)	(3)	(4)
VARIABLES				
Price of supplement UI in 2007	0.004**	0.003	0.004	0.005**
	(0.002)	(0.002)	(0.003)	(0.002)
Industry-level unemployment rate in 2006		0.216	0.179	0.028
		(0.152)	(0.161)	(0.141)
Share above 55 years of age			0.095	0.159*
			(0.114)	(0.085)
Share who work part time				0.158***
				(0.042)
R-squared	0.938	0.939	0.940	0.952

Note: Standard errors are reported in parentheses. Each column represents a separate regression of the reduction in the share of workers covered by supplement UI on industry-level characteristics in 2006 . All specifications control for the share employed by government, county council, municipality and share of white and blue collar workers in the private sector. The price of supplement UI has been standardized such that the mean in zero and the variance is one.

Table 1.5. *Determinants of the reduction in industry level UI-coverage.*

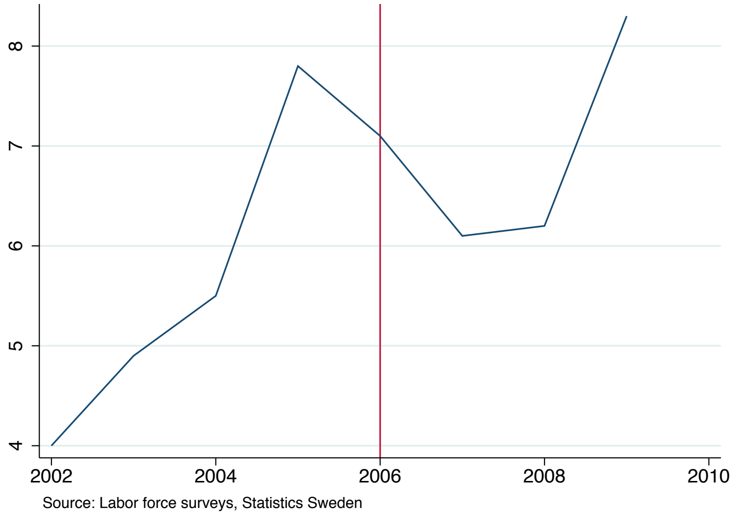

Figure 1.8. Unemployment rate

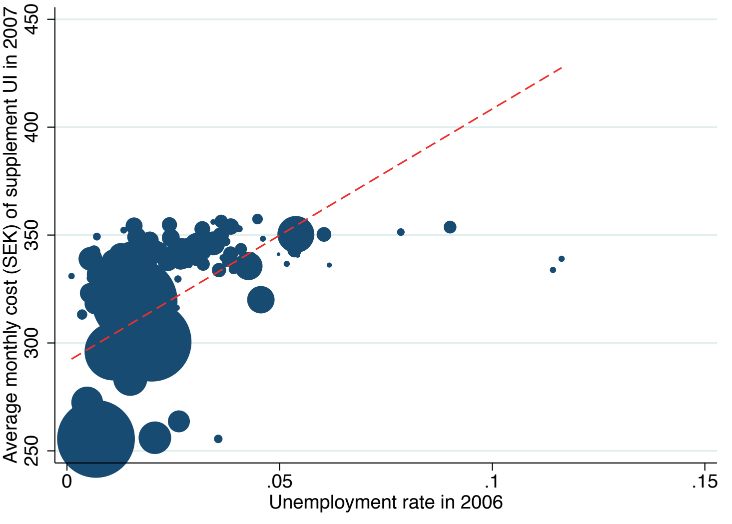

Figure 1.9. Industry level unemployment and the price of comprehensive UI

Note: The figure plots the relationship between the average monthly premia for supplement UI in 2007 and industry level unemployment in 2006. The dashed red line shows the regression coefficient from a weighted regression with the number of workers employed in the industry in 2006 as weights.

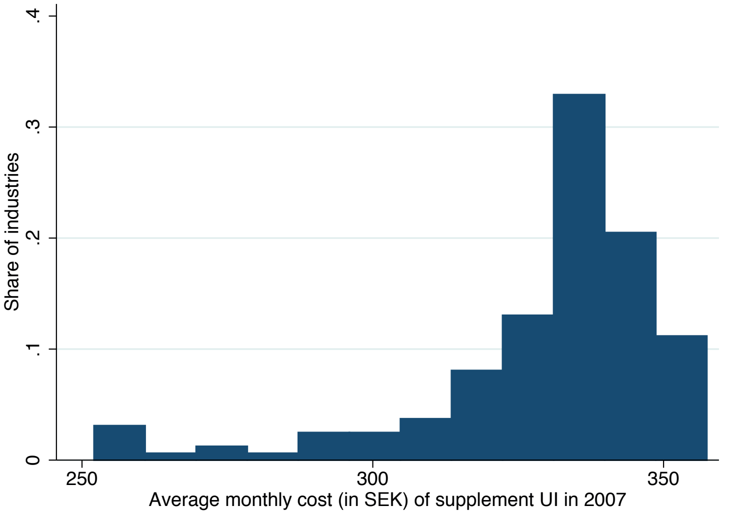

Figure 1.10. The distribution of the monthly premia for UI in 2007

Note: Figure 1.10 illustrates the distribution of the average monthly cost of UI across industries in 2007.

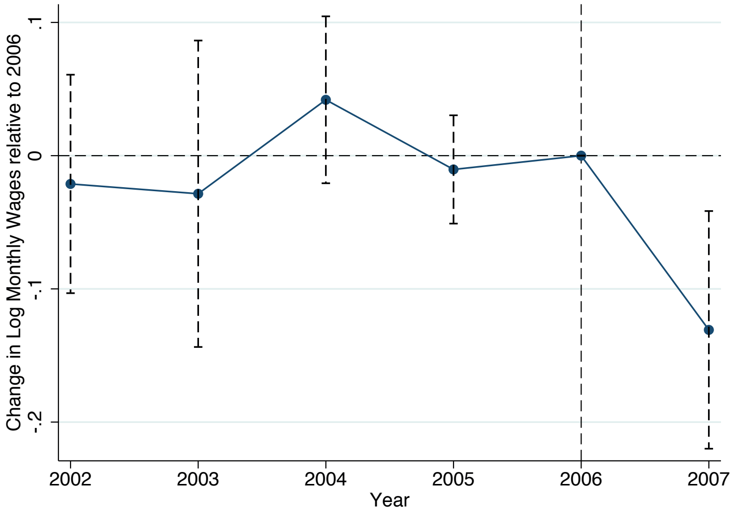

Figure 1.11. Wage trends and part-time employment

Note: Figure 1.11 shows the difference-in-difference estimates from a model that pools data from 2002 - 2007 and interacts the year fixed effects with the share of workers that held part-time employment in 2006. I omit year 2006 so that the interaction terms measure difference in wage growth relative to the year prior to the reform.

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2. Unemployment Insurance and Youth Labor Market Entry

2.1 Introduction

Labor market entry is a high stakes setting for young workers. Those who become unemployed in the school-to-work transition suffer persistent earnings penalties (Gregg and Tominey, 2005; Bell and Blanchflower, 2011; Mroz and Savage, 2006) and continue to have lower attachment to the labor market later on (Gregg, 2001; Nordstrom Skans, 2011). In addition, the school-to-work transition determines the type of jobs school leavers are matched to, and workers who hold positions that they are overeducated for obtain a significantly lower education-earnings-premium (Leuven and Oosterbeek, 2011).[1]

Access to unemployment insurance (UI) benefits could potentially smooth the school-to-work transition, since some of the financial pressure to find a job immediately is alleviated. If young workers have access to UI benefits they can reject job offers of poor quality and focus on finding employment that match their education, which could lead to higher wages, improved career prospects and increased employment stability. On the other hand, there is a clear risk that UI benefits disincentivize job search and induce school leavers to stay unemployed longer. Quantifying the potential trade-off between longer unemployment durations and improved match quality has important implications for the design of UI-policy.

In this paper, I study the effects of UI-benefits on job finding rates and entry level wages for unemployed high school leavers. My empirical strategy is to exploit age-discontinuities in the Swedish UI-system. Between 2002 and 2006, all unemployed high school leavers became entitled to UI once they turned 20 years. Moreover, Swedish high school student typically graduate in spring the year they turn 19 years. Therefore, the start of an unemployment spell relative to the 20:th birthday creates plausibly exogenous variation in the duration until they become eligible for UI.

The experiment I have in mind can be described as follows. Consider two groups of high school students that graduates the year they turn 19 years. They are unable to find employment and register as unemployed in October. Now, assume that the first group is born in January and the second in February. This imply that no one can claim UI between October - December (since they are all 19 years of age at the time). However, in January, the first group turns

[1] Overeducated is defined as having more schooling that the job require.

20 years and become eligible for UI. My empirical strategy is to compare the employment hazards in January, when the first group can claim UI but the second one cannot.

I generalize this analysis, and match each group to a comparison cohort that become unemployed at the same time, but are born one month later and therefore have to wait an additional month until they become eligible for UI. Again, the idea is to compare the employment hazards in the period when the first group can claim UI but the comparison group cannot.

The analysis builds on the econometric framework pioneered by Van den Berg et al. (2010). They show that such comparisons identify a causal effect of the treatment on the employment hazard under two conditions. First, the treatment and control group have to be comparable when they enter unemployment. Second, the dynamic selection has to be identical until treatment is imposed. This is achieved by imposing the "no anticipation" assumption, i.e that future UI-benefit eligibility does not effect current job search behaviour. This assumption would be violated if, for instance, unemployed high school leavers adjust their job search behaviour in anticipation of future UI-benefit payments.

I begin the analysis by validating the identifying assumptions through a series of falsification tests. First, I implement balancing tests and verify that the distribution of important covariates, such as high school grades, are balanced across treatment and control groups at the start of the unemployment spells. To validate the "no anticipation" assumption, I implement a series of placebo checks that test for differences in the employment hazards in the pretreatment period, when neither group can claim UI. Overall, 5.3% of the placebo tests are significant at the 5% level, which is expected by chance.

I proceed and estimate the effect of UI benefits on the employment hazard. Variation in time to treatment for individuals born at different points of the year allows me to explore how the effect of UI benefits vary depending on the elapsed duration that payments are made. The estimates suggest that UI-benefits reduce job finding rates, but the effects does vary depending on the waiting time until payments are made.

To gain precision, I pool all of the available treatment-control group comparisons and estimate them jointly. The results indicate that becoming eligible for UI benefits reduce monthly job finding rates by 1 percentage point. The baseline hazard is roughly 8%. Hence, the treatment effect translates into a 12.5% reduction in the conditional probability of leaving unemployment.

In the final part of the paper, I leverage the same treatment-control group comparisons and estimate the effect of UI-benefits on entry level wages. There are no detectable effects of UI on entry level wages. Altogether, these results suggest that UI-benefits induce high school leavers to postpone labor market entry, but does not seem to affect job match quality.

There is a large literature on the school-to-work transition (Kramarz and Skans, 2014; Hensvik and Nordström Skans, 2013; Wolpin, 1987; Semeijn et al., 2005; Scherer, 2004; Neumark and Wascher, 1995; Raaum and Røed, 2006). However, evidence on the effects of UI benefits in the school-to-work transition is very scarce. To the best of my knowledge, Cockx and Van Belle (2016) is the only exception. They exploit age discontinuity's in the waiting period before school-leavers qualify for UI in Belgium, and find no effects on job-finding rates and post unemployment wages. There are at least two important differences between Cockx and Van Belle (2016) and this paper. First, they study the effects of extending the waiting period for UI. I study the effects of receiving UI-benefits, which is clearly different. Second, they restrict the sample to individuals at least a bachelors degree, whereas I consider high school graduates. Therefore, the population of interest in this paper are younger, have lower levels of education and less experience on the labor market.

Furthermore, this paper is the first application of the econometric framework pioneered by Van den Berg et al. (2010). This method has some attractive features. Existing methods for studying the effects of a policy change on the hazard rate rely heavily on untestable model structure. In particular, duration models that allow for unobserved heterogeneity typically impose a mixed proportional hazard structure to achieve identification. Such semi-parametric assumptions may be unappealing. The econometric framework that I use does not impose any model structure. On the other hand, the method is designed for studying short run effects, which has some important drawbacks. Importantly, it is not well-suited for studying the effects of prolonged exposure to UI.

The rest of this paper is organized as follows. The next section describe the institutional details. Section 2.3 presents the data sources and perform some descriptive analysis. The empirical approach is outlined in section 2.4. Section 2.5 test the identifying assumptions. Section 2.6 and 2.7 presents the main empirical results. Section 2.8 concludes.

2.2 Institutional Framework

2.2.1 UI and the Schooling System

In Sweden, UI is typically restricted to workers who become unemployed after a sufficiently long-lasting job.[2] School-leavers who become unemployed in the school-to-work often fail to fulfil this condition. Motivated by this, the Swedish government introduced a special set of UI-rules in 1998. These rules exempted unemployed school-leavers from the labor market attachment criterion.[3] Hence, those who became unemployed in the school-to-work transition were eligible for UI, even if they had never worked.

[2]The labor market attachment criteria restricts UI to workers who have been employed for at least 6 out of the last 12 months prior to displacement.

[3]School leavers is defined as graduating from high school or university.

School-leavers were entitled to flat rate benefits. These are paid per "working day", which means that there were 5 days of benefits paid per week. The benefit level was revised in 2002 and the rules were abolished altogether in 2007. To keep the policy regime constant, I restrict attention to the years 2002 - 2006, when the benefit level remained at 320 SEK per day. The benefit level prior to 2002 was 270 SEK per day.

Unemployed school-leavers qualified for flat rate UI if two conditions were met: (i) they were 20 years of age and (ii) they fulfilled a 90 day waiting period. The waiting period starts when the individual register as unemployed at the Public Employment Service (PES).

The Swedish educational system is tuition free at all levels. Children typically start school the year they turn 7 years and are required by law to complete 9 years of compulsory schooling. Afterwards, about 98% choose to enrol in upper secondary school. Here, the educational system is separated into academic and vocational tracks.[4] Academic tracks are intended for students who plan to pursue further education at the university level. Students enrolled in vocational tracks typically enter the labor market immediately after high school. The vocational tracks contain specializations such as construction and nursing. Secondary school lasts for 3 years and students typically graduate in spring the year they turn 19 years.

2.2.2 Potentially Confounding Policies

My identifications strategy consists of exploiting age-discontinuity's in the Swedish UI system. In particular, I exploit that school leavers become eligible for UI once they turn 20 years. Clearly, this identification strategy require that there are no other policy discontinuity's around this threshold. Two Active Labor Market Programs (ALMP) are potential confounders.

ALMP:s in Sweden is typically restricted to individuals who are at least 20 years of age. However, some municipalities offer municipality youth programs (MYP) to job seekers between 18 and 20 years of age. The municipalities offer these on a voluntary basis and the programs typically involve a small monetary compensation,[5] education and worker practice, although the precise content varies across municipalities.

Once the unemployed school leaver turn 20 years, they become eligible for other ALMP:s, such at the "youth guarantee" (YG). The YG was also administrated by the municipalities on a voluntary basis, but provision was much more restrictive compared to the MYP (Forslund and Sibbmark, 2005).

[4] There are also preparatory tracks for high school.

[5] Those who failed to complete high school received a monthly allowance of 1360 SEK ($\approx$ 150 USD). The monthly compensation for those with a completed high school education varied across municipalities. This information is not publicly available

Therefore, there is a discontinuity in ALMP policies once job seekers become 20 years of age. They lose access to the MYP and become eligible for the YG. I will address this in a sensitivity analysis in section 2.6.

2.3 Data, Sample Selection and Descriptive Statistics

2.3.1 Data

This paper uses data from several Swedish administrative registries. The first registry contains yearly information on enrolment in secondary school between 1995 and 2010. I use the last observed registration to identify the year of graduation.

The dataset also contain information about the high school track that students were enrolled in. Some of the cannot be classified as academic or vocation. I label these as "other high school program".

I add data on grades from compulsory school. These are more informative about the ability of students compared to grades from high school, since the curriculum is identical for all students in primary school.

I further add data on unemployment outcomes from the Swedish Public Employment Service. It covers the universe of unemployment spells between 1990 and 2015. Entry and exit from unemployment are recorded at the daily level. There is also detailed information on labor market program participation and the reason for terminating the unemployment spell (regular or subsidized employment, lost contact and so on). I complement this data with information on UI-benefit spells from 1999 onwards from the registry ASTAT. ASTAT records all UI benefit payments at the daily level.

I add data from the matched employer-employee registry RAMS. This data reports monthly earnings for all individuals employed in Swedish firms from 1985 to 2015. If an employment spell is observed the year after exit from unemployment, I take this as the post unemployment outcome. If an individual has several employment spells, I focus on the spell with the highest monthly wage.

2.3.2 Sample

I restrict the sample to individuals who leave high school the year they turn 19 years, and left between 2002 and 2005. The sample is further restricted to those who did not receive any income related UI benefits during their unemployment spell. This restriction is imposed since those who have fulfilled the requirements associated with income-related UI benefit can claim benefits prior to their 20:th birthday. Consequently, their treatment status do not change when they become 20 years of age.

The empirical strategy is to exploit age-discontinuity's in the Swedish UI-system. In particular, I make use of the fact that the elapsed duration at which school leavers qualify for UI is determined by the start of an unemployment spell relative to their 20:th birthday. The empirical strategy is to match individuals born in a given month to a comparison cohort that become unemployed at the same time, but are born one month later, and therefore have to wait an additional month before they qualify for UI. To estimate the effect of UI on job finding rates, I compare the employment hazards in the month when the first group can claim UI while the control group cannot.

Therefore, I define the running variable as follows:

$$s_i = t_{1,i} - t_{0,i} + 1 \tag{2.1}$$

where s_i is the value of the running variable for individuals i, $t_{1,i}$ is the calender month in which individual i become 20 years of age and $t_{0,i}$ is the calender month when the unemployment spell begins. Hence, the running variable is defined as the number of calender months between the start of an unemployment spell and the 20:th birthday. Adding 1 to the right hand side of equation (2.1) ensures that the calender month when the spell start is counted as a full month of unemployment. To ensure that all subjects are eligible for UI benefits when they become 20 years of age, I restrict the sample to those who enter unemployment at least 3 calender months prior to their 20:th birthday. Put differently, I restrict the sample to spells where the running variable is at least equal to 3. If an individual had several spells that satisfied these requirements, I restricted attention to the first spell. The sample size is reduced to 132 observation for values of the running variable greater than 13 calender months. I exclude these and restrict attention to spells where the running variable ranges from 3 to 13. The final sample contains 58641 unemployment spells.

2.3.3 Descriptive Statistics

Descriptive statistics of the sample are collected in appendix Table 2.4. The bottom row shows the mean and standard deviations of selected covariates for the entire sample. The other rows report descriptive statistics for selected covariates separately for each value of the running variable. Note that grades from primary school have been standardized in the full population such that the mean is zero and the standard deviation is equal to one. Overall, those who enter unemployment have significantly lower grades than their peers. There is a fairly even distribution of graduates from vocational and academic programs. Moreover, the share of graduates from programs that cannot be classified as academic nor vocational is low (8%). The sample consists of approximately 50% males and females respectively.

It is interesting to note that grades from primary school are smoothly decreasing with higher values of the running variable. This is most likely driven by school starting age effects. Specifically, individuals with larger values on the running variables will, on average, be born later in the year. Moreover, individuals born later in the year typically start school when they are a bit younger.

Therefore, they are often outperformed by their older peers, and these effects have been shown to persist in the labor market (Fredriksson and Öckert, 2014).

Figure 2.1 show the share of unemployed job-seekers who claim UI-benefits over the unemployment spell. Take-up rates are shown separately for cohorts with different values on the running variable, s. There are clear spikes in the share of individuals who claim UI-benefits the calender month after they become 20 years of age. However, the take-up rate is remarkably low. Only about 6% of unemployed school-leavers claim UI-benefits the calender month after they turn 20 years. This is because individuals are only eligible to apply for UI once they become 20 years of age. Those who apply before their 20:th birthday are automatically rejected and have to wait until they are 20 years before they can submit a new application. Moreover, case workers at the UI-fund have to review and approve the application before any payments are made. This process can take several months. In addition, some unemployed school-leavers may be unaware that they are eligible for UI. This explains the low initial take-up rate.

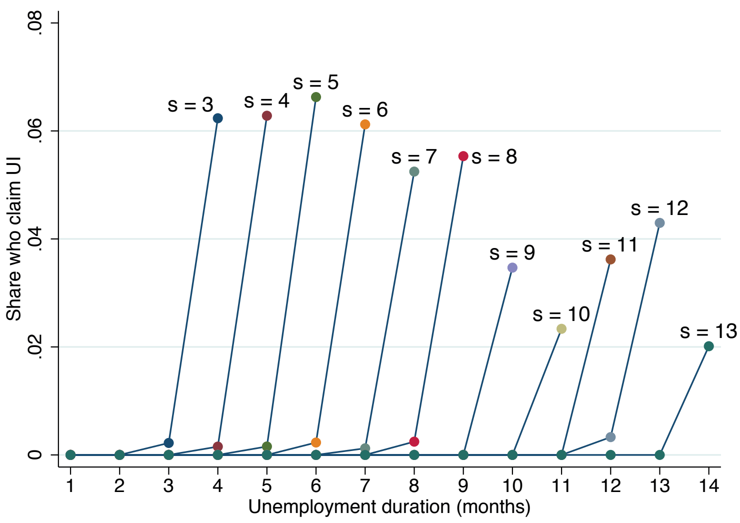

Figure 2.1. UI take-up effect

Note: The figure shows the take-up rate of UI over the unemployment spell for individuals with different values of the running variable. The running variables is labeled s.

2.4 The Empirical Approach

Figure 2.2 illustrates the approach to estimating the effect of UI on the employment hazard. Cohort 1 are marginally older at the start of the unemployment spell. They become 20 years at an elapsed duration s.

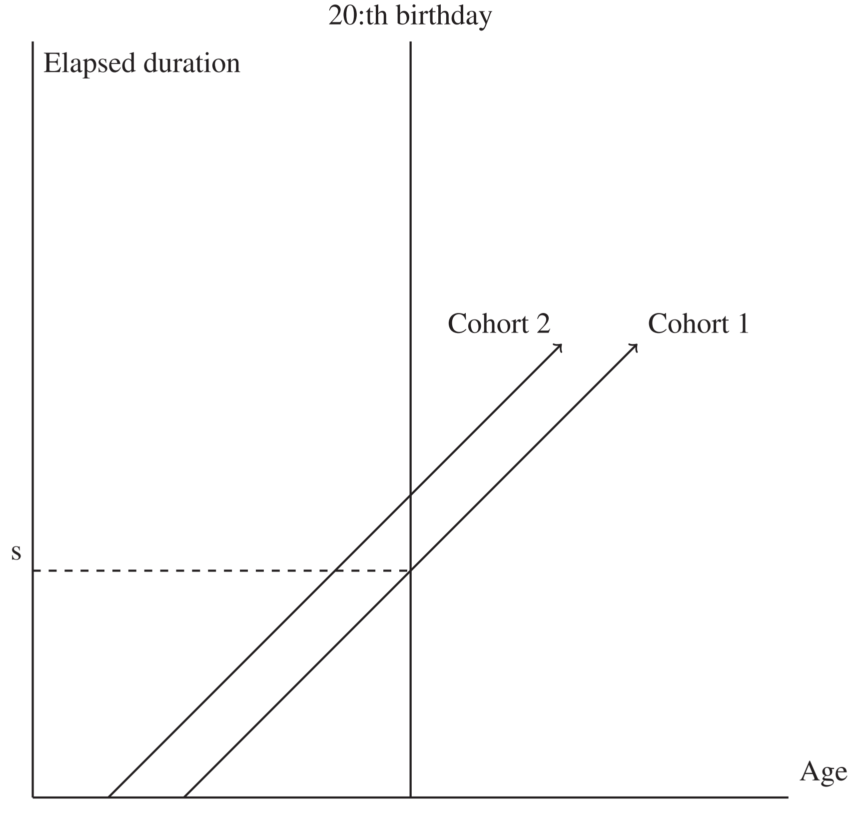

Figure 2.2. Identification based on two cohorts.

Here, s corresponds to the "running variable" for cohort 1. Moreover, cohort 2 are slightly younger when they enter unemployment. They become 20 years at some elapsed duration higher than s. The idea is essentially to compare the employment hazards at an elapsed duration s, when cohort 1 can claim UI benefits whereas cohort 2 cannot.

Van den Berg et al. (2010) derive conditions under which this comparison will identify a meaningful causal effect of UI-benefit-eligibility on the employment hazard. This is a binary treatment that is assigned to commence at some elapsed duration $s \in [0, \infty)$. Here, ∞ correspond to the hypothetical scenario where the individual never becomes eligible for UI, regardless of how long she remains unemployed.

For each treatment arm, there is a random variable T(s). This is the potential outcome duration if the individual becomes eligible for UI-benefits at an elapsed duration s. Unemployed school-leavers are allowed to differ with respect to observed and unobserved characteristics, denoted X and V respectively. Treatment is assumed to be randomized conditional on X and V. Further, treatment assignment should be orthogonal to V given X. Formally:

Assumption 1 (Assignment). $S \perp T(s) \mid (X,V)$, and $S \perp V \mid$ X.

Assumption 1 is fulfilled if treatment is as good as random when conditioning on observed covariates X. In terms of Figure 2.2, cohort 1 and 2 have to be comparable when they enter unemployment. However, since we are interested in the causal effect of becoming eligible for UI at an elapsed duration *s*, cohort 1 and 2 have to be comparable *conditioning* on staying unemployed until *s*. To achieve this, I impose the "no anticipation" assumption. Formally:

Assumption 2 (No anticipation). For all s $\in (0,\infty)$ and for all t $\leq$ s and all X, V $\Theta_{T(s)}(t|X,V) = \Theta_{T(\infty)}(t|X,V)$

See Abbring and Van den Berg (2003) for a detailed discussion. Here, $\Theta_{T(s)}$ is the integrated hazard for the potential duration outcome associated with treatment assignment at time s. Hence, $\Theta_{T(\infty)}$ corresponds to the integrated hazard if treatment is never assigned. Assumption 2 imposes that individuals should behave as if treatment will never be assigned up until the point when it is assigned. Put differently, unemployed school leavers should not change their job search behavior in anticipation of future UI-benefit payments. This assumption holds if treatment exposure is completely unanticipated or if individual simply do not act on information about future exposure to treatment.

Consider Figure 2.2. If assumption 1 and 2 holds, the difference in the employment hazards at an elapsed duration *s* consistently estimates the following parameter:

$$ATTS(s,s'|X) = E[\theta_{T(s)}(s|X,V) - \theta_{T(s')}(s|X,V)|X,T(s) \geq s] \qquad (2.2)$$

Where $\theta_{T(s)}$ and $\theta_{T(s')}$ are the hazard functions associated with the potential duration distributions T(s) and T(s'). This parameter is called "the average treatment effect on treated survivors". It does not depend on s', as long as s' is greater than s. We can therefore replace it with $ATTS(s|X)$, which is the instantaneous causal effect of becoming eligible for UI-benefits at an elapsed duration s. Furthermore, the "treated survivors" are those who are still unemployed at s, if they become eligible for UI at an elapsed duration s. Therefore, the treatment effect if averaged over subpopulations with longer potential outcome durations when the treated cohort becomes eligible for UI further into the unemployment spell.

In my setting, each cohort can be matched to several potential comparison groups. For instance, consider the cohort that becomes eligible for UI after 3 months. They could potentially be matched to a comparison group that become eligible for UI at $s = 4$, $s = 5$,..., $s = 13$. However, assumption 1 impose that the treatment and control group should be comparable when they enter unemployment. Moreover, the identifying variation is derived from the

Running variable		
Treatment group	Control group	Segment
3	4	1
4	5	2
5	6	3
6	7	4
7	8	5
8	9	6
9	10	7
10	11	8
11	12	9
12	13	10

The table show the treatment and control group at each segment on the running variable

Table 2.1. *Segments, Treatment and Control Groups.*

fact that school-leavers differ slightly in age when they enter unemployment. Specifically, the control group will always consist of individual born at least one month later in the year.

The descriptive evidence in appendix Table 2.4 shows that grades from primary school are smoothly decreasing with higher values on the running variable. This suggest that assumption (1) is more likely to hold if each cohort is matched to a comparison group exposed to treatment at a marginally higher duration. Therefore, I will match each cohort to a comparison group that become eligible for UI after one additional month of unemployment.

I define a segment on the running variable as a pairing of a treatment and a control group. At each segment, the comparison group become eligible for UI after an additional month of unemployment. There are 10 segments on the running variable, listed in Table 2.1.

2.5 Validity of the Research Design

The empirical design consistently estimates the instantaneous causal effect of UI-benefits on the employment hazard if: (i) the treatment and comparison groups are comparable when they enter unemployment (random assignment), (ii) the dynamic selection is identical until treatment is imposed (no anticipation). To test this, consider the following regression model:

$$D_{ic} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + X'_{ij}\phi_c + \varepsilon_i \tag{2.3}$$

$D_{ic} = 1$ if individual i belongs to the treatment group at segment c, α_c is a segment fixed effect, $\tau_1, ..., \tau_k$ are month of inflow fixed effects and $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation. Finally, X_{ic} is a vector of individual background characteristics.[6] Equation (2.3) provides means of testing whether the treatment and control groups are comparable when they enter unemployment (assumption 1). If any elements in ϕ_c are statistically significant at the start of the unemployment spells, this indicates that individual background characteristics can predict treatment group status at segment c, and that assumption (1) is violated.

Running specification (2) after conditioning on staying unemployed until the treatment group becomes eligible for UI provides a test of assumption 2 (identical dynamic selection until treatment exposure). Intuitively, if the cohorts are comparable when they enter unemployment, (assumption 1), and the dynamic selection is identical in the pre-treatment period, (assumption 2), the cohorts should also be comparable after conditioning on staying unemployed until the treatment group becomes eligible for UI.

I run specification (2) separately for each segment on the running variable. This produces the results collected in appendix Table 2.5. Here, I report p-values for the Wald tests $\phi_c = 0$ at the start of the unemployment spells *and* conditional on survival until the treatment group becomes treated. The covariates tend to be strongly balanced. There are no statistically significant differences between treated and controls when they enter unemployment. However, some unbalancing do emerge when we move to higher values of the running variable *and* condition on survival until the treatment group becomes treated. Moreover, Table 2.5 contains the results from twenty balancing tests. Out of those, two came out statistically significant which is line with what we should expect just by chance.

The "no anticipation assumption" can also be tested by comparing the survival probabilities during the pre-treatment period, when neither group could claim UI. If the "no anticipation assumption" holds, treatment and control groups should leave unemployment to the same extent during the pre-treatment period. Appendix Figure 2.3 depict the empirical survivor functions for the treatment and control groups during the period when neither group could claim

[6]The vector of background characteristics include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other.

UI. The survival curves are very similar in almost every treatment-control group comparison. This supports the "no anticipation" assumption.

2.6 UI benefits and the Employment Hazard

This section presents estimates of the instantaneous causal effect of UI benefits on the employment hazard. Let i be an indicator for the individual, c index segments on the running variable and t indicate the month of unemployment. Furthermore, let s_c denote the value of the running variable for the treated cohort at segment c. Consider the following specification:

$$y_{ict} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + \sum_{t=1}^{s_c+1} \beta_{1t} m_t + \sum_{t=1}^{s_c+1} \beta_{2t} m_t D_{ic} + X_{ic}' \phi + \varepsilon_{ict}, \tag{2.4}$$

where y_{ict} is a dummy variable for whether individual i in segment c exits to employment in the t:th month of unemployment, α_c is a segment fixed effect, $\tau_1, ..., \tau_k$ are fixed effects for month of entry into unemployment, $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation, $m_1, ..., m_{s_c+1}$ are dummy variables for month in the unemployment spell, D_{ic} is equal to one if individual i belongs to the treatment group in segment c and X_{ic} is a vector of controls.[7] The interaction terms $m_1 D_{ic}, ..., m_{s_c+1} D_{ic}$ capture monthly differences in the employment hazards in each segment.

Figure 2.1 showed clear spikes in UI benefit payments the calender month after the 20:th birthday. The treatment effect is thus estimated with the final interaction term $m_{s_c+1} D_{ic}$. The remaining interaction terms are placebo tests.

I begin the analysis by pooling all of the segments and estimate the discontinuity's (the $m_{s_c+1} D_{ic}$ terms) jointly.[8] Adding fixed effects for segments and month of entry into unemployment means that the identifying variation is derived from the fact that the comparison group in each segment is one month younger than the treatment group at the start of the unemployment spell. The results are collected in Table 2.2. Standard errors are clustered by person since the same spell can appear multiple times when the discontinuity's are pooled. The point estimates are strongly significant and robust across various specifications. Overall, the results in Table 2.2 suggest that UI benefit eligibility decreases the conditional probability of leaving unemployment within one month by approximately 1 percentage point. Moreover, appendix Figure 2.4 shows that the monthly baseline hazard is approximately 8%. Therefore, the estimated treatment effect translated into a 12.5 percent drop in the monthly job-finding probability. This is quite a substantial effect.

[7] The vector of controls includes gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other.

[8] The approach of pooling the discontinuity's is analogous to the "regression discontinuity design with multiple cutoffs", see Bertanha (2016); Papay et al. (2011); Cattaneo et al. (2016).

VARIABLES	(1)	(2)	(3)	(4)
Treatment effect	-0.011***	-0.011***	-0.009***	-0.010***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	31,392	31,392	31,392	31,392
Clustered standard errors	✓	✓	✓	✓
Year of graduation FE	✓	✓	✓	✓
Controls		✓		✓
Month of inflow FE			✓	✓

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Estimated treatment effect after pooling all the available thresholds. The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other. *** p<0.01, ** p<0.05,
* p<0.1

Table 2.2. *Pooled Estimates*

Next, I run specification (2.4) separately for each segment of the running variable. The $m_{s_c+1}D_{ic}$ terms yields an estimate of ATTS($s_j+1|$X). For ease of notation, I will replace ATTS($s_j+1|$X) by the shorthand measure ATTS($t|$X). This is the instantaneous causal effect of UI benefits on the employment hazard at an elapsed duration t. The remaining interaction terms $m_1D_{ic},...,m_{s_c}D_{ic}$ estimate differences in the employment hazards during the pre-treatment period, when neither group can claim UI. Therefore, these placebo checks provide further means of testing the "no anticipation" assumption.

Figure 2.5 in the appendix plots the estimates of ATTS($t|$X) together with 95% confidence intervals. The point estimates do not vary systematically depending on the elapsed duration that treatment is imposed. However, precision is decreasing for higher values of t. Overall, there is no evidence that the treatment effect varies depending on the elapsed duration that UI-benefit payments are made. Table 2.6 in the appendix presents the full set of regression outputs.

Appendix Figure 2.6 shows the distribution of the placebo estimates from my preferred specification with controls and month of inflow fixed effects. Overall, 5.3% of the placebo tests are significant at the 5% level, which is expected by chance. This provides additional support for the "no anticipation" assumption.

It is interesting to consider how job finding rates evolve further into the spell. The dynamic selection in the treated and control group will however be unaligned after treatment is first imposed. Consequently, differences in the job finding hazards further into the spell cannot be given a causal interpretation. Graphs are nonetheless provided in appendix Figure 2.4.

2.6.1 Sensitivity Analysis

The policy discontinuity in UI benefit eligibility coincides with a change in ALMP policies. Specifically, unemployed teenagers are allowed to participate in the MYP until they are 20 years of age. Youths in the age range 20-24 are by contrast eligible for the YG after 90 days of unemployment. Moreover, both programs are administrated by the municipalities but provision of the YG is much more restrictive. Hence, one might expect a sharp drop in ALMP participation after individuals become 20 years of age.

Appendix Figure 2.7 depicts the evolution of ALMP participation rates. There is a sharp drop in ALMP participation the calender month after the 20:th birthday. If the program exerts a casual effect on job finding rates, this would confound the estimated impact of UI benefits. To address this, I repeat the main analysis but remove individuals who enroll in ALMP during the "pre treatment" period. Figure 2.8 in the appendix depicts the enrolment rates after this restriction was imposed. Note there are no longer any clear discontinuity's around the 20:th birthday. Next, I repeat the previous analysis using the restricted sample. Specifically, all of the discontinuity's are pooled together, segment fixed effects are included and standard errors are clustered by person. This produces the results collected in appendix Table 2.7. The main estimates do not change much when the restricted sample is used. Any potential bias induced by the discontinuity in ALMP policies should therefore be negligible.

2.7 UI-benefits and Entry Level Wages

Mortensen (1977) laid out the theoretical arguments for a positive relationship between the generosity of UI benefits and the reservation wage. The idea is essentially that UI benefits increases the value of unemployment. Consequently, the lowest wage an unemployed worker is willing to accept (the reservation wage) will increase as the generosity of UI benefits increases. Moreover, the likelihood that a wage offer is sufficiently high for the worker to accept the job decreases as the reservation wage increases. This creates a positive relationship between the generosity of UI benefits and the duration of unemployment. This section tests whether the drop in job finding rates associated with the spikes in UI benefits is consistent with higher reservation wages.

I follow the unemployment spells throughout the calender year after high school graduation. Spells that are still in progress are treated as right censored. Note that both groups become eligible for UI benefits during the period. However, the treated cohort can start their benefit spells earlier. Consequently, if UI benefits increase worker selectivity, we would expect to observe higher post unemployment wages for the treated cohort.

Entry level wages are considered for those who left unemployment before the end of the observation window and were wage earners the year after the spell ended. For simplicity, I will refer to those who fulfill this criterion as employed. Since the outcome is only observed for a selected subset, we need to check weather sample selectivity is a concern (Heckman, 1974). Consider the following regression model:

$$y_{ic} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + \beta_c D_{ic} + \varepsilon_{ic} \quad (2.5)$$

Where y_{ic} is a dummy variable for observing the wage of individual i in segment c, D_{ic} is dummy variable for belonging to the treatment group in segment c, α_c is a segment fixed effect, $\tau_1, ..., \tau_k$ are fixed effects for month of entry into unemployment and $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation. I run specification (4) separately for each segment on the running variable. Point estimates of β_c together with p-values for the hypothesis $\beta_c = 0$ are reported in appendix Table 2.8. The estimates are economically small and insignificant. This would suggest that sample selectivity should not be major concern.

The previous section shows that UI benefit eligibility induce an immediate drop in the employment hazard. However, Table 2.8 show that treated and controls are employed at comparable rates once the tracking period is expanded. This imply that an additional month of UI benefit eligibility does not effect the likelihood of finding a job within a year after high school graduation. The duration until a job offer is accepted will however increase marginally. To test whether this translates into higher entry level wages, consider the following regression equation:

$$w_{ic} = \alpha_c + \tau_1 + ... + \tau_k + \gamma_1 + ... + \gamma_4 + \beta_c D_{ic} + X_{ic}'\phi + \varepsilon_{ic} \quad (2.6)$$

Where w_{ic} is the log monthly wage of individual i in segment c, α_c is a segment fixed effect, $\tau_1, ..., \tau_k$ are fixed effects for month of entry into unemployment, $\gamma_1, ..., \gamma_4$ are dummy variables for year of graduation, D_{ic} is a dummy variable equal to one if individual i belongs to the treatment group at segment c, and X_{ic} is a vector of controls.[9]

I once again begin the analysis by pooling the discontinuity's and estimate them jointly. Standard errors are clustered by person and segment fixed effects are included in every specification. This produces the results collected in Table 2.3. The estimates are small, and statistically insignificant once fixed effects for month on entry are included.

[9]The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other.

VARIABLES	(1)	(2)	(3)	(4)
Treatment effect	0.008***	0.007***	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	76,677	76,677	76,677	76,677
Clustered standard errors	✓	✓	✓	✓
Year of graduation	✓	✓	✓	✓
Controls		✓		✓
Month of inflow FE			✓	✓

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Estimated treatment effect after pooling all the available thresholds. The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other. *** p<0.01, ** p<0.05, * p<0.1

Table 2.3. *UI Benefits and Entry Level Wages, Pooled Estimates*

Next, I run specification (2.6) separately for each segment on the running variable. This produces the results collected in appendix Table 2.9. There is no evidence that UI benefits increase post unemployment wages. Altogether, these results suggest that the drop in job finding rates associated with the spike in UI benefit payments operates through some other channel than increased worker selectivity, for instance reduced search intensity.

2.8 Conclusion

This paper attempts to estimate the effect of UI-benefits on job-finding rates and entry level wages for unemployed high school leavers. Between 2002 and 2006, all school-leavers became eligible for UI once they turned 20 years. Therefore, the start of an unemployment spell relative to the 20:th birthday creates potentially exogenous variation in the duration until jobseekers become eligible for UI. This was exploited using the framework developed by Van den Berg et al. (2010).

I find that UI-benefit eligibility reduce the conditional probability to leave unemployment by one percentage point. The baseline hazard is roughly 8%, which means that the treatment effect translate into a 12.5% drop in the employment hazard. I further find that UI-benefit eligibility have no effect on entry-level wages. This would suggest that the impact of UI on job finding rates operates through some other channel than increased selectivity, perhaps reduced search intensity.

It is worth stressing that the estimates provided in this paper focus on short-run effects of UI-benefits on job finding rates and entry-level wages. Specifically, I focus on the effects of being exposed to UI for one month. It is not necessarily the case that the results can be generalized to prolonged exposure to UI. However, it is not possible to study long-run effects of UI on the employment hazard without imposing some untestable model structure to deal with dynamic selection. Hence, although there are some clear limitations of focusing on short-run effects of UI, it provides more robust evidence of the effects of UI on employment.

2.9 Appendix

Running variable	Grades	Academic	Vocational	Other	Male
3	-0.073	0.471	0.436	0.092	0.525
	(0.817)	(0.499)	(0.496)	(0.289)	(0.499)
4	-0.072	0.471	0.438	0.090	0.527
	(0.831)	(0.499)	(0.496)	(0.287)	(0.499)
5	-0.094	0.467	0.443	0.090	0.536
	(0.840)	(0.499)	(0.497)	(0.286)	(0.499)
6	-0.113	0.469	0.449	0.082	0.533
	(0.835)	(0.499)	(0.497)	(0.274)	(0.499)
7	-0.112	0.464	0.449	0.087	0.523
	(0.825)	(0.499)	(0.497)	(0.282)	(0.500)
8	-0.147	0.450	0.463	0.087	0.524
	(0.829)	(0.498)	(0.499)	(0.282)	(0.499)
9	-0.152	0.457	0.457	0.086	0.515
	(0.853)	(0.498)	(0.498)	(0.280)	(0.500)
10	-0.182	0.437	0.479	0.084	0.501
	(0.851)	(0.496)	(0.500)	(0.277)	(0.500)
11	-0.207	0.431	0.484	0.085	0.476
	(0.859)	(0.495)	(0.500)	(0.278)	(0.500)
12	-0.211	0.426	0.487	0.087	0.480
	(0.870)	(0.495)	(0.500)	(0.282)	(0.500)
13	-0.268	0.427	0.502	0.071	0.501
	(0.886)	(0.495)	(0.500)	(0.257)	(0.500)
Total	-0.133	0.456	0.457	0.087	0.518
	(0.842)	(0.498)	(0.498)	(0.282)	(0.500)

Notes: The table show the mean and standard deviation (in parenthesis) of selected across the distribution of the running variable. Academic, Vocational and Other refers to academic, vocational and other high school program respectively.

Table 2.4. *Descriptive Statistics*

Running variable			
Treatment group	Control group	Elapsed duration	p-value
3	4	0	0.97
3	4	3	0.77
4	5	0	0.59
4	5	4	0.75
5	6	0	0.26
5	6	5	0.15
6	7	0	0.84
6	7	6	0.48
7	8	0	0.28
7	8	7	0.67
8	9	0	0.62
8	9	8	0.40
9	10	0	0.19
9	10	9	0.31
10	11	0	0.18
10	11	10	0.79
11	12	0	0.96
11	12	11	0.03
12	13	0	0.55
12	13	12	0.01

The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other. The distribution of covariates are compared when subjects flow into unemployment and conditional on survival until the treatment group becomes eligible for UI benefits.

Table 2.5. *Treatment vs. Control group. P-values for Wald Test Comparing the Distribution of Covariates*

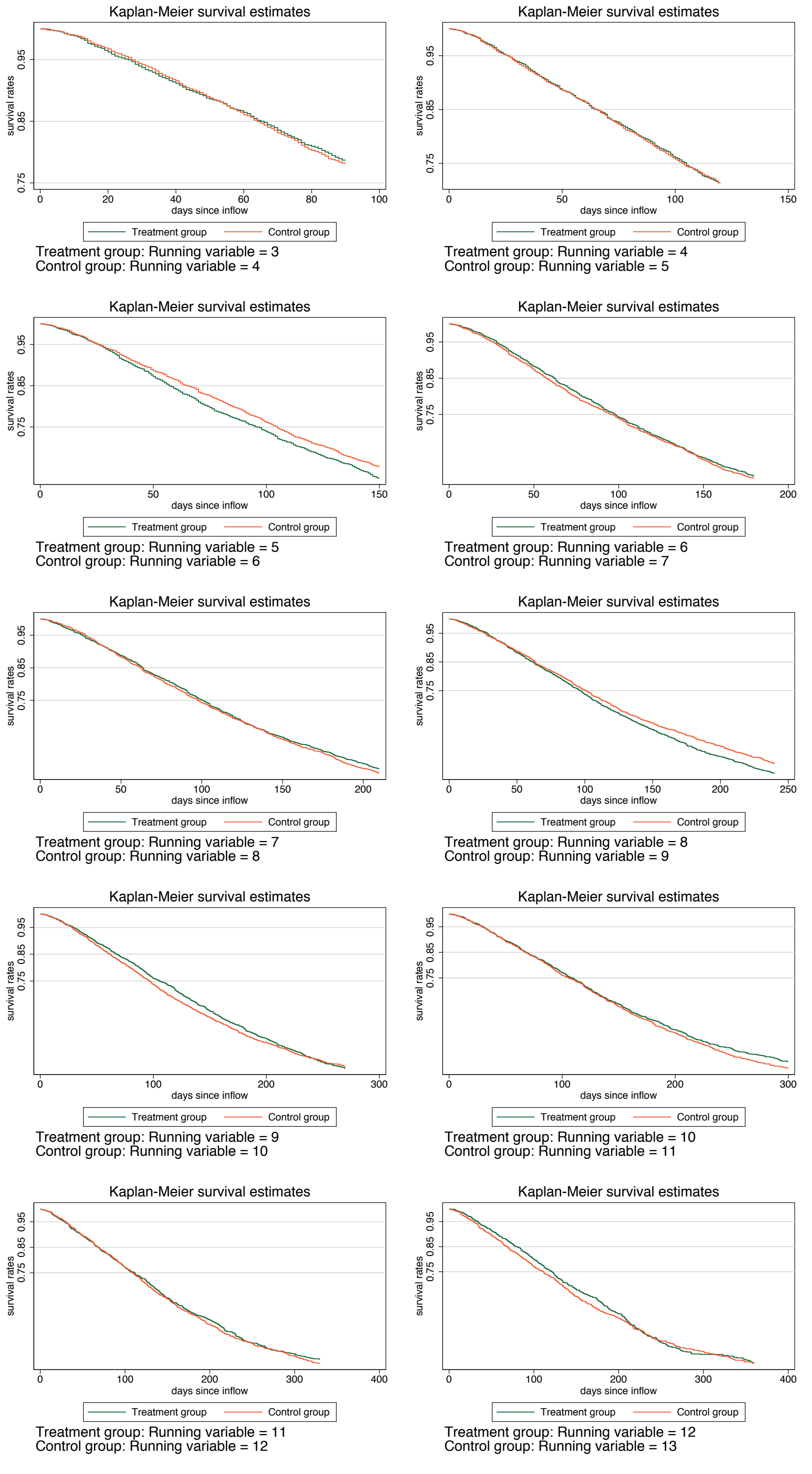

Figure 2.3. Empirical Survivor Functions

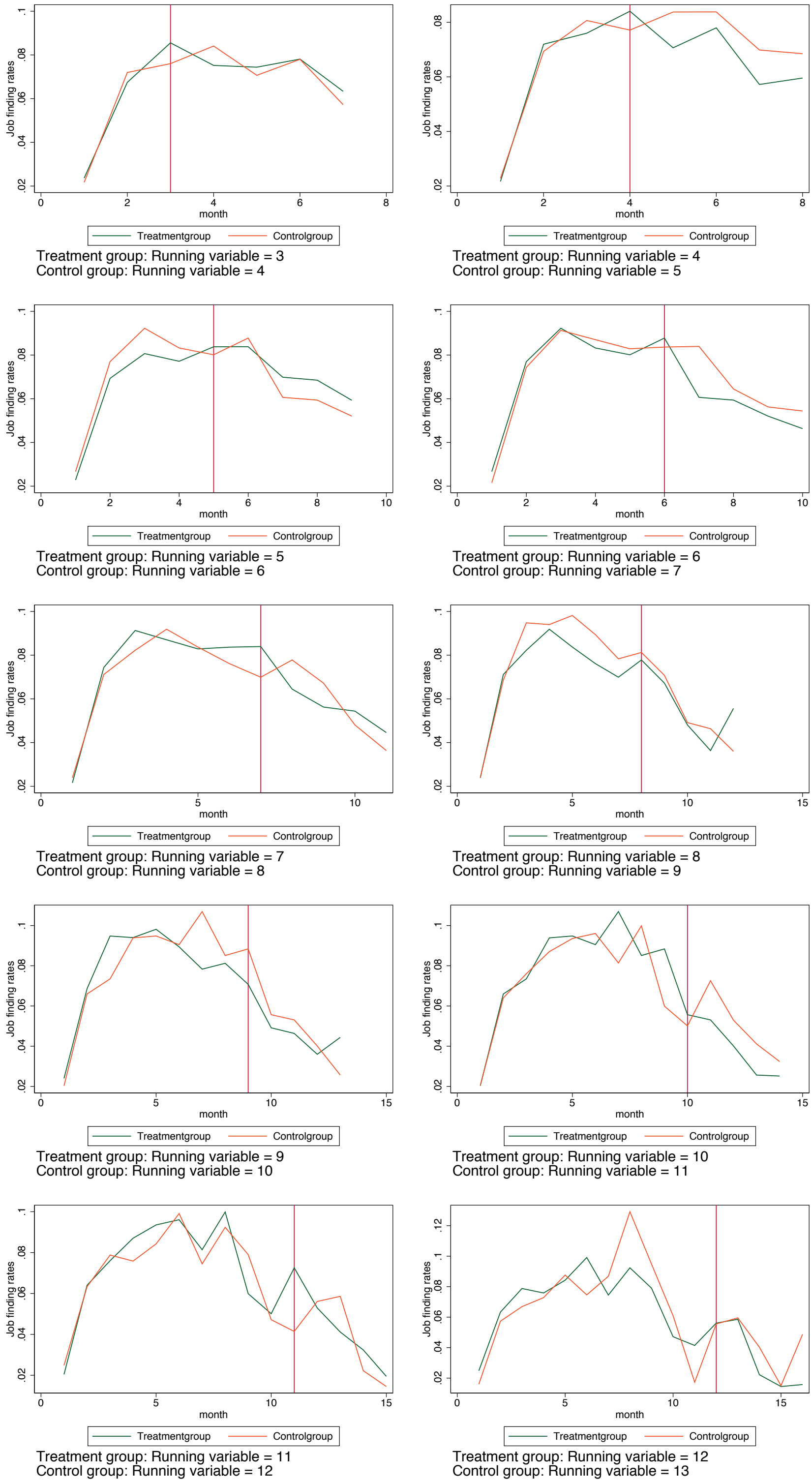

Figure 2.4. Job Finding Rates for the Treatment and Control Groups

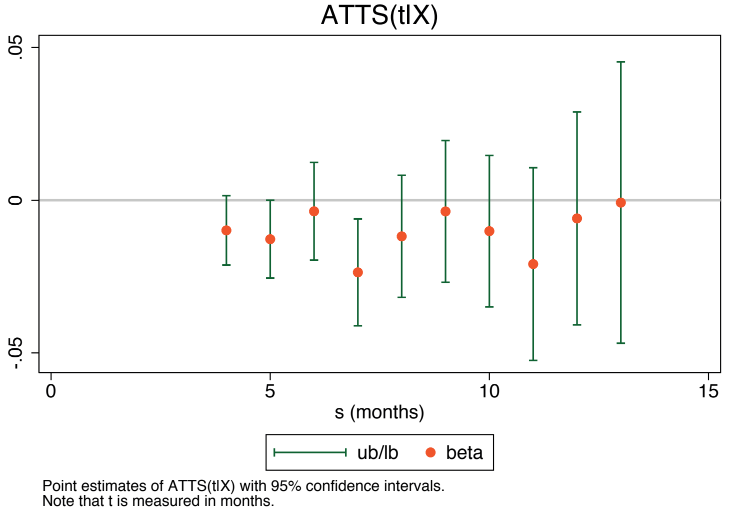

Figure 2.5. UI benefits and the Employment Hazard

Running variable				
Treatment/Control group	Model 1	Model 2	Model 3	Model 4
3,4	-0.009	-0.009	-0.010*	-0.010*
	(0.006)	(0.006)	(0.006)	(0.006)
4,5	-0.013**	-0.013**	-0.013**	-0.013**
	(0.007)	(0.006)	(0.007)	(0.007)
5,6	-0.004	-0.005	-0.003	-0.004
	(0.008)	(0.008)	(0.008)	(0.008)
6,7	-0.024***	-0.023***	-0.024***	-0.024***
	(0.009)	(0.009)	(0.009)	(0.009)
7,8	-0.013	-0.013	-0.012	-0.012
	(0.010)	(0.010)	(0.010)	(0.010)
8,9	-0.004	-0.003	-0.004	-0.004
	(0.012)	(0.012)	(0.012)	(0.012)
9,10	-0.007	-0.008	-0.010	-0.010
	(0.013)	(0.013)	(0.013)	(0.013)
10,11	-0.019	-0.020	-0.020	-0.021
	(0.016)	(0.016)	(0.016)	(0.016)
11,12	-0.003	-0.004	-0.005	-0.006
	(0.018)	(0.018)	(0.018)	(0.018)
12,13	-0.001	-0.001	-0.001	-0.001
	(0.023)	(0.023)	(0.023)	(0.023)
Year of graduation FE:s	✓	✓	✓	✓
Controls		✓		✓
Month of inflow FE:s			✓	✓

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table reports differences in the employment hazards in the calender month when the treatment group can claim UI-benefits and the controls cannot.

Table 2.6. *UI Benefits and the Employment Hazard*

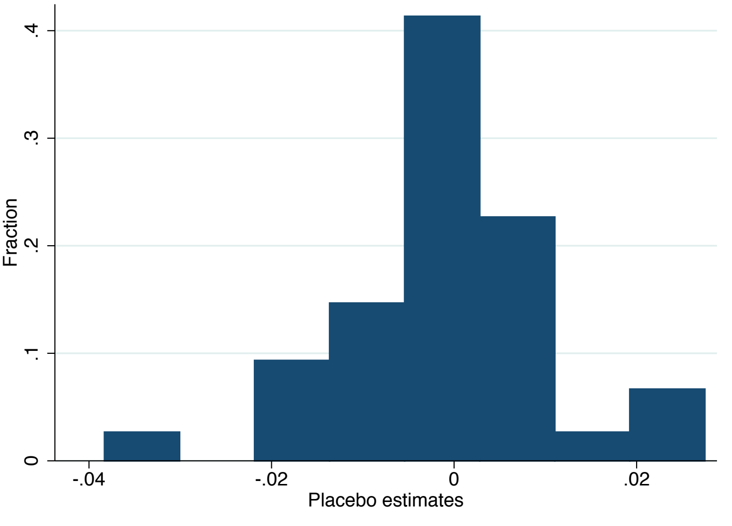

Figure 2.6. Empirical Distribution of Placebo Estimates

Note: The figure shows the empirical distribution of placebo estimates. These estimates are computed by comparing the employment hazards in treated and control groups during the period where no one could claim UI-benefits. Overall, 5.3% of the placebo estimates are significant at the 5% level.

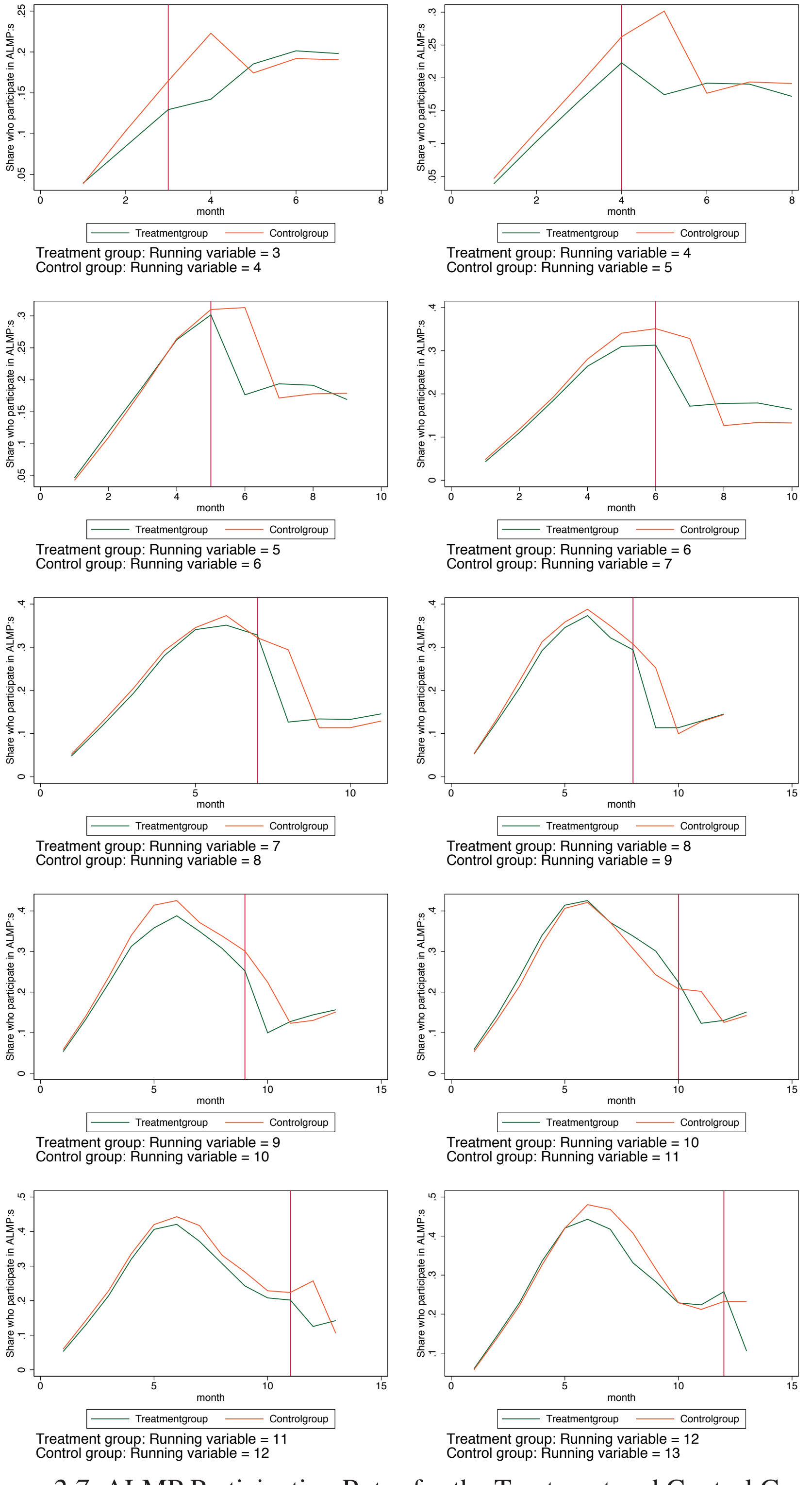

Figure 2.7. ALMP Participation Rates for the Treatment and Control Groups

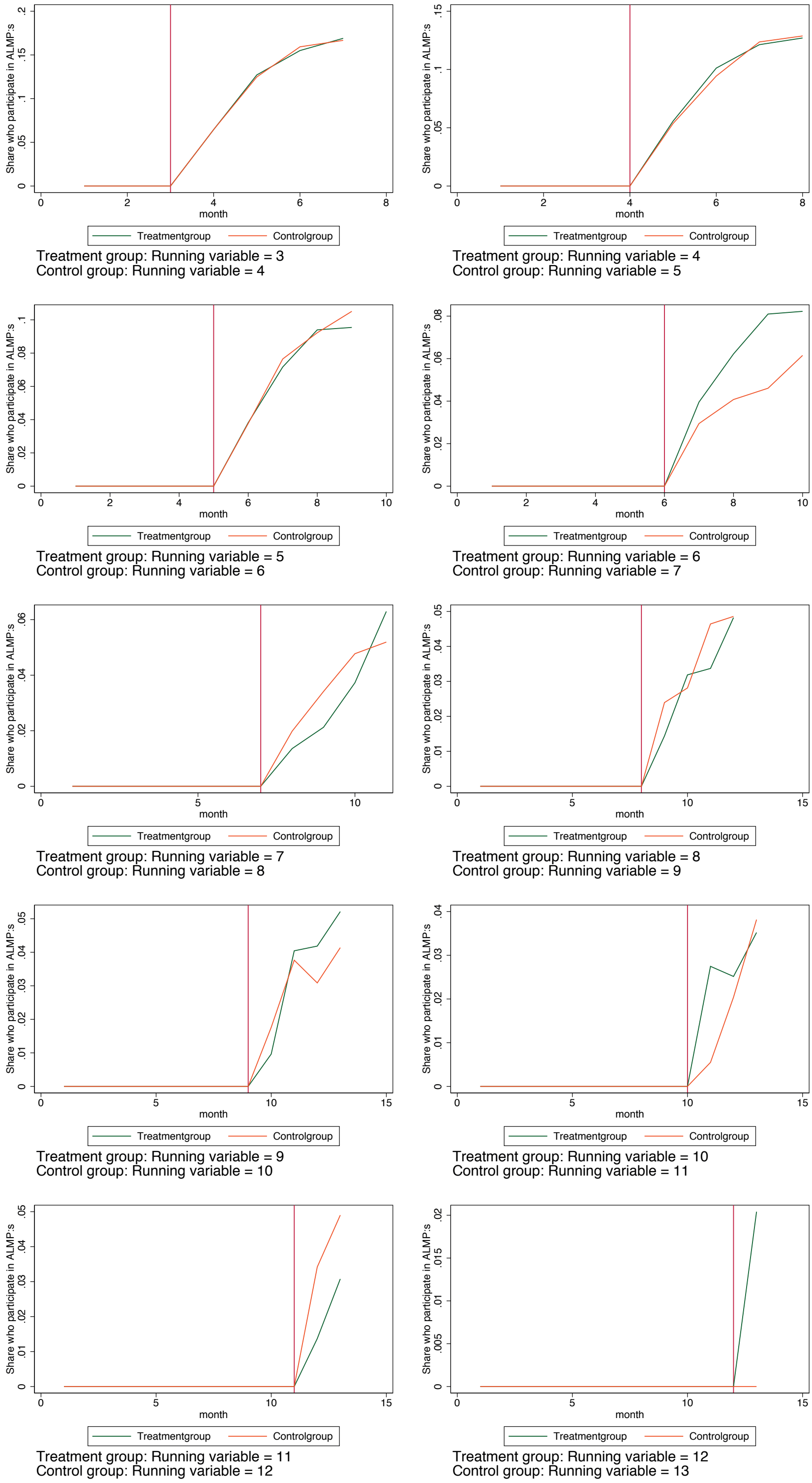

Figure 2.8. ALMP Participation Rates, with Censoring

VARIABLES	(1)	(2)	(3)	(4)
Treatment effect	-0.009**	-0.009**	-0.008**	-0.008**
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	20,161	20,161	20,161	20,161
Clustered standard errors	✓	✓	✓	✓
Year of graduation FE	✓	✓	✓	✓
Controls		✓		✓
Month of inflow FE			✓	✓

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Estimated treatment effect after pooling all the available thresholds. The vector of controls include gender, grades from primary school and dummy variables for type of high school program, i.e academic, vocational or other. *** p<0.01, ** p<0.05, * p<0.1

Table 2.7. *Pooled Estimate, Robustness Test*

Running variable			
Treatment group	Control group	β_j	p-value
3	4	0.0017	0.82
4	5	0.0096	0.19
5	6	0.0116	0.13
6	7	0.0119	0.13
7	8	-0.007	0.39
8	9	0.0038	0.65
9	10	0.0031	0.74
10	11	0.0062	0.53
11	12	-0.0006	0.95
12	13	-0.0001	0.99

Table 2.8. *Treatment vs. Control group. P-values for Wald Test Comparing the Likelihood of Employment.*

Running variable Treatment/Control group	Model 1	Model 2	Model 3	Model 4
3,4	0.019	0.020	0.012	0.013
	(0.022)	(0.021)	(0.022)	(0.022)
4,5	-0.014	-0.014	-0.021	-0.020
	(0.021)	(0.020)	(0.021)	(0.021)
5,6	0.011	0.011	0.008	0.006
	(0.021)	(0.021)	(0.022)	(0.021)
6,7	0.012	0.008	0.011	0.008
	(0.022)	(0.021)	(0.022)	(0.022)
7,8	-0.007	-0.003	0.006	0.009
	(0.022)	(0.022)	(0.023)	(0.022)
8,9	0.010	0.005	0.021	0.016
	(0.024)	(0.024)	(0.024)	(0.024)
9,10	0.048*	0.041	0.026	0.030
	(0.026)	(0.026)	(0.027)	(0.027)
10,11	-0.020	-0.025	-0.030	-0.034
	(0.028)	(0.028)	(0.029)	(0.029)
11,12	-0.008	-0.005	-0.014	-0.010
	(0.030)	(0.029)	(0.031)	(0.031)
12,13	0.047	0.051	0.040	0.039
	(0.037)	(0.036)	(0.039)	(0.039)
Year of graduation FE:s	✓	✓	✓	✓
Controls		✓		✓
Month of inflow FE:s			✓	✓

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table reports differences in log monthly wages between treatment and control groups.

Table 2.9. *UI Benefits and Wages*

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3. Partial Identification in Difference-in-Difference Models with Missing Outcome Data

3.1 Introduction

Difference-in-Differences (DID) is a simple, transparent, and immensely popular method to estimate the effect of a treatment.[1] However, assessing the impact of an intervention is often complicated by sample selection, for instance when estimating the effect of a job training program on wages. The outcome of interest is only observed for those who are employed. Moreover, the composition of individuals who have a job can change over time because of the treatment and general time trends. Therefore, a naive DID analysis cannot separate a causal effect of the treatment on wages from changes in sample composition.

This problem cannot be solved through a randomized experiment (Lee, 2009; Heckman, 1977). This is because randomization only ensures that the treatment and control groups are comparable at baseline. Once the treatment is implemented, the treated and controls can very well be systematically different *conditional* on employment. This is because the treatment can also affect the composition of employed workers in the treatment and control groups.

There has been significant methodological progress on how to deal with the selection problem in the context of a randomized experiment. In particular, Lee (2009) develops an attractive method to bound average causal effects with non-random selection, the so called "Lee Bounds". This approach is, however, only applicable to a randomized experiment, and effective methods to deal with sample selection in the DID design are currently lacking.

In this paper, I extend the "Lee bounds" to the DID setting. Identification is achieved in two steps. First, I impose a threshold crossing model for selection, similar to the "treatment participation" equation in De Chaisemartin and d'Haultfoeuille (2015).[2]

[1] Bertrand et al. (2004) found that between 1990 and 2000, 92 DID papers were published in 6 top journals: American Economic Review, the Industrial and Labor Relations Review, the Journal of Political Economy, the Journal of Public Economics, the Journal of Labor Economics, and the Quarterly Journal of Economics.

[2] De Chaisemartin and d'Haultfoeuille (2015) considers DID settings where the treatment rate only increases more in the treatment group, i.e "Fuzzy" designs. They also restrict attention to the case when the outcome of interest is always observed. Hence, the setting in this paper is substantially different.

An individual is observed if the unobserved latent index is above some threshold. The defining feature of a group is that the distribution of the latent index does not change over time within groups (Athey and Imbens, 2006). If the outcome of interest are wages, the latent index can be interpreted as a unit's propensity to be employed, since the wage is only observed if the individual is employed.

The second step is to modify the crucial identifying assumption. In particular, "Lee bounds" are designed for randomized experiments, where the treatment and control group are comparable by construction. The crucial identifying assumption in the DID design is that the trends in the outcome are the same in both groups in the absence of treatment. Therefore, I introduce a slightly modified version of this parallel trend assumption.

The method I propose have several potential empirical applications. Carling et al. (2001) and Van Ours and Vodopivec (2006) use the DID design to study the effects of UI benefits on the duration of joblessness. Further research on the effects of UI benefits on post unemployment wages would require effective selection corrections. Moreover, Pischke (2007) looks at the effects of shortening school term length on the likelihood of grade repetition using a DID approach. Further analysis about the effects on learning (proxied by grades) would also require selection corrections.

The remainder of this paper is organized as follows. The next section provides an overview of the relevant literature and describes Lee's bounding procedure. Section 3.3 outlines the framework proposed for generalizing Lee's method to the DID design and presents the identification result. Section 3.4 and 3.5 discusses estimation and inference. Section 3.6 concludes.

3.2 Relevant Literature

3.2.1 Existing approaches

It is often crucial for researchers to be able to distinguish between effects on wages as distinct from total earnings, which can be written as the product between price of labor (the wage) and hours worked. The price of labor is typically used as a proxy for human capital. Moreover, public policy's aimed at improving labor market outcomes for disadvantages individuals typically involves substantial investments in human capital through job training and eduction. In order to assess if these policies increase human capital, it is crucial to distinguish between wage and labor supply effects. If a policy is found to increase earnings, this effect could solely depend on increased labor supply. This motivates studying effects on wages as opposed to earnings.

The methodological challenge is that wages are only observed for those who are employed. Moreover, policy's that are likely to impact wages typically effect employment probabilities. Standard labor supply theory predicts a positive relationship between wage offers and the likelihood of employment, which creates a selection problem (Heckman, 1974). It is well understood that this problem cannot be solved even with a randomized experiment. Randomization will ensure that the treatment and control group are comparable at baseline. However, if the policy affects the likelihood of employment, the groups may very well be systematically different conditional on employment status. This problem has spurred a long literature attempting to correct for selection bias when estimating wage effects of labor market policy's.

The conventional approach, following Heckman (1977), is to explicitly model the process determining selection. However, this approach requires several distributional assumptions about the selection process. Another approach is to assume that there are some exogenous variables that affect selection but are unrelated to the outcome of interest. Such exclusion restriction are utilized in semi-parametric selection models (Ahn and Powell, 1993; Das et al., 2003). It is however often very difficult to find plausible instruments. Most variables that affect employment typically tend to impact wages as well.

Manski (1989) proposes abandoning the use of parametric identifying assumptions for selection models and instead consider non-parametric bounds on the object of interest. Manski argues that by sacrificing point identification, it is possible to derive informative bounds on the treatment effect under much more plausible assumptions. The bounding approach suggested by Manski (1989) was however only applicable to the case where the outcome variable has bounded support. Moreover, labor economists typically focus on wages, which arguable have unbounded support. In recent years, however, several papers have derived bounds on wage effects by imposing weak restrictions, typically derived from economic theory (Blundell et al., 2007; Kline and Santos, 2013).

In particular, Lee (2009) developed a method for bounding average treatment effects on wages without imposing restrictions from economic theory. The purpose of this paper is to generalize this bounding procedure to the DID design. Lee's method is described in more detail in the next subsection.

3.2.2 Lee (2009) Bounds

Lee's motivating application is the study of a randomized training program which raises earnings and the likelihood of employment. We want to study whether the program was successful in raising wage rates but face a selection problem since wages are only observed for those who are employed. Moreover, even though randomization guarantees that the treatment and control group are comparable at baseline, they will differ systematically conditional on employment since the program effected the composition of those who work.

Lee consider partial identification and derives bounds on the treatment effect under a monotonicity assumption. Formally, let Y be the outcome of interest (wages), D an indicator for program participation, and S a dummy variable for employment. Note that Y is only observed when $S = 1$. We can now introduce the problem using a latent variable model:

$$
\begin{aligned}
Y &= \alpha_1 + \beta D + \varepsilon \\
S^* &= \alpha_2 + \gamma D + \mu \\
S &= 1[S^* > 0].
\end{aligned}
$$

Without loss of generality, assume that $\gamma > 0$, i.e that the program raises the likelihood of employment. Now, consider the expectation of wages conditional on employment in the treatment group

$$
\begin{aligned}
E[Y|S = 1, D = 1] &= \alpha_1 + \beta + E[\varepsilon|D = 1, S = 1] && (3.1)\\
&= \alpha_1 + \beta + E[\varepsilon|\mu > -\alpha_2 - \gamma], && (3.2)
\end{aligned}
$$

and in the control group

$$
\begin{aligned}
E[Y|S = 1, D = 0] &= \alpha_1 + E[\varepsilon|D = 0, S = 1] && (3.3)\\
&= \alpha_1 + E[\varepsilon|\mu > -\alpha_2]. && (3.4)
\end{aligned}
$$

Note that randomization allows us to drop the conditioning on D when moving from (3.1) to (3.2) and from (3.3) to (3.4). The difference in means is thus given by

$$
\begin{gathered}
E[Y|S = 1, D = 1] - E[Y|S = 1, D = 0] \\
= \\
\beta + E[\varepsilon|\mu > -\alpha_2 - \gamma] - E[\varepsilon|\mu > -\alpha_2],
\end{gathered}
$$

which generally differs from β if ε and μ are correlated. Lee noted that identification of β is still possible if we could estimate

$$
E[Y|\mu > -\alpha_2, D = 1] = \alpha_1 + \beta + E[\varepsilon|\mu > -\alpha_2], \qquad (3.5)
$$

since (3.4) could be subtracted to yield a consistent estimate of β. The quantity in (3.5) is unobserved, however. The crucial observation is that the mean in (3.5) can still be bounded. This is because the observed mean in the treatment group can be written as

$$
\begin{gathered}
E[Y|S = 1, D = 1] \\
= \\
pE[Y|D = 1, -\alpha_2 > \mu > -\alpha_2 - \gamma] + (1 - p)E[Y|D = 1, \mu > -\alpha_2],
\end{gathered}
$$

where $p = \frac{Pr(-\alpha_2 > \mu > -\alpha_2 - \gamma)}{Pr(\mu > -\alpha_2 - \gamma)}$. The observed mean in the treatment group is a weighted average of (3.5) and the mean for a sub-population of "marginal" individuals ($-\alpha_2 > \mu > -\alpha_2 - \gamma$) who are employed because they participated in the program. Lee noted that if we can figure out who the marginals are, we can simply discard them and estimate the treatment effect for the subpopulation with $\mu > -\alpha_2$. This subpopulation are called "inframarginals" and consist of those who would be employed irrespectively of whether they participate in the program or not.

Unfortunately, it is not possible to determine which observations are "marginal" and which are "inframarginal". But if we know the fraction p of them in the population we can construct bounds by trimming the observed outcome distribution in the treatment group by the fraction p. Moreover, the trimming proportion p is given by

$$\frac{Pr(S=1|D=1) - Pr(S=1|D=0)}{Pr(S=1|D=1)},$$

where each quantity is identified from the data. Consequently, we can estimate the trimming proportion, p, and trim the top or the bottom of the observed outcome distribution in the treatment group by the fraction p. This allows us to put bounds on the quantity given in (3.5). Subtracting the observed mean in the control group will then allow the researcher to put upper and lower bounds on the treatment effect for the subpopulation of "inframarginals".

Lee's identification result apply to a much broader class of selection models. It relies on two assumptions, and none of the structure imposed above. Consider the following notation. Let $S(1)$ and $S(0)$ denote potential sample selection indicators for the treated and control state respectively. Furthermore, let $Y(1)$ and $Y(0)$ denote latent potential outcomes and let D be the observed treatment status. Lee's general result relies on (i) independence and (ii) monotonicity. Formally, these assumptions can be stated as follows:

Independence
$(Y(1), Y(0), S(1), S(0)) \perp D$

Monotonicity
$S(1) \geq S(0)$ with probability 1

The independence assumption holds by construction in the context of a randomized trial. The monotonicity assumption imply that the treatment can only affect sample selection in one direction. This essentially means that the treatment cannot cause the outcome to be observed for some units, and simultaneously cause the outcome to be unobserved for other units. I will come back to the independence and monotonicity assumptions throughout the analysis and highlight parallels between Lee's original result and the setting considered in this paper.

Remark
At first, it may seem trivial to extend Lee's approach to the DID setting. For example, one might be tempted to: (1) estimate the time trend using the change in the outcome experienced by the control group; (2) add the time trend to the treatment groups outcome distribution in the first period; (3) treat the distribution obtained from step (1) and (2) as the treatment groups counterfactual outcome distribution in the second period; (4) apply Lee's bounds. This procedure does not work.

The reason for this is twofold. First, the time effect is not point identified, unless there are no time-trends in the share of individuals with a non-missing outcome. This is because a naive comparison of the change in the outcome experienced by the control group cannot separate the time effect from changes in sample composition. Second, the group fixed effects are not point identified. This is because the outcome is only observed selectively. Therefore, the average outcome in the first period will consist of a group effect *and* selection bias.

3.3 Identification

This section derives bounds for the DID design. As before, suppose that we are interested in measuring the effect of a binary treatment D on some outcome Y. Let $Y(1)$ and $Y(0)$ denote the two potential outcomes in the treated and untreated state for the same unit. S is a binary indicator for sample selection, so that we only observe Y when $S = 1$. The observed outcome is $Y = S\{DY(1) + (1-D)Y(0)\}$.

I consider the case where we have two groups (treatment and control) who are observed in two periods. The data at our disposal can thus be divided into time periods and groups represented by the random variables T and G respectively. Here, G is a dummy variable for units in the treatment group and T is a dummy variable for the second period. In the second period, all units in the treated group are subjected to treatment. Hence, we have a "sharp" DID setting where $D = T \times G$.

Before presenting the main result, let me introduce some additional notation. For any random variable R, let $R_{gt} \sim R|G = g, T = t$. Where $\sim$ denotes equality in distribution. We therefore have that $E[Y|G = 1, T = 1] = E[Y_{11}]$, $E[Y|G = 0, T = 1] = E[Y_{01}]$, and so on.

I will now introduce two assumptions maintained throughout the analysis.

Assumption 1 (*selection equation*)
$S = 1[V \geq v_{GT}], with\, V \perp T|G$

Assumption 2 (*Increasing participation*)
$v_{11} \leq v_{10}, v_{01} \leq v_{00}$

Assumption 1 impose a threshold crossing model for selection (Vytlacil, 2002). Units are observed if the latent variable V crosses the threshold v_{GT}. V may be interpreted as a units "propensity" to be selected. Moreover, the threshold in this setting depends on both time and group. We therefore have that $E[S|T = 1, G = 1] = Pr(V \geq v_{11}|G = 1)$ and $E[S|G = 0, T = 1] = Pr(V \geq v_{01}|G = 0)$ and so on. This threshold crossing model is identical to the "treatment participation equation" in De Chaisemartin and d'Haultfoeuille (2015). Assumption 2 simply means that we are considering the case where more treated and control units are observed in the second period. The results will be equally valid if we assume "decreasing participation" ($v_{10} \leq v_{11}, v_{00} \leq v_{01}$).

It is important to realize that Assumption 1 may not be innocuous. The monotonicity assumption in Lee's original paper is actually implicitly imposed through the selection equation. Specifically, Assumption 1 implies that units can switch selection status in only one direction over time. Moreover, in Lee's setting, monotonicity only restricts the direction in which the treatment assignment can affect sample selection. Here, the combined effect of treatment *and* general time trends can only affect sample selection in one direction.

Concretely, suppose that we are interested in evaluating the effect of a job training program on wages using the DD design. Assume further that treatment is found to increase employment rates and that business cycle conditions improve over time so that more treated and controls are observed in the second period. Assumption 1 would then imply that every individual with a non-missing wage in the first period would also have been employed in the second period.

Furthermore, Assumption (1) and (2) can be used to divide the population of interest into four different subpopulations.

Treatment group inframarginals
$\{v_{10} \leq V,\ G = 1\}$

Treatment group marginals
$\{v_{11} \leq V < v_{10},\ G = 1\}$

Control group inframarginals
$\{v_{00} \leq V,\ G = 0\}$

Control group marginals
$\{v_{01} \leq V < v_{00},\ G = 0\}$

In repeated cross sections, the "marginals" are those for which the outcome would have been observed in the second period, but not in the first period. The inframarginals are those for which the outcome would have been observed in both time periods. The parameter of interest is given by:

$$\Delta = E[Y_{11}(1) - Y_{11}(0)|v_{10} \leq V] \tag{3.6}$$

The parameter Δ corresponds to the average treatment effect on the treatment group inframarginals. I focus on this parameter because the standard DID estimand gives the average effect of the treatment on the treated (ATT). Hence, any trimming procedure will estimate the effect of the treatment on some subset of individuals where $\{G = 1, T = 1\}$. Lee's trimming procedure is to discard those whose selection status if effected by the treatment, and focus on the inframarginals, who would have been observed under *any* treatment assignment. Therefore, it makes sense to focus on the inframarginals in the group where $\{G = 1, T = 1\}$, i.e the treatment group inframarginals.

Besides Assumptions 1 and 2, I will introduce a slightly modified version of the parallel trend assumption,

Assumption 3 (*Parallel trends for treatment and control group inframarginals*)

$$\begin{gathered} E[Y_{11}(0)|v_{10} \leq V] - E[Y_{10}(0)|v_{10} \leq V] \\ = \\ E[Y_{01}(0)|v_{00} \leq V] - E[Y_{00}(0)|v_{00} \leq V]. \end{gathered}$$

This assumption states that the untreated outcomes evolve in the same way over time in the subpopulation of treatment and control group inframarginals. Moreover, Lee's first identifying assumption is randomized treatment assignment. Note that this has been replaced by a modified version of the "parallel trend" assumption. The proof of partial identification will proceed in two steps.

First, I will prove that $E[Y_{11}(1)|v_{10} \leq V]$ can be bounded. I will then proceed and show that the counterfactual $E[Y_{11}(0)|v_{10} \leq V]$ is also partially identified. Second, I combine these results and provide bounds on Δ.

Proposition 1: Let $Y(1)$ be a continuous random variable. If assumption 1 and 2 holds, then $\Delta^1_{LB} \leq E[Y_{11}(1)|v_{10} \leq V] \leq \Delta^1_{UB}$ where

$\Delta^1_{LB} \equiv E[Y_{11}|S = 1, Y \leq t_{1-p_1}]$

$\Delta^1_{UB} \equiv E[Y_{11}|S = 1, Y \geq t_{p_1}]$

$t_q \equiv G^{-1}(q)$, with G the CDF of Y, conditional on $G = 1, S = 1, T = 1$.

$p_1 \equiv \frac{Pr(S=1|G=1,T=1) - Pr(S=1|G=1,T=0)}{Pr(S=1|G=1,T=1)}$.

Proof.

Lemma. Let Y be a continuous random variable and a mixture of two stochastic variables with cdfs M(y) and N(y) with known mixing proportion p. We then have that F(y) = pM(y) + (1-p)N(y). Let $G(y) = \max(0, \frac{F(y)-p}{1-p})$, which is the cdf of Y after truncating the p lower tail of Y. Then $\int_{-\infty}^{\infty} ydG(y) \geq \int_{-\infty}^{\infty} ydN(y)$. $\int_{-\infty}^{\infty} ydG(y)$ is a sharp (in the sense of Horowitz and Manski (1995)) upper bound for $\int_{-\infty}^{\infty} ydN(y)$.

Proof of lemma. See Horowitz and Manski (1995), Corollary 4.1.

Proof of proposition 1. It suffices to show that $\Delta_{UB}^{1} = E[Y_{11}|S=1, Y \geq t_{p_1}]$ is a sharp upper bound for $E[Y_{11}(1)|v_{10} \leq V]$. A similar argument for the lower bound will follow. Let F(y) be the CDF of Y conditioning on $S=1$, $G=1$, $T=1$. Let M(y) be the density of Y conditioning on $G=1$, $T=1$, $v_{11} \leq V < v_{10}$ and N(y) be the cdf of Y conditioning on $G=1$, $T=1$, $v_{10} \leq V$. Assumption 1 and 2 together with the conditional law of total probability imply that we can express the cdf of Y as $F(y) = pM(y) + (1-p)N(y)$ where $p = \frac{\Pr(v_{11} \leq V < v_{10}|G=1)}{\Pr(v_{11} \leq V|G=1)}$. Moreover, p is clearly point identified in the data since assumption 1 and 2 imply that

$$\frac{\Pr(v_{11} \leq V \leq v_{10}|G=1)}{\Pr(v_{11} \leq V|G=1)} \tag{3.7}$$

$$= \frac{\Pr(v_{11} \leq V|G=1) - \Pr(v_{10} \leq V|G=1)}{\Pr(v_{11} \leq V|G=1)} \tag{3.8}$$

$$= \frac{\Pr(S=1|G=1, T=1) - \Pr(S=1|G=1, T=0)}{\Pr(S=1|G=1, T=1)} \tag{3.9}$$

$$= p_1 \tag{3.10}$$

By the lemma,

$$\Delta_{UB}^{1} \equiv \frac{1}{1-p_1} \int_{t_{p_1}}^{\infty} ydF(y) \geq \int_{-\infty}^{\infty} ydN(y) = E[Y_{11}(1)|v_{10} \leq V] \tag{3.11}$$

$E[Y_{11}|S=1, Y \geq t_{p_1}]$ is therefore a sharp upper bound for $E[Y_{11}(1)|v_{10} \leq V]$.

Proposition 2: Let $Y(0)$ be a continuous random variable. If assumption 1,2 and 3 holds, then $\Delta^0_{LB} \leq E[Y_{11}(0)|v_{10} \leq V] \leq \Delta^0_{UB}$ where

$\Delta^0_{LB} \equiv E[Y_{10}|S=1] + E[Y_{01}|S=1, Y \leq c_{1-p_0}] - E[Y_{00}|S=1]$

$\Delta^0_{UB} \equiv E[Y_{10}|S=1] + E[Y_{01}|S=1, Y \geq c_{p_0}] - E[Y_{00}|S=1]$

$c_q \equiv G^{-1}(q)$, with G the CDF of Y, conditional on $G=0, S=1, T=1$.

$p_0 \equiv \frac{Pr(S=1|G=0,T=1)-Pr(S=1|G=0,T=0)}{Pr(S=1|G=0,T=1)}$.

Proof.

I only prove the result for the upper bound since a similar argument follows for the lower bound. From assumption 3, we have that

$$E[Y_{11}(0)|v_{10} \leq V] = E[Y_{10}(0)|v_{10} \leq V] + E[Y_{01}(0)|v_{00} \leq V] - E[Y_{00}(0)|v_{00} \leq V] \tag{3.12}$$

From assumption 1, we further have that

$$E[Y_{10}(0)|v_{10} \leq V] = E[Y|G=1, T=0, S=1] \tag{3.13}$$

$$E[Y_{00}(0)|G=0, v_{00} \leq V] = E[Y|G=0, T=0, S=1] \tag{3.14}$$

Equation (3.13) and (3.14) show that the first and third argument on the right hand side of equation (3.12) are point identified. The second argument $E[Y_{01}(0)|v_{00} \leq V]$ can be bounded. The argument is analogous to the proof of proposition 1. Let F(y) be the density of Y conditioning on $S=1$, $G=0$, $T=1$. Let M(y) be the density of Y conditioning on $G=0$, $T=1$, $v_{01} \leq V < v_{00}$ and N(y) be the cdf of Y conditioning on $G=0$, $T=1$, $v_{00} \leq V$. Assumption 1 and 2 together with the conditional law of total probability imply that we can express the cdf of Y as $F(y) = pM(y) + (1-p)N(y)$ where $p = \frac{\Pr(v_{01} \leq V \leq v_{00}|G=0)}{\Pr(v_{01} \leq V|G=0)}$. Moreover, p is clearly point identified since assumption 1 and 2 imply that

$$\frac{\Pr(v_{01} \leq V < v_{00}|G=0)}{\Pr(v_{01} \leq V|G=0)} \tag{3.15}$$

$$= \frac{\Pr(v_{01} \leq V|G=0) - \Pr(v_{00} \leq V|G=0)}{\Pr(v_{01} \leq V|G=0)} \tag{3.16}$$

$$= \frac{\Pr(S=1|G=0, T=1) - \Pr(S=1|G=0, T=0)}{\Pr(S=1|G=0, T=1)} \tag{3.17}$$

$$= p_0 \tag{3.18}$$

By the lemma,

$$\frac{1}{1-p_0}\int_{c_{p_0}}^{\infty} ydF(y) \geq \int_{-\infty}^{\infty} ydN(y) = E[Y_{01}(0)|v_{00} \leq V] \tag{3.19}$$

$E[Y|T=1, G=0, S=1, Y \geq c_{p_0}]$ is therefore a sharp upper bound for $E[Y_{01}(0)|v_{00} \leq V]$. Combine this result with equation (3.13) and (3.14) and the expression for the counterfactual mean in equation (3.12) and the result follows.

An immediate consequence of proposition 1 and 2 is that the estimand of interest, Δ, can also be bounded.

Proposition 3 If assumption 1,2 and 3 holds, then

$\Delta^1_{LB} - \Delta^0_{UB} \leq \Delta \leq \Delta^1_{UB} - \Delta^0_{LB}$

Proof.

This result follows immediately from proposition 1 and 2.

Note that there are some parallels to the original method suggested by Lee (2009). All that we have done is essentially applied Lee bounds twice. First, we put bounds on $E[Y_{11}(1)|v_{10} \leq V]$. This procedure can be put into Lee's original framework if we think about $F(Y|T=1, G=1)$ as the outcome distribution for the "treatment group" and $F(Y|T=0, G=1)$ as the corresponding distribution for the "control group".

Furthermore, when we put bounds on $E[Y_{01}|v_{10} \leq V]$ this can also be put into Lee's original framework. One would simply think about $F(Y|T=1, G=0)$ as the outcome distribution for the "treatment group" and $F(Y|T=0, G=0)$ as the corresponding distribution for the "control group". Once the bounds on $E[Y_{01}|v_{10} \leq V]$ have been computed, we used the parallel trend assumption to get bounds on the counterfactual outcome $E[Y_{11}(0)|v_{10} \leq V]$.

Moreover, if we were to assume "decreasing participation" ($v_{10} \leq v_{11}, v_{00} \leq v_{01}$) the treatment and control groups outcome distributions in the first period would instead be trimmed.[3]

[3]The method that I propose can also be generalized to the case when $\{v_{10} < v_{11},\, v_{00} > v_{01}\}$, i.e when the participation rate increases in the control group, and decreases in the treatment group. In this case, the control groups outcome distribution in the second period should be trimmed, and the treatment groups outcome distribution in the first period should be trimmed. The case when $\{v_{10} > v_{11},\, v_{00} < v_{01}\}$ can be handled by simply reversing the procedure described above, i.e trim the control groups outcome distribution in the first period and the treatment groups outcome distribution in the second period.

3.4 Estimation

Suppose that we observe an iid sample $\{(Y_i, G_i, T_i, S_i),\ i \in 1,...,n\}$. Then, estimators for the bounds $\Delta^1_{UB}, \Delta^1_{LB}, \Delta^0_{UB}, \Delta^0_{LB}$ can be constructed by simply replacing the population moments by the sample analog. For example,

$$\hat{\Delta}^1_{UB} \equiv \frac{\sum_{i=1}^n Y_i G_i T_i S_i 1[Y_i \geq \hat{t}_{\hat{p}_1}]}{\sum_{i=1}^n G_i T_i S_i 1[Y \geq \hat{t}_{\hat{p}_1}]},$$

where $\hat{t}_q$ and $\hat{p}_1$ are defined as

$$\hat{t}_q \equiv \min[y : \frac{\sum_{i=1}^n G_i T_i S_i 1[Y_i \leq y]}{\sum_{i=1}^n G_i T_i S_i} \geq q]$$

$$\hat{p}_1 \equiv \frac{(\frac{\sum_{i=1}^n G_i T_i S_i}{\sum_{i=1}^n T_i G_i} - \frac{\sum_{i=1}^n G_i (1-T_i) S_i}{\sum_{i=1}^n G_i (1-T_i)})}{\frac{\sum_{i=1}^n G_i T_i S_i}{\sum_{i=1}^n T_i G_i}}.$$

The procedure is analogous for $\hat{\Delta}^1_{LB}, \hat{\Delta}^0_{UB}, \hat{\Delta}^0_{LB}$.

3.5 Inference

Imbens and Manski (2004) derives confidence intervals for partially identified parameters. They assume that (i) the estimators for the bounds are $\sqrt{N}-$ asymptotically normally distributed, (ii) there are consistent estimators for the standard errors on the bounds.

In this setting, asymptotic normality follows immediately from the central limit theorem. Consistent estimates of the standard errors can be obtained by bootstrapping. The results from Imbens and Manski (2004) implies that confidence intervals for Δ can be formed using the following formula:

$$[(\hat{\Delta}^1_{LB} - \hat{\Delta}^0_{UB}) - \bar{C}_n \frac{\hat{\sigma}_{LB}}{\sqrt{n}}, (\hat{\Delta}^1_{UB} - \hat{\Delta}^0_{LB}) + \bar{C}_n \frac{\hat{\sigma}_{UB}}{\sqrt{n}}], \tag{3.20}$$

where n is the sample size and $\hat{\sigma}_{LB}$, $\hat{\sigma}_{UB}$ are the bootstrap standard errors on the upper and lower bound. Set $\bar{C}_n$ such that

$$\Phi(\bar{C}_n + \sqrt{n} \frac{(\hat{\Delta}^1_{UB} - \hat{\Delta}^0_{LB}) - (\hat{\Delta}^1_{LB} - \hat{\Delta}^0_{UB})}{\max(\hat{\sigma}_{LB}, \hat{\sigma}_{UB})}) - \Phi(-\bar{C}_n) = 0.95, \tag{3.21}$$

and the interval in (3.20) will contain the parameter Δ with a probability of at least 0.95.

Estimation of the standard errors on the bounds $\{(\hat{\Delta}^1_{LB} - \hat{\Delta}^0_{UB}), (\hat{\Delta}^1_{UB} - \hat{\Delta}^0_{LB})\}$ can be done via the bootstrap procedure described below.

1) Generate bootstrap samples $\left\{Y_{i,b}, T_{i,b}, G_{i,b}, S_{i,b}\right\}_{i=1}^n$, b = 1,...,B by sampling with replacement from the original data $\{Y_i, T_i, G_i, S_i\}_{i=1}^n$; for some large integer B.

2) Compute the trimming fractions $\{\hat{p}_{0,i}, \hat{p}_{1,i}\}_{i=1}^{B}$ for each bootstrap sample.

3) Find the relevant trimming quantiles $\left\{\hat{t}_{q,i}, \hat{c}_{q,i}\right\}_{i=1}^{B}$ for the density's $F(Y|G=1, T=1, S=1)$ and $F(Y|G=0, T=1, S=1)$.

4) Compute $\left\{\hat{\Delta}^1_{LB,i}, \hat{\Delta}^0_{LB,i}\right\}_{i=1}^{B}$ and $\left\{\hat{\Delta}^1_{UB,i}, \hat{\Delta}^0_{UB,i}\right\}_{i=1}^{B}$

5) Compute $\{(\hat{\Delta}^1_{LB} - \hat{\Delta}^0_{UB}), (\hat{\Delta}^1_{UB} - \hat{\Delta}^0_{LB})\}$

6) Compute the standard deviations of the bootstrap distributions obtained in (5) and use these to estimate the standard errors on $\{(\hat{\Delta}^1_{LB} - \hat{\Delta}^0_{UB}), (\hat{\Delta}^1_{UB} - \hat{\Delta}^0_{LB})\}$.

3.6 Conclusions

The problem of selection is pervasive in applied econometrics. Labor economists typically estimate regression model where log wages constitute the dependent variable. However, wages are only observed for those who are employed. These units typically constitute a selected subset. This has spurred a long literature with innovative procedures attempting to correct for sample selection bias. The parametric and semi-parametric methods proposed in the literature can be used to recover point estimates of the treatment effect. However, identification typically requires strong assumptions. This has led to a series of papers who argues that by sacrificing point identification, it is possible to derive informative bounds on the treatment effect of interest under much more plausible assumptions. An example of a bounding method that has taken off in the empirical literature are the Lee (2009) bounds.

This bounding procedure is however only applicable to randomized trials. This is problematic since most public policies are not implemented with an experimental design. This paper generalizes Lee's method to the DID design. Informative bounds on the average treatment effect on the treatment group "inframarginals" are derived. The parameter of interest is very similar to the quantity considered in Lee's original paper. It essentially restricts attention to units in the treatment group whose selection status is unaffected by the treatment and any general time trends. The parameter of interest in Lee's original paper was defined using units whose selection was unaffected by the treatment.

Identification is obtained by using a slightly modified version of the parallel trend assumption. In the typically DID design, the untreated outcome trend in the treatment and control group are assumed to be the same. Here, this needs to hold true for the subpopulation of treatment and control group "inframarginals". Finally, a slightly stronger version of Lee's monotonicity assumption is imposed. In Lee's original paper, treatment is assumed to unilaterally effect selection in one direction. Here, the combined impact of the treatment and any general time trend can only effect selection in one direction.

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