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Retrenchments in Unemployment Insurance Benefits and Wage Inequality: Longitudinal Evidence from the Netherlands, 1985–2000

Irma Mooi-Reci

Abstract: This study includes retrenchments in unemployment insurance (UI) benefits as an understudied mechanism to investigate possible explanations for wage inequality in the labor market. Using longitudinal data from the Dutch Labor Supply Panel (OSA) over the period 1985–2000, and adopting a quasi-experimental design, we not only extend current research by asking if restrictive changes in UI benefits affect re-employment wages, but also explore variation by the level, and eligibility conditions of UI benefits across gender and over time. Results from a series of fixed-effects models show that lower and shorter UI benefits lead to persisting wage inequalities over time. When investigating whether wage penalties vary across gender, we find that women experience the largest penalties. These findings provide evidence that these particular types of restrictions in UI benefits have likely increased rather than decreased wage inequalities between men and women.

Introduction

The impact of unemployment insurance (UI) benefits on (dis)incentives to re-enter the labour market is a persistent and controversial issue in contemporary research. One major question that has pre-occupied research in this area is: how and why do generous UI benefit systems affect workers' unemployment durations? Research on this topic has shown that UI benefits not only influence workers' job search incentives and strategies but also their unemployment durations which become significantly shorter when the level and duration of the UI benefits is restricted. Yet, if cut-offs in UI benefits stimulate workers' return to the labour market and improve their future employability through increasing work experience, we would expect that workers' re-employment wages over longer run should be affected in a positive way.

Empirical evidence, however, is contentious about the effects of UI benefit restrictions on workers' subsequent wages. Some studies demonstrate that lower and shorter UI benefits lead to lower and deteriorated re-employment wages due to a lack of time and economic resources (Burgess and Kingston, 1976; Ehrenberg and Oaxaca, 1976; Holen, 1977; Addison and Blackburn, 2000; Gangl, 2004, 2006; Shen, 2006; Petrongolo, 2007). Still other studies have found no significant results for any relationship between cut-offs in the UI benefits and re-employment wages (Classen, 1977; Blau and Robins, 1986; Kiefer and Neumann, 1989; Meyer, 1995; van Ours and Vodopivec, 2008).

Despite the advances in existing research, two central shortcomings remain. First, to assess the role of UI benefits on workers' subsequent wages more elaborate analyses that include longer observation periods are needed. In particular, we lack research that investigates the effects of UI benefits that may accrue over longer periods. As a result, the tradeoff between lower search intensities in the short-term and positive labour market wage outcomes in long-term has remained irreconcilable in existing literature. This tradeoff is important because it not only provides a more balanced view on UI benefit

Assistant Professor of Social Research Methodology, Vrije Universiteit (VU) Amsterdam, De Boelelaan 1081, 1081 HV Amsterdam, The Netherlands. Tel: +31-20 598 6780; Email: i.reci@vu.nl

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effects, but also creates a framework from which existing theory can be advanced and developed further. Second, the focus of previous literature on the short-term duration outcomes has meant that more specific questions about the heterogeneity and development of wage outcomes between men and women have been overlooked. Most empirical analyses produce aggregated results for men and women thereby missing any systematic examination of gender differences.

The central aim of this study is to advance existing research by providing new empirical evidence on the long-term wage trajectories of workers that have been affected by retrenchments in the level and durations of UI benefits. We then go beyond existing research to examine the gender-specific impact of UI retrenchments on workers' re-employment wages. By virtue of this, we link the ongoing discussion of gendered labour market outcomes to the persistent and controversial issue of the impact of UI benefits on subsequent wages. To identify the effect of cuts in the level and duration of UI benefits on subsequent re-employment wages, this study follows a twofold strategy. First, it takes advantage of two radical reductions in the level and duration of UI benefits enacted in the Netherlands during 1985 and 1987, respectively. This is necessary to disentangle the effects of UI benefits from other effects related to individuals' previous work history and earnings. Second, it uses longitudinal data from the Dutch labour force supply panel (OSA) over the period 1985–2000 with wage observations before and after the policy change to trace the long-term wage trajectories of workers that were affected by the policy change.

Results in this study demonstrate that men and women affected by cuts in the level and duration of UI benefits suffer from persisting wage penalties. During the first retrenchments women experience larger wage penalties compared with men affected by the same changes. These findings provide support for theoretical models that stress the unintended consequences of welfare state interventions that inhibit women's labour force participation, occupational achievement, and earning capacities (e.g., Orloff, 1996; Misra and Akins, 1998; Mandel and Semyonov, 2006).

The remainder of this article is organized as follows: the next section summarizes the context of the Dutch UI benefit system and highlights the implications of each major structural reform. Subsequently, labour market theories will be used to predict the relationship between UI benefits and re-employment wage developments. The next section describes the data and discusses the statistical methods used before presenting the empirical results. Finally, the last section summarizes the findings and concludes.

The Dutch UI Benefit System and the Changes in the UI Benefits

In the Netherlands, the Unemployment Insurance Act (Werkloosheidsverzekerings Wet) dates back to 1949. After the Second World War, a high labour demand, swift economic growth and low unemployment levels characterized the Dutch labour market. It was around the 1970s when unemployment started to become a problem and when the UI benefit system, like in many other Western countries, started to receive a critical attention. The reason for this negative attention was related to the high number of UI benefit claimants, an increase in unemployment rates and a low economic growth (Van Ours, 2003).

Before the 1980s, eligibility conditions were relatively simple. To become eligible for UI benefits, *bread-male-winners* should have worked for at least 13 weeks prior to their involuntary job interruption. If this condition was satisfied, individuals were entitled to UI benefits that amounted to 80% of their last earned incomes for a period of a maximum of six months (WRR, 1985). However, as result of the oil crisis in early 1980s and the poor economic situation, the Dutch government implemented some deep reconstructions in the benefit system that started in the mid-1980s. The first reconstruction was directed towards the level of the UI benefit. As of 1 January 1985, the level of UI benefits was brought back from 80 to 70 per cent of the last earned income. This cutback is often referred to as the 'price' policy-cut because it was meant to keep the welfare system affordable (Van Oorschot, 1998).

Two years later, in 1987, a second major change was directed towards reductions in the numbers of the UI benefit claimants which was referred to as the 'volume' policy-cut (SZW, 1998). This time the qualifying conditions for UI benefits were restricted for those men and women (as opposed to bread-male-winners) who had worked at least 26 of the previous 52 weeks (as opposed to 13 weeks) immediately prior to unemployment (Abbring, Van den Berg, and Van Ours, 2005). This condition was referred to as the 'week' condition. Under the new system, to become entitled to salary-related benefits a 'year' condition was added, namely individuals should have received incomes from employment in at least three out of the last five employment years. If this condition was satisfied, individuals were entitled to UI benefits, which amounted to 70 per cent of their last earned income which dependent on one's employment history lasted for a minimum of 6 months and a maximum of 5 years. For those not satisfying this last condition, the short-term UI benefits with a maximum duration of six months was introduced which amounted to 70 per cent of the statutory minimum wage.

Unemployment Benefits and Re-employment Wages: Some Essential Mechanisms

Drawing upon the broader body of sociological literature, we focus on two central theories that allow us to specify detailed mechanisms that drive wage inequalities ranging from micro- (individual) to macro- (state) level explanations.

Differences in Job Search and Re-employment Wages

To uncover the mechanisms that drive men's and women's job search behaviour during unemployment we turn to classic *job search theory* (Lippman and McCall, 1976; Mortensen, 1977). The standard job search theory portrays the dynamic job search of an unemployed worker according to a set of exogenously determined wages (Lippman and McCall, 1976; Mortensen, 1977), based on the assumption that all unemployed workers receive UI benefits with an infinite duration. In a competitive labour market, workers are assumed to have access to imperfect information about the job offers, which requires not only time to select but also money to cover the search costs (Halaby, 1988; Gangl, 2004). Search costs are not only related to the application and screening of information (i.e. direct search costs) but also to the foregone earnings when rejecting a job offer or foregone benefits when accepting a job offer (i.e. opportunity costs). An important implication from this theory is that generous benefits reduce the direct and opportunity costs, while at the same time create incentives to wait and search longer. From this point of view, longer search periods should predict better job matches, higher productivity and hence higher initial and subsequent wages on long term.

In a scenario when the level and duration of UI benefits is reduced, workers will have less money to sort out jobs. We expect that the response of the unemployed worker to restrictive policy changes is to adjust the reservation wage *further* downwards by selecting jobs with fewer entry barriers in exchange for a 'poorer' job match. In this case, adjustment of reservation wages for those treated reflects the selection of low-quality jobs and the poor match, whereas the reservation wages of those otherwise similar workers not affected reflects *only* the adjustment of the reservation wage due to the depreciation of their human capital. Based on these arguments, one testable prediction from the job search theory is that those affected by a reduction in the level or duration of the UI benefits will carry a higher penalty in their hourly re-employment wages relative to those not affected due to a poor job match *(the poor-match hypothesis)*.

Previous studies that have examined effects of stricter UI benefits on re-employment wages, find that lower levels and durations of UI benefits lead to lower re-employment wages due to costs that are related to industry dislocations and labor market segmentation (Burgess and Kingston, 1976; Ehrenberg and Oaxaca 1976; Gangl, 2006). In his studies Gangl (2006) demonstrates that unemployment has the potential to create a pool of previously unemployed workers concentrated in lower paid and less favorable jobs. In this study, we expect such effects to become magnified with the tightening of UI benefits. We assume that—driven by the pressure to find a job—affected workers will be pushed to change industries or sectors more often and willing to accept jobs with poor work conditions and fringe benefits. This in turn may increase the risks of dismissals and job mismatches over time. As result, those affected are expected to suffer more often from fragmented careers that lower the chances of a durable employment and may predict downward earning spirals over time. These arguments lead to the expectation that—on longer run—those affected workers may become trapped in a 'low-pay-no-pay circle' that may lead to *persisting* wage differentials over time relative to those not affected *(trap hypothesis)*.

Gendered Wage Outcomes and Welfare State Interventions

An understudied mechanism to explain gender-based inequalities in wages is the unintended role played by the welfare state via state interventions. In the literature that examines the issue of gender inequality in the labour market, there is often an implicit assumption that the welfare state enhances women's opportunities to participate in the labour market by providing a set of services and benefits such as childcare facilities, and/or maternity leave benefits. As also acknowledged by Mandel and Semyonov (2005, 2006), we argue that state interventions can actually have negative consequences on women's occupational opportunities (labour market, working times, and higher positions) and earnings. By creating a 'sheltered' labour market for women, the state produces and reinforces less favourable labour market outcomes for women, which in turn strengthens their power and role in the household, dependency on the male breadwinner and the traditional sex-segregation of market-family responsibilities (Sorensen and

McLanahan, 1987). The support of the state to allow women to work part-time and take extended care leaves preserves their role as mothers bound to the household and impedes any serious possibility for women to compete equally with men in the labour market (Blossfeld and Hakim, 1997; Aisenbrey *et al*., 2009). This mechanism is highly salient in the Dutch context, which is often characterized as a 'male-breadwinner' model (i.e. man in full-time employment and woman in a part-time position) (van Gils and Kraaykamp, 2008). Starting from the 1980s, women's employment has been concentrated in part-time jobs, interspersed with exits from the labour market during childbearing and rearing periods (van der Lippe and van Dijk, 2002).

An implication is that women's patchwork employment histories make them less likely to meet the more stringent eligibility criteria for UI benefits that are based on past employment and wage history (Hobson, 1990; Evertsson *et al*., 2009). In the current study, we extend the literature by arguing that by introducing UI retrenchments that do not recognize the often fragmented career paths of women, women are more likely to become ineligible for UI benefits or if they are deemed eligible, only for an inferior (short-term) benefit. This in turn impacts women's job search process and the ability to remain unemployed while searching for a good job match. This argument leads to the expectation that women affected by stricter UI benefit will be more likely to end up in jobs characterized by lower wages compared to men with more extensive work experience and benefit level and durations *(gender hypothesis)*.

Data, Empirical Strategy and Variables

Data Set

To test the preceding hypotheses, this study uses longitudinal data derived from the Dutch Labor Supply Panel (OSA) for 1985–2000. First in April 1985, and then from September 1986 every 2 years, standard interviews were used to collect retrospective data about labour market dynamics of the working population. The panel is a face-to-face biannual panel survey among a random sample of about 2,000 households in each wave resulting in information of about 13,000 respondents that participated multiple times in the panel.[1] These are sampled from the total number of households in the Netherlands. Household members between 16 and 65 years old are asked a series of detailed demographic, labour market, and income-related questions. Besides information on a range of labour market issues at the date of interview, the data set also includes retrospective data about maximum eight labour force changes of respondents between their last and current interview. In addition to the labour force information, starting from April 1985, this data set provides information on wages of workers at the time of interview. The summary statistics appear in Table A1 of the Appendix.

To study the effects of UI retrenchments on re-employment wage dynamics, the analyses are limited to persons who at the time of interview had become employed only after a spell of unemployment. The initial sample consisted of 3,408 person–biannual wage observations spread over 1,799 respondents who were employed at the time of interview. Due to within-group estimations, the analyses in this study were restricted to respondents with at least two wage observations. Therefore, the sample size declines to 2,887 biannual wage observations spread over 1,151 respondents, an average of 2.5 biannual wage observations per worker in the sample.

Empirical Strategy and Statistical Modelling

The empirical strategy for this study relies on a 'difference-in-difference' (DD) approach, which assumes that selection bias across treatment groups is time invariant and can be removed by taking differences over time. In addition, this approach assumes that effects of other time-varying factors are the same across groups. Comparing the wages before and after the UI reform for the treatment group reflects the re-employment wage change under the influence of the exogenous shock. The control group instead reflects the wages under the influence of changes in the labour market conditions only (Blundell and McCurdy, 1999).

In this study, we distinguish between the following control and treatment groups. The first treatment group that is affected by the policy change in 1985 comprises those registered unemployed that had worked for at least 13 weeks prior to becoming unemployed with a maximum daily wage ranging between 91 and 300 Guilders. When their wages were first observed in April 1985, these workers were subject to the pre-policy 1985 reform rules. But when they are next observed 6 months later (October 1986) they will be subject to post-policy reform rules. The control group in 1985 comprises those unemployed who receive 91 or less guilders maximum daily wages which lie at the bottom of the daily wage distributions and are therefore supported by the state. This latter group is therefore not affected by the retrenchments and contains a natural control group in our analyses.

The groups affected by the 1987 UI reform are distinguished by two central factors: (i) the week requirement, which is at least 26 weeks of work out of last 52 weeks; (ii) the year requirement, which is consecutive work experience and receipt of wages in at least 3 years out of last 5 years. The treatment group in 1987 comprises therefore those registered unemployed who (i) had worked less than 26 weeks out of the 52 weeks immediately prior to becoming unemployed, (ii) had an interrupted work experience prior to their unemployment and (iii) received no successive wages over the last 3 years. This group qualified for the short-term UI benefits with a replacement rate of 70 per cent of the statutory wage up to 6 months (as opposed to 70 per cent of last earned income with the duration of 2 years). When first observed in April 1985 and September 1986 this group was subject to the pre-policy 1987 reform rules. But when next observed in September 1988 this group is subject to post-policy reform rules. The control group in 1987 comprises those registered unemployed who had worked at least 26 weeks out of the last 52 weeks prior to becoming unemployed with at least 5 year work experience who did not meet the wage requirement of receiving wages in 3 out of 6 last years but rather only received wages in less than two of the last 6 years. This group continued to receive 70 per cent of the last earned incomes but because of a limited wage sequence did not experience an extension or a cut in the UI benefit duration. This latter group is therefore not affected by the retrenchments and contains a natural control group in our analyses.

Statistical Modelling and Variables

The dependent variable is the *natural log of hourly wages* for the respondent's current job, excluding overtime pay and overtime hours. The dependent variable is deflated by the ratio of mean wages earned in 1985 and only contractual hours are used. To investigate the policy effects using a 'difference-in-difference' approach, the re-employment wage effects using ordinary least square (OLS) can be written as follows:

$$\ln w_{it} = \beta' \boldsymbol{x}_{it} + \gamma_1 a_i + \gamma_2 p_t + \eta(ap)_{it} + e_{it} \qquad (1)$$

where $\ln(w_{it})$ is the natural logarithm of hourly wages of individual i in year t deflated to the 1985 prices by the retail prices index; $\mathbf{x}_{it}$ is a vector of labour market history and human capital controls; β refers to the vector of coefficients related to people's observable characteristics; and p_t indicates the period *after* which the policy changes were enacted. Furthermore, the value of γ_1 and γ_2 refer to the coefficients related to the main effects of the treatment and period variable. The value of η equals the estimated coefficient of the interaction term between the treatment variable and the period variable (ap), which captures the policy effect of UI retrenchments on those treated. In the equation, e_{it} refers to the equation error term. One main problem when estimating OLS models from a panel data is that the assumed independence of the error and the observable characteristics is likely to be violated and as a result incorrect standard errors are produced (Green, 2000). To avoid the problem with correlated errors within panels this study uses fixed-effects models with clustered standard errors. These models eliminate the influence of time-invariant unobserved heterogeneity and deal with the possible correlation that occurs when individuals are in the sample for several periods in a row. Re-employment wage effects using fixed effects modelling can be specified as follows:

$$\ln w_{it} = \beta' \mathbf{x}_{it} + \gamma_1 a_i + \gamma_2 p_t + \eta(ap)_{it} + \alpha_i + e_{it} \qquad (2)$$

where the value of a_i indicates those affected by the policy change α_i which is a time-invariant, individual-specific error. To differentiate between the policy effects for men and women separate models will be run. In addition, to guard against selectivity problems that arise when the wage information is not available for all workers, this study combines Heckman's two-step procedure (1979) with a procedure used by Vella and Verbeek (1994) to deal with the panel character of the data.[2] Table A2 of the Appendix provides estimates of the probability of belonging to a specific eligibility group, with one or more wage observations conditional on several observable characteristics.

To identify the treatment effects in this study, we construct two time-varying period—dummy variables where 0 refers to the period prior to the UI retrenchments and 1 to the period thereafter. The *attained level of education* distinguishes three categories: (i) elementary school level; (ii) lower and upper intermediate secondary school level; and (iii) college or university degree. The variable *work experience* reflects the potential years of working experience, a proxy for knowledge acquired at work. This variable results from the following subtraction: age—years of education—6—periods of unemployment or non-employment.

To control for re-employment wage penalties related to previous unemployment history, several variables appear in the model, including *unemployment spell*, or the most recent period of unemployment; *unemployment duration squared*, which can reveal whether any negative wage penalty related to unemployment spells diminishes or remains persistent over time. Other control variables are also included in the models such as, *age at employment, region of work* (1 = Groningen; 2 = Drenthe; 3 = Overijssel; 4 = Gelderland; 5 = Utrecht; 6 = Noord-Holland; 7 = Zuid-Holland; 8 = Zeeland; 9 = Noord-Brabant; 10 = Limburg;

11 = Amsterdam; Rotterdam; Den Haag; 12 = Flevoland), *contractual number of working hours* (12–40 h), *type of working contract at time of interview* (0 = temporary; 1 = permanent) and whether individuals worked in a *public or private sector* (1 = public; 0 = private). To disentangle the reform effects from compositional effects and changes in the socio-economic context *annual unemployment rates* and the *GDP per capita growth rate* are included in the models.

Empirical Results

A Descriptive Comparison between the Treatment and the Control Group

Before starting with more elaborate analyses, it is useful to assess the characteristics of the treatment and control groups before both the changes in 1985 and 1987. Descriptive statistics in Table 1 show that those treated by the 1985 UI reform (which lowered the benefit level), have slightly higher hourly wages, are more often married men with medium and high education and have higher cumulated work experience, compared to those not affected (control group). Those affected by the 1987 UI reform (which restricted the qualifying conditions) are more often prime-age working women with a low and/or medium education that earn relatively lower hourly wages compared to the control group. This group is also characterized by longer spells of unemployment and lower labor market experience. It is obvious that those affected by the 1987 UI reform are more often women with fragmented work careers that have not been able to build up a consistent and stable work career. Both the distribution of the observable characteristics over the treatment and control groups as well as their wage development before and after the changes in UI benefits seem similar, with exception of previous spells of unemployment, making it reasonable to suggest that the comparison group is a credible counterfactual estimate.

Tracing the Wage Differentials between the Treatment and Control Groups

To understand the underlying negative relationship between cuts in the level and duration of UI benefits and re-employment wages, we argued that job mismatches and shifts into jobs with less favorable work conditions

Table 1 Means and standard deviations of demographic and human capital variables for the treatment and control groups before the UI reforms, the Netherlands 1980–2000

	1985 UI Reform		1987 UI Reform	
	Control	Treatment	Control	Treatment
Log Hourly Wages *(in Guilders)*	1.48	1.53	1.52	1.49
	(0.10)	(0.08)	(0.31)	(0.45)
Female	0.66	0.34	0.39	0.57
	(0.37)	(0.52)	(0.49)	(0.50)
Age	43.52	43.31	38.18	37.25
	(9.64)	(11.54)	(7.85)	(13.34)
Marital status	0.83	0.86	0.82	0.71
	(0.37)	(0.38)	(0.38)	(0.45)
Low educated	0.54	0.44	0.42	0.45
	(0.49)	(0.50)	(0.50)	(0.50)
Medium educated	0.31	0.36	0.36	0.34
	(0.49)	(0.48)	(0.48)	(0.48)
High educated	0.15	0.20	0.22	0.21
	(0.39)	(0.38)	(0.41)	(0.41)
Recent months in unemployment	16.22	9.12	11.47	13.43
	(7.39)	(13.21)	(6.25)	(13.38)
Cumulated work experience (in months)	41.68	51.82	52.36	30.32
	(38.32)	(41.13)	(38.86)	(38.75)
Work Experience (in years)	21.17	21.00	24.31	20.22
	(15.21)	(13.15)	(10.31)	(9.52)
# Prior unemployment (>0)	1.39	1.12	1.16	1.32
	(0.36)	(0.75)	(0.37)	(0.74)

Source: Author's calculations using data from the OSA Supply Panels, 1985–2000.

Table 2 Unstandardized coefficients for the effect of UI reforms on individuals' log hourly wage 1 until 5 years after the UI reforms, from OLS estimates, the Netherlands 1980–2000

	1985 UI reform			1987 UI reform		
	Model 1 1 year	Model 2 3 years	Model 3 5 years	Model 4 1 year	Model 5 3 years	Model 6 5 years
Post-retrenchment period	0.213***	0.215***	0.219***	0.099*	0.175***	0.246***
	(0.009)	(0.009)	(0.008)	(0.057)	(0.048)	(0.045)
Affected/10	0.007***	0.007***	0.007***	0.018***	0.018***	0.018***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)
Period * Affected	−0.060***	−0.058***	−0.057***	−0.016***	−0.017***	−0.019***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	1.257***	1.254***	1.258***	1.480***	1.482***	1.480***
	(0.007)	(0.007)	(0.007)	(0.030)	(0.032)	(0.032)
Observations	749	947	1109	302	417	777
R^2	0.683	0.489	0.442	0.024	0.039	0.053

Source: Author's calculations using data from the OSA Supply Panels, 1985-2000.
*Note: Robust standard errors in parentheses; ***P < 0.001; **P < 0.05; *P < 0.1; two-tailed tests.*

are two possible mechanisms through which restrictions in the UI benefits may affect subsequent wages. That is, those affected by the UI retrenchments will have less time and money to sort out jobs that match with their previous experience and will therefore be more likely to occupy jobs with lower returns relative to those not affected.

To assess the mid- and long-run effects of restricted UI benefit levels, Table 2 reports OLS estimates of equation (1) from six baseline regression models that test for the re-employment wage effects one to five years after the policy change. The dependent variable is the *log of hourly wages* in the current job. The coefficients of interest are a full set of post-reform period × treatment status, which indicates the policy's effect on workers. At this stage, OLS estimates provide evidence regarding the one-year wage effects of the UI retrenchments, which is not possible using a fixed-effects model. To simplify the interpretation of the continuous treatment variables in both 1985 and 1987, we have divided those by 10.

The results from Models 1–3 in Table 2 reveal several interesting implications. Consistent with the theoretical expectations from our *poor match hypothesis*, we find evidence that a decrease in the replacement ratio of UI benefits by 10 Guilders yields a re-employment wage penalty of about 6 percentage points one year after the policy change. These penalties remain significant and persistent three years (5.8 per cent) and 5 years (5.7 per cent) after the implementation of the 1985 UI reform.

Results from Models 4–6 in Table 2 indicate that also the 1987 UI reform has led to significant negative effects on wages of those affected. The results imply that a restriction by ten additional weeks for the qualifying and base periods decreases workers' re-employment wages around 2 per cent several years after the policy changes. These penalties remain constant and do not diminish in longer run. Although, in contrast to earlier findings of Blau and Robins (1986); Classen (1977) van Ours and Vodopivec (2008) that find no wage effects of UI benefit restrictions, these results suggest that restrictions in UI benefits depress wages in a persisting rather than a temporary way. This can be related to the fact that affected workers experience a 'double' wage setback because they not only select low-quality jobs but also suffer from a poor match.[3]

But do these wage penalties persist after controlling for heterogeneity and modeling the probability that individuals would receive a sanction? To consider the possibility that the preceding results are driven by differences in the observable and unobservable characteristics that might influence differently the wages between affected and not affected groups, several fixed-effects regression estimates from equation (2) are conducted. The estimates limited to 5-year effects are shown for the 1985 UI reform in Models 1A–3A in Table 3, while the estimates for the 1987 UI reform are shown in Models 1B–3B in Table 3. To assess potential sample selection bias, the effects of each UI reform are modelled separately, including a separate correction term for each model. Robust standard errors correct for any pattern of correlation among errors within individual workers (Rogers, 1993). As in studies that investigate policy effects Type II errors are likely to increase, this

Table 3 Unstandardized coefficients for the effect of UI reforms on individuals' log hourly wage, 5 years after the UI reforms, from fixed-effects models, the Netherlands 1980–2000

	1985 UI Reform			1987 UI Reform		
	Model 1A	**Model 2A**	**Model 3A**	**Model 1B**	**Model 2B**	**Model 3B**
Policy measures						
Post-retrenchment period	0.055***	0.038***	0.037***	0.178***	−0.006	0.007
	(0.013)	(0.012)	(0.014)	(0.031)	(0.049)	(0.048)
Affected/10	0.011***	0.013***	0.011***	0.003***	0.006**	0.006**
	(0.004)	(0.004)	(0.000)	(0.001)	(0.002)	(0.002)
Period*Affected	−0.012***	−0.014***	−0.010**	−0.004***	−0.006**	−0.006**
	(0.004)	(0.004)	(0.004)	(0.001)	(0.003)	(0.003)
Employment and job characteristics						
Recent unemployment duration		−0.001	−0.001		−0.000	−0.000
		(0.000)	(0.000)		(0.002)	(0.002)
Unemployment duration squared		0.000	0.000		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Age		0.009***	0.004		0.033***	0.003
		(0.003)	(0.003)		(0.008)	(0.011)
Type of contract (=Temporary)		0.002	0.003		−0.000	−0.000
		(0.006)	(0.006)		(0.024)	(0.025)
Region of work		−0.002*	−0.002		−0.014*	−0.013
		(0.001)	(0.002)		(0.008)	(0.010)
Working hours		−0.004***	−0.004***		−0.005***	−0.005***
		(0.001)	(0.001)		(0.001)	(0.001)
Sector (= Public)		−0.010	−0.011		0.008	0.013
		(0.008)	(0.008)		(0.036)	(0.034)
Human capital characteristics						
Attained level of education		0.002	0.002		−0.162	−0.161
		(0.003)	(0.003)		(0.119)	(0.119)
Work experience (in years)		0.003**	0.003*		0.037***	0.038***
		(0.001)	(0.001)		(0.009)	(0.009)
Work experience squared		−0.000	−0.000		−0.001***	−0.001***
		(0.000)	(0.000)		(0.000)	(0.000)
Macro measures						
Annual unemployment rate			−0.046			−0.053
			(0.369)			(0.168)
GDP per capita growth			0.150			0.109**
			(0.104)			(0.045)
Lambda (λ)			0.011**			-0.001
			(0.005)			(0.029)
Constant	1.429***	1.198***	1.187***	1.582***	1.822**	1.383***
	(0.013)	(0.082)	(0.085)	(0.019)	(0.150)	(0.154)
Observations	1109	1109	1109	777	777	777
Number of respondents	663	663	663	300	300	300
R^2	0.232	0.362	0.357	0.085	0.690	0.691

Source: Author's calculations using data from the OSA Supply Panels, 1985–2000.
*Note: Robust standard errors in parentheses; ***P < 0.001; **P < 0.05; *P < 0.1; two-tailed tests.*

study increases the power of the statistical tests to a 10 per cent level to minimize Type II errors. This implies that results with a 10 per cent significant level will be addressed and interpreted in this study. For substantive reasons, I focus on the interpretation of the results in the final model (columns 3A and 3B), which control for labour market, job and human capital characteristics as well as macro variables.

Results in Model 3A and 3B indicate that wage differentials between those affected and not affected remain present in both the retrenchments after controlling for observable and unobservable characteristics.

Table 4 Unstandardized coefficients for the effect of UI reforms on individuals' log hourly wage, 5 years after the UI reforms, from fixed-effects models, by sex, the Netherlands 1980-–2000

	1985 UI Reform		1987 UI Reform	
	Model 1 Men	Model 2 Women	Model 3 Men	Model 4 Women
Post-retrenchment period	0.027*	0.074***	−0.023	0.056
	(0.015)	(0.018)	(0.045)	(0.083)
Affected/10	0.009**	0.023***	0.004**	−0.007
	(0.000)	(0.008)	(0.002)	(0.010)
Period*Affected	−0.006	−0.026***	−0.005**	0.006
	(0.005)	(0.009)	(0.002)	(0.011)
Lambda (λ)	0.002	0.020**	0.008	−0.016
	(0.005)	(0.009)	(0.034)	(0.055)
Constant	1.312***	1.003***	1.903***	3.927***
	(0.098)	(0.126)	(0.366)	(0.304)
Observations	659	358	413	207
Number of respondents	362	226	178	100
R^2	0.414	0.413	0.637	0.678

Source: Author's calculations using data from the OSA Supply Panels, 1985-2000.

Note: Variables that control for differences in employment and job characteristics, human capital characteristics, and macro-specific changes are also included in the analyses but not shown here.

*Note: Robust standard errors in parentheses; ***P < 0.001; **P < 0.05; *P < 0.1; two-tailed tests.*

More specific, results in Model 3A show that a 10 Guilders decrease in the monthly replacement rate relates to a 1.0 per cent decrease in the subsequent hourly wages 5 years after the policy change. This means that, a worker with an average pay of 25 Guilders[4] per hour would suffer 2 Guilders penalty per day or else 633 Guilders (= 288 Euros) penalty per year on the basis of full-time work (22 days per month). Model 3B, which estimates the policy effects of the 1987 UI reform, shows that after controlling for differences in individual, labour market characteristics, and socio-economic differences, workers suffer 6 per cent in their hourly wages for each 10 additional qualifying weeks in the eligibility criteria. This means that a worker with an average hourly wage of 25 Guilders suffers over 8 Guilders per day or else over more than 2,100 Guilders (=960 Euros) per year on a full-time basis. As argued earlier in the theoretical framework, an explanation for these large persisting penalties may be related to the job mismatching. Apparently choosing a job that does not fit to the previous experience becomes a dead end for workers affected by the UI retrenchments, leading to wage differentials that do not disappear over time.

The correction term in Models 1A–3A is positively significant, suggesting that those affected workers by the 1985 UI reform earn on average 1.1 per cent higher wages per hour than those not affected. In contrast, the lambda of the Models 1B–3B remains insignificant suggesting that the potential selectivity in the availability of wage information is uncorrelated with the processes that determine wages during the 1987 UI reform.

Gendered Wage Outcomes: Do women Incur Larger Pay Penalties?

The results presented thus far provide powerful evidence about the average effect of lower and shorter UI benefits on the log of hourly re-employment wages among those affected, but at the same time raise additional questions about the distribution of these effects among lines of gender. Next, the study considers separate models for men and women to investigate the distribution of wage penalties across gender. The fixed effect Models 1 and 2 in Table 4 display the estimation results for the 1985 UI reform, while Models 3 and 4 display the results for the 1987 UI reform. Earlier we argued that women's patchwork employment histories make them less likely to meet the more stringent eligibility criteria for UI benefits or if they are deemed eligible, they may only take up inferior (short-term) benefits. This in turn would impacts women's job search process and their opportunities to find a job with higher returns compared to men.

Results in Model 1 and 2 in Table 4, partly support argumentations from the *gender hypothesis* and offer some striking results. First, results indicate that women suffer by far the largest wage penalties during the restrictions in 1985. When comparing the magnitude of the effects between models for men and women results indicate that negative wage effects from the 1985 UI retrenchments disappear after 5 years for men while they remain persistent for women. Additional results (not shown here) suggest a significant negative gender effect during the 1985 restrictions ($\beta = -0.029$ per cent; t-value $= 4.86$) implying a further exacerbation of wage inequalities among men and women with an additional 2.9%. Wage effects related to the 1987 UI retrenchment show another picture. In particular, men continue to suffer higher wage penalties after 5 years, while for women the negative wage effects disappear. However, a cross-equation test of statistical significance (not presented here) shows no significant gender effects during the 1987 retrenchment ($\beta = 0.018$ per cent; t-value $= 0.83$). These results, suggest that the magnitude of wage penalties is highly dependent on the type of the state intervention, namely for women lower UI benefits induce higher wage penalties than shorter durations. In contrast, for men shorter unemployment durations generate more persisting negative effects. These results are consistent with the theoretical expectations and suggest that when women experience a cut in their UI benefit entitlements, they have a higher likelihood to accept jobs with lower prestige, rewards and wages relative to men which become wage traps over time. These results support earlier claims from feminist theories that argue that state policies or changes in institutions tend to reproduce gender inequalities by protecting and favouring mainly men while penalizing women's labour outcomes.

To test whether wage penalties are distributed more equally along lines of human capital, a three-way interaction between period, treatment group, and work experience was added to the model. Analyses (not shown here) demonstrate that men and women with higher work experience suffer larger penalties relative to those with lower work experience. Finally, to assess whether women are penalized by the sector where they take up jobs, a three-way interaction between period, treatment group and sector was added to the model. Analyses (not shown here) show that women working in the private sector suffer much higher penalties during both the 1985 and 1987 UI reform.

Summary and Conclusion

This study examined how and under which circumstances retrenchments in UI benefits lead to unequal patterns of wage developments across gender and over time. Treating retrenchments in UI benefits as natural experiments and using data from the Dutch Labor Supply Panel (OSA) for the period 1985–2000, this study developed several hypotheses about the initial and long-term wage outcomes of those affected workers. A central finding in this study reveals that restrictions in the UI benefit level, duration, and eligibility criteria affect workers' re-employment wages negatively. That is, a decrease in UI benefits by 10 Guilders relates to a 1.0 per cent decrease in the hourly re-employment wages or over 633 Guilders loss of incomes per year, whereas a restriction by 10 additional qualifying weeks relates to a 6 per cent decrease in the hourly re-employment wages or over 2,100 Guilders loss of incomes per year. It is striking to notice that the penalties related to the 1985 UI restrictions (that involved a cut in the UI benefit level) do not diminish for women after five years and are particularly significant for those women who are more skilled employed in the private sector.

But what explains these long lasting wage penalties? Using argumentations from job search theory and sociological literature, an explanation for these enduring penalties is related to the process of job mismatching. The persisting wage penalties point out that workers affected by UI retrenchments are more likely to accept jobs that do not match their attained education and work experience and fail to move into better located jobs in the primary sector. In addition, an explanation for the persisting gendered wage effects of UI retrenchments is (partly) consistent with theoretical models from feminist theories that predict larger gender pay gap inequalities due to the nature of benefits that favour more men's career histories. The results of this study suggest that the implemented UI retrenchments in the Netherlands may have been adequate to stimulate exit rates out of unemployment (Mooi-Reci, 2008), but they also appear to have damaged workers' and especially women's earnings prospects over time.

These findings also have implications for future research. First, because it appears that UI retrenchments create two-tiered systems, further research should assess with greater rigor whether restrictions in UI benefits lead to increased labour market segmentation by excluding certain occupations from the reach of those groups affected by a specific restriction. Researchers thus might investigate the hiring decisions and behaviour of firms during periods marked by changes in the UI benefits to assess whether and how such restrictions contribute to greater labour market segmentation. Second, this study has focused only on men and women that managed to find a job after the changes in the UI benefits, excluding those with zero wages from the analyses. As a consequence the study provides only limited view on the effects of these changes in general. There is, however, existing evidence in the Netherlands showing that these particular changes in the UI benefits have led to not only significant shorter unemployment durations but also to poor labor market outcomes particularly for women (Mooi-Reci, 2008). However, the question of what is best: a mismatched job or an unemployment spell still remains unexplored.

Third, though it is beyond the scope of this study to examine monitoring measures for UI benefits, additional research should investigate and assess more broadly the efficiency of restricting UI policies and monitoring systems. Such an investigation might find effective ways that, on the one hand, stimulate labour market dynamics and, on the other hand, protect workers from the insecurities involved with periods of unemployment. Finally, this study assumes a proper job match is the product of higher subsequent wages; additional research could address the process of job matching and subsequent occupational mobility more carefully.

Notes

1. The OSA-panel accounts an attrition rate of around 35 per cent.
2. This combined procedure is estimated using a random-effects panel probit model in the first selection stage, which includes also the mean of time-varying variables as additional regressors. In doing so, the correlation between two successive error terms for the same individual remains constant over time.
3. To control for possible anticipation effects, interaction effects between the periods *before* the policy changes with the treatment groups has been introduced. When doing so, these interaction terms remained not significant. The lack of any decline in wages before the changes in the UI benefits and the significant declines thereafter show that it has not been likely that such changes have affected men's and women's search behaviour before the changes were enacted.
4. 2.20 Guilders = 1 Euro.

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Appendix

Table A1 Means and standard deviations of summary statistics

	Mean (SD)
Dependent variable	
Log of hourly wages in guilders	1.66 (0.61)
Treatment groups	
Treatment group in 1985 ($n=937$)	0.68 (0.33)
Control group in 1985 ($n=465$)	0.32 (0.61)
Treatment group in 1987 ($n=556$)	0.70 (0.23)
Control group in 1987 ($n=232$)	0.29 (0.70)
Labour market history measures	
Most recent unemployment spell (in months)	12.80 (12.40)
Most recent unemployment spell squared	31.78 (31.21)
Region of work at time of interview	8.42 (2.06)
Type of contract at time of interview	1.37 (0.91)
Working hours	34.4 (10.3)
Sector (1 = public)	0.34 (0.47)
Human capital measures	
Attained years of education	11.72 (3.24)
Work experience (in years)	24.72 (11.61)
Demographic measures	
Age (16–65 years)	41.23 (10.52)
Married/Cohabiting	0.83 (0.21)
Had kids (1 = had kids)	1.26 (0.44)
Number of kids	1.37 (1.39)
Macro-economic measures	
Annual unemployment rate	0.071 (0.01)
GDP per capita growth	0.054 (0.01)
Person biannual wage observations	2,887
Number of workers observed	1,151

Source: Author's calculations using data from the OSA Supply Panels, 1985–2000.

Table A2 Random effect probit estimates for the probability of being eligible for UI benefits and having more than one wage observations, by separate UI reforms, The Netherlands 1980–2000

	1985 UI Reform Model 1	1987 UI Reform Model 2
Post-reform period	0.738***	0.062
	(0.195)	(0.179)
Most recent unemployment spell (in months)	0.151***	−0.002
	(0.013)	(0.014)
Attained years of education	0.214***	−0.076
	(0.025)	(0.048)
Work experience (in years)	0.128***	−0.002***
	(0.018)	(0.001)
Region	−0.041	−0.009
	(0.029)	(0.018)
Permanent contract	−0.317**	−0.245**
	(0.160)	(0.011)
Sector (public=1)	0.383***	0.100
	(0.133)	(0.080)
Age at employment	−0.062	−0.024
	(0.228)	(0.069)
Additional variables in the selection equation		
Unemployed two waves earlier (T–4)	−0.050**	−0.000
	(0.025)	(0.000)
Married	−0.034	−0.320
	(0.157)	(0.268)
Had kids	0.188*	−0.381
	(0.109)	(0.340)
Number of kids	−0.098***	−0.077*
	(0.034)	(0.041)
Average age throughout the panels, by respondent	−0.136***	0.045***
	(0.019)	(0.017)
Average employment duration, by respondent	0.062***	−0.038
	(0.012)	(0.026)
Average unemployment duration, by respondent	0.083	−0.146***
	(0.086)	(0.056)
Average number of unemployment spells, by respondent	−1.353***	0.317**
	(0.475)	(0.157)
Constant	−3.054***	−1.441
	(0.455)	(0.905)
Observations	1018	1390
Respondents	585	718
Log Likelihood	−436.96	−194.79
Wald Chi^2	153.95	117.18

Source: Author's calculations using data from the OSA Supply Panels, 1985–2000.

*Note: Robust standard errors in parentheses; ***$P<0.001$; **$P<0.05$; *$P<0.1$; two-tailed tests.*