Unemployment Scarring by Gender: Human Capital Depreciation or Stigmatization? Longitudinal Evidence from the Netherlands, 1980-2000

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Abstract

Using longitudinal data from the Dutch Labor Force Supply Panel (OSA), this article examines how unemployment scarring (i.e., wage setbacks following unemployment) and its underlying mechanisms operate across gender in the Netherlands over the period 1985-2000. A series of fixed effect panel models that correct for unobserved heterogeneity, reveal a notable disparity in unemployment scarring by gender. Interestingly, while unemployment scarring is short-lived and partly conditional upon human capital differences among women, it is strongly persistent among men and contingent upon old age, ethnicity, and tight economic conditions. Our findings provide new evidence regarding unemployment scarring by gender while they support the hypothesis that among women the effects of unemployment scarring are predominantly driven by human capital depreciation, while among men stigma effects dominate.

Keywords: Unemployment scarring; gender; stigma; human capital depreciation; wage inequality.

1. Introduction

The link between unemployment and subsequent economic disadvantage has been at the heart of public and academic debates over more than three decades. Early unemployment has been found detrimental for workers' future employment opportunities because it reduces the future likelihood to be hired, and inflicts a setback in re-employment wages that perpetuates long after the initial unemployment occurrence (Arulampalam 2001; DiPrete 1981; DiPrete and McManus 2000; Gangl 2004, 2006; Gregg 2001; Jacobson et al. 1993; Kuhn 2002; Moore 2010; Ruhm 1991). This wage setback is referred to in the literature as *'unemployment scarring'*.

While unemployment has been increasingly recognized as a disruptive event that may become the onset of adverse wage trajectories and inequalities in the labor market, surprisingly little is known about how unemployment processes operate across gender. The singular focus of previous studies on the scarring effects among men, has mostly led to the omission of women from these analyses. As we already know, changes in the employment structure – as result of skilled-biased technological change and globalization – have influenced employment opportunities and dynamics among both men and women since the 1980s (Autor 2010; Buchmann and DiPrete 2006; Farber 2011). To the extent that these changes have influenced disproportionally the risk of unemployment among disadvantaged groups (such as women or older workers), unemployment is no longer a disruptive event in the employment careers of men, but has become a lived experience in the lives of many women. Yet, with few exceptions (Albrecht et al. 1999; Kuhn 2002; Gangl 2006; Wilkins and Wooden 2013), evidence has remained scarce about how the size and strength of unemployment scarring among women compares to that of men. Consequently, the question of whether and how unemployment scarring varies by gender still remains not yet fully assessed.

In addition, relatively little attention has been devoted to understanding the conditions under which unemployment scarring operates across gender. For instance, existing research has offered two key mechanisms underlying the process of unemployment scarring. A *resource-related* mechanism that links scarring to workers' loss or depreciation of skills during periods of unemployment; and a *signaling-related* mechanism that links unemployment scarring to the stigma attached to it. These mechanisms may work out differently amongst men and women because of differences in the accumulation of human capital and the different gender prejudgments that surround employers' hiring decisions. Yet, how human capital and signaling mechanisms reduce or introduce scarring across gender has received little systematic attention. Do these mechanisms govern the scarring process similarly across gender or is this process contingent upon individual and contextual level variation?

These questions fall within and contribute to the broader sociological debates about the gender wage gap and will be the core of our study, which adds two major contributions. First, we advance theory on this topic by investigating the heterogeneous effects of unemployment scarring across men and women of different social groups and in different economic conditions. Similar to Omori (1997), we argue that if stigma drives unemployment scarring, then scarring effects should exacerbate in specific (tight) labor market situations and among specific (disadvantaged) groups (e.g., gender, age, parenthood, and ethnicity). By contrast, little or no contextual variation would indicate that human capital depreciation effects dominate. This distinction helps us understand the gendered disparity in unemployment scarring.

Second, we extend existing research by including multiple dimensions of unemployment – previous unemployment occurrence, repetition and duration – to investigate how each influences men's and women's re-employment wages. In doing so, our study provides a more nuanced view about the effects of unemployment and extends research that has mainly focused on singular dimensions of unemployment. We also assess the full magnitude of unemployment scarring, by combining the various unemployment dimensions into a single 'unemployment index'. This approach provides a comprehensive and statistically powerful measure of the unemployment scarring effects, which is new in existing research.

We test our hypotheses about unemployment scarring by gender among a sample of workingmen and women in the Netherlands over a twenty-year period (1980-2000). The Dutch case is interesting because of its unique labor market structure (with a high share of women working in part-time jobs), high employment protection, and the prevailing work culture that adds contrasting evidence and additional insights on the processes underlying unemployment scarring by gender. Our analyses rely on a rich and comprehensive longitudinal dataset, the Netherlands Labor Supply Panel (OSA) spanning over the period 1980-2000 with a biennial panel design. The analytical strategy in our study is to apply the same model to a sample of workers who differ only with respect to their route into employment: one group came into employment via a spell of unemployment and the other group via employment. We use fixed-effects panel models that correct for time-constant unobserved heterogeneity to analyze the effects of unemployment and to disentangle human capital depreciation from stigma effects on men's and women's re-employment wages.

2. Theoretical Background and Expectations

Evidence in different countries has shown that unemployment leaves significant scars in the re-employment wages of the previously unemployed such that wage setbacks remain largely persistent after the initial unemployment instance (Gangl 2004; 2006 Gregg and Tominey 2004; Ruhm 1991).

Several theories are used to explain these group differences in wages, two of which are the most prominent and will guide us through the development of our hypotheses.

2.1 Human Capital Depreciation and Unemployment Scarring

The first "resource-specific" explanation, originating from the human capital theory (Becker 1964, 1993), emphasizes that wage losses following an unemployment spell reflect the process of human capital depreciation and skill relocation. Theory suggests that human capital can be divided into a *generic* part, which is acquired through education and is transferable across employers, and a *specific* part, which is acquired through acquisition of job-specific human capital through experience in a specific firm or sector and is non-transferable across employers (Becker 1993). For both men and women, a direct implication of this distinction is the expectation that interruption of job specific training may lead to lower levels of productivity, both instantaneously and in the long run. In particular, skills related to specific occupations, firms, or industries are lost when unemployment occurs. By contrast, generic human capital depreciates over longer spells of unemployment.

Existing studies show that the velocity with which human capital depreciates depends on the duration, repetition, and recency of unemployment spells. For instance, a single occurrence of unemployment leaves a significant scar on re-employment wages (Jacobson, LaLonde, and Sullivan 1993), which becomes larger with more frequent (Stevens 1997) and longer unemployment spells (Gangl 2004; Gregory and Jukes 2001). While this process is evident across gender, literature suggests two major factors that lead to a diverging human capital depreciation among previously unemployed men and women. First, given the erratic nature of women's labor market trajectories in the Netherlands, which include more frequent interruptions due to periods of childbearing and caring, women – more than men – accumulate a reduced amount of work experience (Datta Gupta and Smith 2002; Gangl and Ziefle 2009). Above and beyond the child-related job interruptions, women may be subjected to unemployment periods that accumulate existing periods out of work. Secondly, and related to this is the fact that longer and more frequent spells of unemployment increase the risk of switching into jobs that are located in other industries or sectors than those predating unemployment. From existing studies, we know that women have higher risks of settling for jobs in other sectors or taking up lower positioned jobs after a job interruption compared to men (Aisenbrey et al. 2009; Engelbrech 1997). We also know that effects of unemployment scarring are short-lived with re-employment in occupations, sectors or industries that are similar to that before unemployment (DiPrete 1981) and long-lasting when re-employment is located in jobs outside of the worker's discipline or sector (Kuhn 2002; Mühleisen and Zimmermann 1994; Stewart 2007). These findings suggest that human capital depreciation effects should be more prominent among previously unemployed women than men. If this holds, unemployment scarring that arises due to human capital depreciation or from sector or industry relocations should largely diminish among women after we control for such differences. Following these considerations we expect that:

Hypothesis 1: Unemployment occurrence, duration, and repetition, alone or in combination, will impose a negative effect on re-employment wages; effects which should be higher among women than men.

2.2 Stigma and Unemployment Scarring

A second "stigma-related" explanation, originating from the signaling theory (Spence 1973), suggests that group differences in wages arise from the information asymmetry surrounding the hiring process. Signaling models posit that employers' hiring decisions are taken against a background of uncertainty about a worker's productive capabilities. Whenever such uncertainty exists, employers rely on the observable characteristics of the worker such as age, ethnicity, and family situation, but also on their past employment history, all of which serve as a statistical screening device in the hiring process (Lockwood 1991; Eliason 1995). Unemployment stigma may be invoked by gender when unemployed men and women are treated differently in the hiring process. In the context of our study, unemployment stigma reflects employers' culturally produced notions about which gender is more likely to experience a certain type of job interruption (i.e., periods of child rearing or unemployment). We anticipate that the expansion of the Dutch labor market with most women working part-time jobs has changed employers' expectations towards working men and women. Specifically, while women are more often expected to take up a part-time job and to experience more fragmented careers (due to childrearing), men are expected to have a full-time and continuous employment career (Mills and Täht 2010). This implies that deviating from these culturally defined pathways would carry more of a stigma for men than for women. Departing from the findings of prior research, stigma effects are likely to exacerbate when combined with characteristics that activate certain stereotypes. In what follows, each of these characteristics will be described in more detail.

2.2.1 Variations of Unemployment Stigma across Age, Parenthood, Ethnicity and Business Cycles

To reveal gendered prejudgments in the hiring process, we examine unemployment scarring as it intersects with a series of individual and economic characteristics. That is, if scarring arises through stigma then scarring effects on re-employment wages should vary contingent upon specific (disadvantaged) group characteristics or economic conditions that reinforce employers' pre-existing unemployment stereotypes and influence their hiring decisions. If these negative effects are gendered, then one would expect to see *different* patterns of wage setbacks among men and women within a specific context; setbacks that move above and beyond the re-employment wage loss due to foregone skills and experience. In what follows we will delve into existing research and focus on individual and economic characteristics that may activate certain stereotypes and influence firms' hiring decisions.

We start with *age* as a potential stigmatizing characteristic. Existing studies find that workers who experience unemployment at older ages (> 50 years) suffer more severe wage setbacks from unemployment scarring compared to their younger counterparts (Arulampalam 2001; Evangelist 2013; Gregg 2001; Stevens 1997; Manzoni and Mooi-Reci 2011). An explanation for this relates to the reluctance of employers to hire older workers because they attribute periods of unemployment to one's failure, and when they do hire, it comes at the cost of lower wages (Wolbers 2008). Another explanation is that older workers who are trained for more traditional occupations are more likely to be hired in jobs that do not require periods of retraining on-the-job and thereby pay less (Kuhn 2002). In particular, with shorter periods of retraining in mind, employers are more likely to hire younger workers because they have acquired skills that are more adaptable for newly created jobs. Stigmatizing arguments surrounding employers' hiring decisions are less prominent among men who experience unemployment at younger ages (below the age of

25). This is because employers expect a ‘job shopping’ behavior in the initial stages of young men’s careers (Arulampalam 2001; Kuhn 2002). Likewise, we expect that culturally driven expectations about women’s standard employment trajectories will weaken the stigma during ages of childbearing and rearing. For instance, it is well established that Dutch women increasingly participate the labor market after obtaining their education degree, then move (briefly) out of the labor market during periods of child-bearing and rearing to return back to employment after their children reach the age to attend the school (OECD 2012; Mills and Täht 2011). As fragmentation of women’s careers is more common around the ages of 30 and 35, unemployment spells during these ages will be less of a negative signal and thereby influence employers’ hiring decision and subsequent wages less negatively. This in contrast to men who would be more penalized for deviating from ‘the’ standard employment trajectory. Overall we expect that:

Hypothesis (2a): Previously unemployed workers at older ages will experience stronger wage penalties compared to their younger counterparts; effects which should be higher among older men than similar older women.

Another potential stigmatizing characteristic is one’s *parenthood status*. Existing research confirms that employers’ hiring decisions are strongly influenced by one’s parenthood status (Budig and England 2001; Budig and Hodges 2010; Correll et al. 2007; England 2005; Gangl and Ziefle 2009). The literature suggests that employers discriminate against working mothers in terms of hiring decisions, promotion opportunities, and wages, but not against fathers. According to Correll et al. (2007), employer discrimination relates to the cultural understanding of the motherhood role. Especially in societies in which women are expected to take care of their children, employers – unconsciously – prejudge working mothers as less productive and less competent (Ridgeway and Correll 2004). It is especially this cultural expectation that leads to discrimination against mothers in hiring, promotion, and wage decisions. By contrast, ‘good’ fathers are expected to work hard and are seen as more committed to their work. Fathers therefore generally experience an advantage over men without children. A recent study by Budig and Hodges (2010) reveals that in the United States the motherhood wage penalties vary in regards to the age(s) of the woman’s children. Specifically, the study finds that the older the children, the higher the hourly wage penalty experienced by the mother, because of foregone human capital accumulation. The motherhood wage penalty is also highly present in the Netherlands where the penalty reaches almost 20 percent and is among the highest in Western societies (Misra et al. 2007). In this study, we extend this evidence by asking whether the combination of being unemployed *and* a mother exacerbates existing gender inequalities even further. We argue that mothers who experience unemployment, may be doubly stigmatized by employers for not only being a ‘bad’ but also a potentially ‘unproductive’ worker. Given the arguments above, this would not hold for fathers. In sum, we expect that:

Hypothesis (2b): Previously unemployed mothers will experience stronger wage penalties compared to previously unemployed fathers.

A worker’s *ethnicity* is another characteristic that may negatively influence employers’ hiring and wage decisions. It is well established that hiring decisions are governed by stereotypes, which enable employers to organize, rank, and interpret the amount of data provided by job applications (Fiske 1998; Moreno and Bodenhausen 1999). Especially when employers have little information about the productivity of an applicant from a specific ethnic group they are more likely to discriminate on statistical grounds. For instance, the ambiguity regarding the recognition of the educational achievements or titles that are specific to certain occupations generate difficulties when it comes to evaluating working skills of non-natives in the hiring process (Chiswick 1991). In addition, language deficiencies in speaking and writing fluency may raise doubts about the ability of non-native workers to perform in high-skilled jobs (Chiswick 1991; Dustmann and Fabbri 2003). Native workers who are more fluent of the host country language will be able to better communicate and promote their qualifications and skills (Dustmann 1994). These ambiguities may translate into discriminatory practices and thereby lead to wage differentials between native and non-native workers.

Wage differentials based on one's ethnicity may exacerbate with periods of unemployment. For instance, the occurrence, duration and frequency of unemployment altogether, may confirm an employer's pre-existing doubts about the performance and/or future productivity of an immigrant worker. These stereotypes do not necessarily lead to the exclusion of previously unemployed immigrant workers from the labor market, rather they are used by the employers to justify the concentration of immigrant workers into lower positioned jobs that are less demanding and thereby pay less (Browne and Kennelly 1999; Moore 2010). According to theories of social closure (Blumer 1965; Tomaskovic-Devey 1993) employer's stereotypes are based on the normative ideas that they share about the economic and social position of the group in which they belong. For instance, employers who belong to a group with higher economic and social position (i.e., the native group), will develop more negative expectations about the productivity of an immigrant worker who originates from a group with lower socio-economic conditions and more common unemployment spells (i.e., immigrants from poorer countries). This means that the closer the social distance between people or groups – in color, appearance and socioeconomic background – the more sympathetic people will be at each other (Blumer 1965; van Tubergen et al. 2004). Likewise, stereotypes that employers have about immigrants may differ across gender. Purely based on their appearance, previously unemployed immigrant male workers from poorer countries are more likely to be labeled as unreliable or unproductive. These inaccurate perceptions by employers can lead to poor job matches, lower productivity and eventually lower wages among previously unemployed male immigrants. Conversely, immigrant women from same poorer countries are more likely to be perceived as nurturing, considerate and more 'obedient' to a firm's rules (England 1992; Tomaskovic-Devey 1993). As employers perceive these traits as less of a threat, they should be less penalizing in terms of re-employment wages relative to their male immigrant counterparts.

To put this in perspective, in the Netherlands the composition of the immigrants has been dominated by four immigrant groups originating from: Suriname, Turkey, Morocco and the Dutch Antilles. In the 1980s these groups represented about 79 percent of the non-western foreign-born population in the Netherlands while this percentage dropped to about 44 percent in the 1990s (CBS, Statline 2013[1]). Different from immigrants from Suriname and the Dutch Antilles that shared a colonial history with the Dutch, workers from Morocco and Turkey were attracted to fill the labor shortages in the manufacturing and services sector. Specifically, male workers from Morocco and Turkey were mainly engaged in low-skilled jobs that were concentrated in manufacturing, cleaning services or metal, timber and food industries (Bevelander and Veenman 2002). Departing from the abovementioned arguments we expect that:

Hypothesis (2c): Previously unemployed non-native workers will suffer stronger wage penalties than native workers; this effect should be higher among men than among equivalent women.

Stigma effects from unemployment will vary with the *business cycle*. In times of economic downturn, with high unemployment rates and few open vacancies, the probability of finding a job decreases. Following arguments from the economic literature, a spell of unemployment incurred during a recession is less of a negative signal to employers than one experienced during an economic upturn (Blanchard and Diamond 1994; Lockwood 1991; Omori 1997). The idea is that when screening job applications, employers will attribute the occurrence of unemployment to firm closures and reorganizations that take place during economic downturns. Consequently, if stigma effects exist, unemployment scarring should be higher when unemployment is encountered during periods with lower unemployment rates than in periods with higher unemployment rates. Following these arguments we posit that:

Hypothesis (2d): Previously unemployed workers will suffer stronger wage penalties when their unemployment occurs during economic upturns compared to those otherwise.

3. Data, Measures and Method

3.1 Data

The data for our study come from the Netherlands Labor Supply Panel (OSA). The OSA panel is the longest and oldest household panel in the Netherlands (Allaart et al. 1987; NIWI 2000; Abbring et al. 2002) and is comparable to other European specific household panels such as the British Household Panel (BHPS) in the UK or the German Socioeconomic Panel (GSOEP) in Germany. The panel study is continually refreshed and is targeted at a representative sample of 4,000 to 5,000 respondents in each wave. The first wave was interviewed in 1985 (with a retrospective component reaching back to 1980) and then re-approached in 1986 with further biennial waves until 2000.

Panel attrition over the nine waves (around 39%) was compensated by adding fresh respondents using a sampling design that was stratified by region, gender, age, and education (NIWI 2000). The data is unique such that it includes a wealth of information about respondent's family background, education, and incomes. In particular, at each wave, the current earnings situation of the respondent was reported.

The respondents in our analyses were between 20 and 54 years of age, employed at the moment of interview, and have a valid wage observation at the time of interview. We excluded wage observations in the bottom 1st percentile (N = 235), which are likely to be produced by measurement problems and can be influential. Finally, we excluded wage observations from those who became unemployed due to seasonal employment (N = 424) as their job loss contradicts the definition of unemployment as an involuntary event resulting from exogenously determined firm decisions.

Given these requirements, we start with a sample containing 20,925 wage observations over 9,490 workers between 20 and 54 years old. In order to apply fixed-effects modeling (which we will explain in the next section in more detail) we need at least two wage observations per worker. This means that workers with less than two wage observations are dropped from our analyses leaving us with an effective sample of 10,897 valid wage observations over 4,815 workers, which is an average of 2.3 wage observations for each individual worker. In Table 1A of Appendix A, we cross tabulate workers' consecutive wage observations by the interview years, across workers of 20 and 54 years of age who reported at least one wage observation. Here, the cells in the diagonal indicate the maximum number of consecutive wage observations, while the rows depict the distribution of wage observations across the different interview years. For instance, the first cell on the diagonal shows the number of wage observations (*N*=1,097) that respondents between the ages of 20 and 54 years reported in two consecutive years (that is, 1985 and 1986). Thus, cases with only one wage observation in either year are excluded from this table. Next, in column 1988, we count 604 wage observations that are reported in two consecutive years, and 658 wage observations (out of the 1,097 initial cases) that are reported in three consecutive years (that is, 1985, 1986 and 1988). The number of consecutive wage observations drops with the elapsed time in the panel (with 46 consecutive wage observations across the 9 waves). This comes due to attrition that appears in studies that span over relatively longer periods and is common to longitudinal types of data. Next, in Table 1B of Appendix A, we examine whether the trend of unemployment in the OSA panel coincides with the more general, national trend of unemployment in the Netherlands over the years 1985-2000. Descriptive statistics reveal no systematic bias herein such that the share of the unemployed between 20 and 54 years of age in the OSA panel compares to the actual distribution of the same group of unemployed over the years 1985-2000 in the Netherlands.

To identify unemployment episodes in our data we have followed a two-step approach. First, we have used each respondent's reported labor force status at the date of interview, distinguishing between (1) employed, (2) self-employed, (3) unemployed, (4) non-participating, (5) in military service, and (6) in education. Unemployment is explicitly defined in the questionnaire as *"currently out of labor and searching actively for a job"*. Other forms of non-participation (e.g., maternity leave, homeworker) are explicitly excluded, both in the questionnaire formulation and in our design. In addition, we used respondents' reasons for the change of their labor force status to identify those with involuntary unemployment spells. Second, the OSA survey asks respondents to report the start and end dates of any change in labor force status that occurred between the current and last interview date. This retrospective information enables us to record all unemployment spells that occurred between two interview dates.

3.2. Measures

3.2.1. Dependent variable

In this study, the dependent variable is workers' *re-employment wage* that is defined as the log of net[2] hourly wages at time *t* for individual *i*, excluding overtime pay and overtime hours. This variable is constructed by dividing the monthly net wages[3] by the hours of work. Before taking the natural logarithm over the hourly re-employment wages, we harmonized the units of measurement by dividing hourly wages by the mean of hourly wages in each particular wave.

3.2.2. Unemployment Indicators

To test the expectations about the scarring effects of unemployment, we have constructed four indicators for unemployment, each of which has been empirically and theoretically shown to negatively influence workers re-employment wages. First, *unemployment occurrence* measured by constructing a lagged binary unemployment variable, which takes the value of 1 if the worker was unemployed in the previous wave and 0 if (s)he was continuously employed. Second, *unemployment duration in years* refers to the most recent unemployment spell and is measured by taking the difference between the start and end period of the most recent unemployment spell. Unemployment spells that are experienced before the observation period are not included in this measure, however, which is a drawback of the unbalanced character of our panel. According to our data, previously unemployed women experience on average slightly longer unemployment spells compared to previously unemployed men, with respectively 17.37 and 15.07 months. Third, *unemployment repetition* is measured by counting the number of unemployment episodes over the entire observation period. The variable distinguishes between three categories (1 = 2 previous unemployment spells; 2 = 3 previous unemployment spells; 3 = 4+ previous unemployment spells), with those in continuous employment as the reference category. Fourth, we have constructed a time-varying indicator for the *first unemployment*, which takes the value of 1 if a worker's unemployment spell in the previous wave was his or her first unemployment and 0 if a worker remained in continuous employment. We expect all these unemployment indicators to relate negatively to re-employment wages.

It is known that unemployment may reflect the quality of a worker's performance in the previous job, which in turn influences the level of wages upon re-employment (Moore 2010). Similar to Moore (2010) we minimize the threat of group differences in the quality of workers' performance, which in turn influences re-employment wages by controlling for the reasons of unemployment. We do so by including a dummy variable for *unemployed due to firm closings* (1 = yes; and 0 = otherwise) and the dummy for *unemployed for own motivations* (1 = yes; and 0 = otherwise). These variables were derived from respondents' reported reasons for labor force change that occurred either between or at the time of the interview.

Additionally, to capture the combined effects of these unemployment indicators, we have also constructed an '*unemployment index*'. To do so, we have first standardized the individual unemployment indicators (e.g., unemployment occurrence, unemployment duration, unemployment repetition and first unemployment) to have a mean of 0 and a variance of 1 (for men and women combined). We then averaged the <u>four</u> standardized unemployment indicators into a single summative[4] index, with the assumption that each of the standardized indicators contributes equally to the index. This index indicates the extent to which one has been unemployed. For the ease of interpretation, we rescale the index into a 0..1 variable such that 0 pertains to those in the reference base who have been in continuous employment and 1 refers to those with the highest extent of unemployment. The index measures changes in relation to this base. An unemployment index of 0.18, for example, means one has experienced an 18 percent increase in unemployment compared to those in the base. This is the average of the *standardized* indicators for previous unemployment occurrence, its duration, whether it was one's first or whether there were multiple unemployment occasions. Thus, one's unemployment index closer to the base (0) indicates shorter and less frequent unemployment spells in the past, and otherwise. In Figure 1A of Appendix A, the kernel distribution of the rescaled unemployment index (only for those who experienced unemployment) is shown for men and women separately. As expected, Figure 1A shows that in general women experience more frequent unemployment spells than men. In particular, there is a large share of women with a relatively low unemployment index suggesting interrupted careers of shorter spells or single unemployment spells. However, the share of men and women with a high unemployment index is also evident in our sample.

3.2.3. Human Capital Indicators

We capture the process of human capital depreciation in various ways. First, we construct a time-varying measure for *education* that captures the generic part of human capital by the highest achieved level of education at the time of interview. This variable distinguishes between: (1) elementary education [LO]; (2) lower intermediate education [VBO-MAVO]; (3) higher intermediate secondary education [MBO-HAVO-VWO]; (4) vocational college [HBO] and (5) university degree [WO]. However, given the small variation in workers' level of education at the time of interview, we can expect minimal effects of this variable on the re-employment wages.

Second, we construct three distinct variables to capture workers' loss of specific human capital. We include *tenure (in years)* with the former employer (which is based upon workers' reported start and end dates of employment with the previous employer); a time-varying dummy variable – based on the two-digit occupation codes according to the 1984 Standard Occupation Classification (SBC)[5] – for whether a respondent moved into the *changed occupation* (0 = no; and 1 = otherwise); and a time-varying dummy variable for whether the respondent *changed industries* (0 = no; and 1 = otherwise). In doing so, we capture the foregone skills with the previous firm, occupation and industries all of which should be negatively correlated with re-employment wages. These three indicators are indirect measures of human capital depreciation. However, within our data constraints they provide a unique opportunity to measure indirectly worker's human capital depreciation. *Age* in years is included as a proxy for work experience and the variable *age squared* is included to examine the curvilinear relationship between the accumulation of work experience and re-employment wages.

3.2.4. Stigma Indicators

To test hypotheses 2a to 2d that capture potential stigma effects we introduce four interaction terms that test these hypotheses respectively. These are between: (a) unemployment index and whether unemployment was experienced at old age (1 = equal or above 50 years; and 0 = otherwise); (b) unemployment index and whether or not children are present in the household (0 = no children; and 1 = co-residing children); (c) unemployment index and respondent's ethnicity (0 = country of origin, the Netherlands, (i.e., Dutch); and 1 = country of origin outside the Netherlands (i.e., Non-Dutch); and finally between; (d) the unemployment index and whether or not unemployment was experienced during tight labor market conditions (= 1 if unemployment rate at year of unemployment lies above the average unemployment rate over the period 1985-2000 and 0 if otherwise);

3.2.5. Demographic Measures, Job Characteristics, and Macro Characteristics

To control for demographic differences, that may influence the likelihood of unemployment and thereby subsequent re-employment wages, two measures were constructed. These are: *marital status* (0 = widowed/divorced; 1 = married/cohabiting; and 2 = single); *number of co-residing children* (ranging from 0 to 4+). We also control for various job-related characteristics that may influence respondents' (un)employment history and their re-employment wages (and thus act as confounding variables). These are the *number of working hours* (ranging between 12 and 40); *type of contract* (1 = permanent; and 0 = temporary contracts); the *level of occupational status* at the time of interview using the International Socio-Economic Index (ISEI) scale of Ganzeboom et al. (1992), and *sector* in which the job at time of interview is located (1 = public; and 0 = private). We expect to find a positive relationship between jobs with higher occupational status and permanent employment contract with re-employment wages (due to firms' investments and subsequent promotion opportunities within the firm). Given a strong collective bargaining history in the Netherlands together with the high share of union membership particularly concentrated in the public sector, we expect to find a positive relationship between jobs in the public sector and re-employment wages (due to a better protection of workers' rights in the work place). Finally, to check for business cycle variations, we include a variable indicating the *annual rate of unemployment* separately for men and women as reported by Statistics Netherlands (2010)[6].

3.3 Methods

To address scarring in terms of wage penalties, a log-wage linear regression panel model is fitted. We apply a fixed-effects model, which eliminates biases that occur by the failure to include controls for unmeasured personal characteristics such as motivation to work or ability to keep a job. In fixed-effects models, comparisons within individuals are conducted by (1) averaging at least two wage observations and by (2) modeling their deviations from this average. Since the unobserved heterogeneity in fixed-effects models is assumed to be time constant, any difference with its mean results in 0 and is dropped from the model. The model yields the following linear specification:

$$\ln w_{it} = \boldsymbol{\beta}'\mathbf{x}_{it} + \alpha_i + e_{it} \qquad (1)$$

where $\ln(w_{\mathrm{it}})$ is the natural logarithm of hourly wage at time t for individual i. $\boldsymbol{x}_{it}$ refers to a vector of observable variables on individual characteristics, **β'** refers to a transposed vector of coefficients associated with the observable characteristics. Finally, $\boldsymbol{\alpha}_i$ refers to the time-invariant individual specific errors that capture the unobserved heterogeneity while the e_{it} is the equation error term. In our study, wage equation (1) is extended to the following specification:

$$\ln w_{it} = \boldsymbol{\beta}'\mathbf{x}_{it} + \boldsymbol{\gamma}'\mathbf{u}_{i,t-1} + \alpha_i + e_{it} \qquad (2)$$

where **β'** includes a vector of the observable characteristics indicating younger and older workers' education, tenure with the previous employer, age, age squared, employed in the same occupation, industry changes, marital status, co-residing children, sector, type of contract, weekly working hours, unemployment due to firm closing, unemployment due to own motivations and unemployment rate. The value of $\mathbf{u}_{i,t\text{-}1}$ refers to the vector of unemployment dimensions such as unemployment occurrence, unemployment duration, first unemployment, and unemployment repetition (with dummies for 2, 3 and 4 or more previous unemployment spells), whereas **γ’** refers to a vector that captures the coefficients associated with each separate dimension of unemployment. Please note that in this equation we include dummy variables for unemployment repetition to assess the distribution of effects across multiple spells. Next, interaction effects are added to the model to examine our stigma-related hypotheses. The wage equation (2) therefore extends to the following specification:

$$\ln w_{it} = \boldsymbol{\beta}'\mathbf{x}_{it} + \boldsymbol{\gamma}'\mathbf{u}_{i,t-1} + \boldsymbol{\lambda}'(\mathbf{ux})_{i,t-1,t} + \alpha_i + e_{it} \qquad (3)$$

where, $\mathbf{ux}_{i,t\text{-}1}$ refers to the vector of interactions between the unemployment index and the disadvantageous micro- and macro-level labor market conditions (i.e., old age, parenthood, ethnicity, and economic downturn) with **λ’** as the pertinent vector of interaction terms with the subset of terms pertaining to the disadvantageous micro –and macro conditions. As mentioned above, our unemployment index averages out four unemployment indicators (e.g., unemployment occurrence, unemployment duration, first unemployment and unemployment repetition). In the index measure, unemployment repetition is included as a categorical variable.

4. Empirical Results

4.1 Testing for Human Capital Effects

A central expectation in our first hypothesis was that the indicators of unemployment, alone or in combination, would entail a negative effect on re-employment wages; an effect that should be higher among women than men. Figure 1 provides an initial clue about the wage differences across workers with and without unemployment in their careers. The plotted wage differentials indicate that unemployed workers (here not stratified by gender) not only have lower wages compared to their continuously employed companions, but that these wage differentials grow larger over time.

FIGURE 1 ABOUT HERE

We next explore whether these results persist once we control for human capital, demographic, as well as job and macro characteristics. Table 1 presents results from four fixed-effect regression models that test for human capital depreciation effects. Models 1 and 2 test our first hypothesis followed by an additional column (Δ) which tests for gender specific differences by combining the sample of men and women and by estimating a full interaction model between gender and all the covariates used in Model 1. In Models 3 and 4, we include the unemployment index to measure the combined effect of unemployment on workers' re-employment wages. Also here, the last column tests for gender differences within our estimates. We interpret our estimates in terms of percentages by taking the antilog of the estimated coefficients minus 1, namely: $(\exp^{coef}) -1$.

TABLE 1

Results from the models provide two key findings that partially corroborate with the first hypothesis. First, we find that the separate indicators of unemployment influence both men and women negatively, and that their relative importance on re-employment wages varies across gender. For instance, Model 1 in Table 2 indicates that for women, the *first unemployment spell* inflicts the deepest wage scar with nearly 17 percent (β = -0.156, t =2.82) relative to women in continuous employment. In addition, women who experienced unemployment more recently (i.e., in the previous wave) earn on average 7.5 percent lower wages per hour (β = -0.073, t = 1.80). We find no evidence of a growing wage gap with more frequent or longer spells of unemployment among women. For men we find that it is particularly the *repetition of unemployment* that causes the highest scarring effects. Specifically, a first unemployment spell produces a wage setback of 10 percent (β = -0.097, t = 3.81). This penalty is lower and not significant with two (β = -0.047, t = 1.41) and three (β = -0.035, t = 0.47) previous unemployment spells, but becomes significantly larger with four or more spells of previous unemployment (β = -0.138, t = 2.82). In addition, the length of unemployment also comes with a re-employment wage penalty. Specifically, for each additional year in unemployment, men suffer about 2.5 percent (β = -0.025, t =2.47) lower hourly re-employment wages relative to men who remain in continuous employment; a penalty that differs also significantly between men and women. In addition, analyses reveal a U-shape pattern in scarring that pertains to the dimension of unemployment repetition.

Second, we find that the combined magnitude of unemployment scarring is similar across men and women. Specifically, the combined dimensions of unemployment into the (standardized) unemployment index in Model 3 indicate a 'total' wage setback from unemployment of about 15 percent among women (β = -0.141, t = 1.80) and in Model 4 of about 18 percent among men (β = -0.173, t = 3.02). At this point, our expectation from the first hypothesis – that women should experience higher scarring effects – is not confirmed. The weak effect among women, suggests that the deep wage scar that women experience after their first unemployment spell does not accumulate with longer durations or more frequent repetition of subsequent unemployment spells. The fact that unemployment scarring among women almost disappears after controlling for human capital, demographic and job characteristics implies that scarring effects are largely short-lived among women and more persistent among men.

It is important to note that most of the human capital, demographic, job –and macro indicators in Model 1 thru 4 influence re-employment wages in the expected direction. As expected, we find that (non)transferability of specific skills influences significantly the level of subsequent wages. For instance, particularly among men, skills tied to the previous employer produce a wage penalty of about 6 percent for each year of lost tenure (β = 0.006, t = 2.99). However, those who *change* occupations suffer between 2.5 and 3 percent lower re-employment wages, depending on one's gender, compared to those who do otherwise. Specifically, the magnitude of this effect is slightly higher among women (β = -0.030, t = 1.82) but the significance is stronger among men (β = -0.024, t = 2.41). Loss of industry-specific knowledge also comes with a re-employment wage penalty but only amongst previously unemployed men (β = -0.055, t = 1.61).

Interestingly, our results reveal a parenthood wage penalty such that women with co-residing children experience a wage penalty of about 2.3 percent, while men gain about 0.07 percent for each co-residing child. These results are in line with findings from earlier studies that find profound evidence of a parenthood wage penalty in the United States (Correll et al. 2007; Budig and Hodges 2010). Apparently, such penalties are not specific to the United States but also evident in the Netherlands. Finally, working hours and the type of contract influence significantly men's and women's re-employment wages. Specifically, the positive coefficient pertaining to the type of employment contract is likely to reflect the gains (due to firm's investment and promotion opportunities) that are related to a stable working career and support earlier empirical evidence found in the Netherlands (Mooi-Reci and Dekker 2013). In addition, the reason of unemployment shows a weak and negative effect particularly on women's log of hourly wages. Interestingly, although not significant, men and women who experience unemployment due to firm closings earn relatively higher wages than those who lose their jobs due to own motivations. These differences may hide potential discriminatory practices when employers make hiring decisions and could be addressed in more detail in future research.

In sum, both men and women are influenced negatively by unemployment. However, there is a notable disparity in the unemployment scarring by gender. While the wage setback following unemployment almost diminishes among women after controlling for the foregone human capital with regard to changing occupations and temporary employment contracts, among men the wage gap remains largely unexplained. The stronger effects of unemployment scarring among men suggest that men are likely influenced through more channels than women.

4.2 Testing for Variations in Unemployment Stigma

Table 2 estimates equation (3), which introduces 4 interaction terms to test for stigma effects (stigma hypotheses 2a thru 2d). The inclusion of the four interaction terms separately for women and men – along with controls for human capital and demographic differences – allows us to assess any unemployment scarring and gender disparities across specific groups and economic contexts.

The major finding from these models is that effects of unemployment scarring are not universal, but vary upon context, namely: its effects are more profound (i) at older ages (50 years and older), (ii) during economic downturns and (iii) among immigrant workers; effects that exist *solely* among men. To be more specific, we start with the interpretation of the results in Model 2 that apply to men. We find that, wage penalties exacerbate with 23 percent if men experience unemployment during tight economic conditions (β = -0.208, t = 1.97). The exacerbation of wage penalties is almost similar in size and strength (β = -0.219, t = 2.00) among workers who experience their first unemployment at older ages (50 years and older) and somewhat smaller and weaker among immigrant workers (β = -0.116, t = 1.84). Interestingly, for women, all the different interaction terms are not significant, suggesting that unemployment wage penalties among women are not skewed along lines of parenthood, age, ethnicity, or labor market conditions. Overall, the systematic contextual variation of unemployment scarring among men reveals a gendered pattern of scarring that, most likely, arises through stigma. Specifically, employers' hiring decisions seem to be highly contingent upon age, ethnicity, and economic conditions, but only among men. These results lend support to our expectation that stigma effects are more prominent among men than women, at least, in the Dutch context.

TABLE 2

Finally, to test whether the panel attrition/selection in a different number of wage observations is at random, we perform a two-step ordered probit Heckman selection model as suggested by Chiburis and Lokshin (2007)[7]. The two-step procedure is constructed as follows. Through an ordered probit model, the first step (e.g., the selection model) estimates the probability of selecting those having 1+ wage observations versus 0 wage observations, 2+ wage observations versus 1 wage observation, 3+ wage observations versus 2- wage observations, etc. up to 6+ wage observations conditional on a series of individual level characteristics. Three instrumental variables were used for the identification of the selection equation: *the unemployment rate at the time of the first spell of unemployment* (i.e., this reflects the effect on subsequent labor market participation but does not directly affect workers' wages); *marital status* and whether or not the respondent had *co-residing children*. These two latter variables are commonly used in the (economic) literature as instrumental variables (see Chiburis and Lokshin 2007; Gregory and Jukes 2001; Arulampalam 2001). The second step (e.g., the outcome model), includes the series of inverse Mills ratios (the ρ's). Through a Wald test, such an application assesses whether the selection of varying number of observable wage observations is generated at random. For both women and men in our sample, the Wald test strongly rejects the null hypothesis. This indicates that sample selection is *not* generated at random. We continued with the estimation of the two-step ordered probit Heckman selection model using the ordinal selection variable in the first step. We then used the estimations from the first step to predict an *overall* Mills ratio in the second step. In a final step, we added the predicted Mills ratio as an additional regressor in our fixed effect estimations. Results are presented in Table 3. Specifically, Models 1 and 2 display results from those presented earlier in Table 1, which control for various individual and job characteristics. Models 3 and 4 display results from the fixed effect regression model that correct for the selectivity of our sample by applying a two-step ordered probit Heckman correction procedure as proposed by Chiburis and Lokshin (2007).

TABLE 3

Results from Table 3 provide interesting information regarding the sensitivity of our estimations. Specifically, estimates from the standard fixed effect regression (Models 1 and 2) differ especially with regard to their magnitude from the estimates that follow from the ordered probit Heckman selection procedure (Models 3 and 4). For women, we find that the (weak) effect of unemployment occurrence at the previous wave (t_{-1}) disappears while the effect of the first unemployment spell on wages becomes slightly higher (β = -0.173, t = 2.94) in the Heckman Model 3. Among men we find that the effects of unemployment duration, unemployment repetition and the first unemployment are slightly higher in magnitude and strength in the ordered Heckman procedure in Model 4. Overall this means that our standard fixed effect models may have *under*estimated the effects of scarring among men and women and that we should present our results with caution.

5. Conclusion and Discussion

The goal of this study was to investigate how wage setbacks following previous unemployment (i.e., unemployment scarring) and its underlying mechanisms operate across gender in the Netherlands over the period 1985-2000. We argued that the singular focus of existing literature on the scarring effects among men has left us with unanswered questions regarding unemployment scarring by gender across specific social groups and in different economic contexts. From the human capital theory, a central hypothesis was derived which predicted that unemployment scarring effects can be attributed to human capital depreciation (i.e., limited work experience and out-dated skills and knowledge), which within the context of the Netherlands, should be more prominent among previously unemployed women and less severe among men. Conversely, from stigma theory, contextual hypotheses maintained that disparity in unemployment scarring should be more severe among men and contingent on individual and economic level variation. Following Omori's approach (1997), we argued that if stigma drives unemployment then scarring effects should exacerbate in (tight) labor market situations and among specific (disadvantaged) groups (e.g., age, parenthood, and ethnicity). By contrast, little or no contextual variation would indicate that human capital depreciation effects dominate. Longitudinal data from the Netherlands Labor Supply Panel (OSA) over the period 1985-2000 were used to test these hypotheses.

Using fixed effects panel models our analyses reveal three central findings. A first key finding was that unemployment inflicts significant wage losses among both men and women. We show that amongst women, unemployment scarring is highest after the first unemployment instance and less pronounced in the subsequent interruptions while for men scarring varies over the course of unemployment repetitions and its duration. An explanation for the distinct pattern of scarring among women may relate to the fact that women are more likely to change or switch into more 'motherhood-friendly' sectors after the first job interruption (Aisenbrey et al. 2009; Datta Gupta and Smith 2002; Engelbrech 1997). Consequently, the costs of foregone skills and benefits are highest after the first job interruption and less pronounced in the subsequent interruptions when women are more established in the 'motherhood-friendly' sectors. Interestingly, our results suggest that scarring effects among women do not accumulate with the duration and repetition of unemployment while this is true among men. Our results provide new clues about the gendered patterns of unemployment scarring and lend support to recent approaches that call for the inclusion of women in the unemployment scarring analyses (Kalil and Ziol-Guest 2008).

A second major finding is that the mechanisms underlying unemployment scarring vary by gender. Specifically, the weaker effects of unemployment scarring among women indicate that scarring mainly relates to the loss of occupation specific knowledge and to the temporary character of women's employment contracts. Re-employment wages of previously unemployed men are also influenced by human capital depreciation. However, the stronger remaining scarring effects and the wide contextual variation across men of different social groups and in different economic conditions suggest that scarring effects among men are distributed through more channels.

Third and finally, we find that discriminatory practices in hiring are more likely to occur (i) among older male workers, (ii) during tight economic conditions or (iii) when originating from a country outside the Netherlands. An explanation may be the fact that changing economic prospects alter the way in which employers make their hiring decisions. Increasing costs (of retraining) of older workers and the ambiguity of the productivity of foreign workers may become a culprit to hire workers that belong in one of these groups. This finding advances existing literature (Blanchard and Diamond 1994; Omori 1997), by showing that scarring is conditional upon gender, age and ethnicity. At the same time, our findings lend support to recent studies that find substantial wage losses among (older) men who lost their jobs during the current economic crisis in the United States (Farber 2011).

What can we learn from the findings of this particular study? In this article, we have shown that it is fruitful to study the effects of different unemployment dimensions simultaneously. The different dimensions of unemployment are measured in different units of analyses and differ considerably between workers. This study shows that creating an unemployment index is a useful tool to measure the 'total' effects of unemployment, and in doing so, calls into question the reported magnitude of unemployment scarring which might have been underestimated in existing literature. Our results also suggest that loss of specific skills that are tied to the previous firm or occupation lead to serious wage setbacks upon re-employment. This implies that mismatching between former and current occupations may be crucial, yet it remains an overlooked area in existing scarring research. Additional research is needed that reveals wage differences resulting from mismatching and differences in workers' search behavior. In addition, our study assumed that wage disadvantage related to ethnicity, was the product of stigma. However, as shown by Moore (2010), lack of social networks and less effective search strategies may be an alternative explanation for this wage gap that should be investigated with greater rigor in future research. Finally, while our results suggest that unemployment scarring arises mainly through human capital depreciation among women, it should be emphasized that for specific groups of women – such as for those who have attained a lower educational level or those in the lower wage quintiles – stigma effects may play a role as well. More research is needed to explore these differences and understand when human capital depreciation effects dominate the stigma effects and for which groups.

While our study advances current knowledge on unemployment scarring, it has been challenged by various drawbacks that should be taken into account when interpreting our results. First, the share of panel attrition raised questions about the validity of our estimations due to sample selectivity. We tested for non-randomness of sample selection by conducting a two-step ordered probit Heckman procedure that tested for multiple response categories simultaneously. Although sample selectivity was not generated at random, results from the sensitivity analyses showed that our fixed effect estimates may have underestimated the presented scarring effects among men and women. Second, given the nature of unemployment, our sample of previously unemployed workers could be governed by unobserved characteristics, which influence both the likelihood of experiencing unemployment and the level of wages upon re-employment. By differentiating between the different reasons for unemployment, our study tried to account for characteristics that may reflect the quality of workers' performance in the previous job and which, in turn, may lead to group differences in pay. Third, in addition to the information at the time of interview, we used respondents' retrospective information to reconstruct (un)employment histories—information which may be hampered by recall errors. By using the reported unemployment as a count (in our repetition variable) and duration data (the unemployment spell variable) we reconstructed unemployment histories that are less sensitive to recall bias and have more acceptable levels of reliability (Pina-Sanchéz 2012). However, information from register data could provide a better alternative to avoid under-reporting of (short) unemployment spells. Finally, the lower internal consistency of the unemployment index reflects the potential multidimensionality of unemployment. While this was the only way to efficiently test the stigma related hypotheses, future studies following our approach should be aware of this limitation.

Our findings also provide directions for future policies. For instance, in our study we show that the best way to prevent unemployment scarring is to avoid falling into unemployment to begin with. In addition, our results in favor of stigma during economic downturns ask for more attention from governments to design policies that protect workers from becoming unemployed (i.e., through wage subsidies or employment programs) during economic crises. Such measures would not only stimulate employers to hire sooner those once unemployed, but would also raise worker's self-esteem and readiness to accept a job (sooner).

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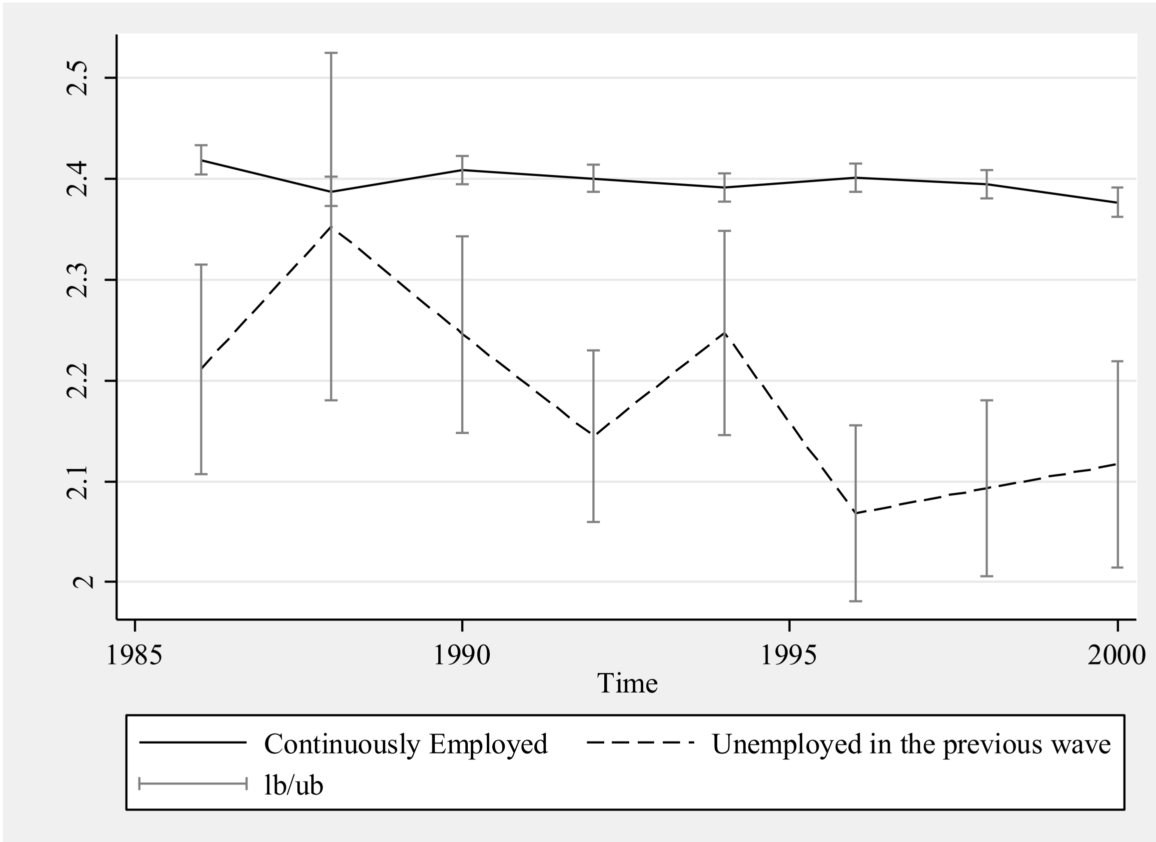
FIGURE 1
Mean of Log Hourly Wages by Employment Status



SOURCE: Data are from the OSA Supply Panels, 1985-2000, Netherlands.

TABLE 1
Unstandardized Coefficients for the Human Capital Effect on Subsequent Log of Hourly Wages, from Fixed- Effects Models by Gender, The Netherlands 1980-2000

	Female	*Male*	Δ	*Female*	*Male*	Δ
	(1)	(2)		(3)	(4)	
Unemployment Indicators						
Unemployment occurrence (t_{-1})	-0.073*	0.000		-	-	
	(1.80)	(0.02)				
Unemployment duration, years (t_{-1})	-0.002	-0.025**		-	-	
	(0.21)	(2.47)				
First unemployment	-0.156***	-0.097***		-	-	
	(2.82)	(3.81)				
2 previous unemployment spells	0.005	-0.047		-	-	
	(0.07)	(1.41)				
3 previous unemployment spells	-0.015	-0.035		-	-	
	(0.15)	(0.47)				
4+ previous unemployment spells	-0.114	-0.138***		-	-	
	(0.87)	(2.82)				
(Standardized) Unemployment index	-	-		-0.141*	-0.173***	
				(1.80)	(3.02)	
Human Capital Indicators						
Education level	0.028	-0.007		0.025	-0.009	
	(1.18)	(0.66)		(1.09)	(0.77)	
Tenure with previous employer (years)	-0.007	-0.006***		-0.005	-0.007***	
	(1.22)	(2.99)		(0.88)	(3.17)	
Changed occupation (2-digit)	-0.030*	-0.024**		-0.033**	-0.021**	
	(1.82)	(2.41)		(2.07)	(2.15)	

TABLE 1. Continued

	Female	*Male*	Δ	*Female*	*Male*	Δ
	(1)	(2)		(3)	(4)	
Changed industries (1-digit)	-0.047	-0.055**		-0.061	-0.050*	
	(0.97)	(1.61)		(1.33)	(1.83)	
Age (years)	0.008	0.033***		0.007	0.032***	
	(0.84)	(7.28)		(0.71)	(7.08)	
Age squared	-0.000	-0.000***		-0.000	-0.000***	
	(0.77)	(7.09)		(0.67)	(6.97)	
Demographic, Job –and Macro Indicators						
Marital status	-0.055*	-0.028*		-0.058*	-0.029*	
	(1.73)	(1.72)		(1.83)	(1.78)	
Non-Dutch	-0.024	-0.018		-0.027	-0.018	
	(1.15)	(1.60)		(1.29)	(1.61)	
Co-residing children	-0.023**	0.007*	***	-0.023**	0.008*	***
	(2.38)	(1.77)		(2.33)	(1.89)	
Working hours	-0.014***	-0.019***	***	-0.013***	-0.019***	***
	(15.25)	(23.30)		(15.09)	(23.21)	
Sector (= public)	0.012	-0.003		0.014	-0.005	
	(0.70)	(0.30)		(0.82)	(0.47)	
Type of contract (= permanent)	0.041*	0.013		0.042**	0.015	
	(1.95)	(1.18)		(1.97)	(1.33)	
Occupation level (ISEI codes)	0.001***	0.001***		0.001***	0.001***	
	(3.69)	(8.13)		(3.66)	(8.19)	
Unemployed due to firm closings	0.034	0.011		0.036	0.012	
	(1.26)	(0.96)		(1.31)	(1.02)	

TABLE 1. Continued

	Female	*Male*	Δ	*Female*	*Male*	Δ
	(1)	(2)		(3)	(4)	
Unemployed due to own motivations	-0.066*	0.001	*	-0.067*	0.006	*
	(1.78)	(0.07)		(1.81)	(0.30)	
Unemployment rate at interview date	-0.004	-0.007***		-0.004	-0.007***	
	(0.96)	(3.24)		(0.98)	(3.34)	
Constant	2.473***	2.613***		2.486***	2.629***	
	(11.12)	(26.28)		(11.23)	(26.57)	
Observations	3,995	6,892		3,995	6,892	
Number of Respondents	1,913	2,901		1,913	2,901	
R-squared (within)	0.128	0.180		0.124	0.177	

SOURCE: - Author’s calculations, using data from the OSA Supply Panels, 1985-2000.
NOTE: The dependent variable is the *log of hourly wages*; - Absolute value of *t*-statistics in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE 2

Unstandardized Coefficients for the Stigma Effect of Unemployment on Subsequent Log of Hourly Wages, from Fixed- Effects Models by Gender, The Netherlands 1980-2000

	Female	*Male*
	(1)	(2)
Main Effects		
Unemployment index	0.117	0.116
	(0.50)	(1.03)
Unemployed ≥ 50 years	-0.048	0.055*
	(0.79)	(1.76)
Co-residing children	-0.035*	0.004
	(1.83)	(0.42)
Non-Dutch	0.002	0.016
	(0.04)	(0.48)
High unemployment at year of unemployment	0.020	0.021
	(0.58)	(0.97)
Interaction (Stigma) Indicators		
Unemployed ≥ 50 years × Unemployment index	0.036	-0.219**
	(0.21)	(2.00)
Co-residing children × Unemployment index	-0.139	0.086
	(0.93)	(0.95)
Non-Dutch × Unemployment index	-0.197	-0.116*
	(1.61)	(1.84)
High unemployment ×Unemployment index	-0.188	-0.208**
	(1.05)	(1.97)
Human Capital Indicators		
Education level	0.017	-0.013
	(0.77)	(1.15)
Tenure with previous employer (years)	-0.003	-0.008***
	(0.65)	(3.77)
Changed occupation (2-digit)	-0.037**	-0.025**
	(2.32)	(2.54)
Changed industries (1-digit)	-0.104*	-0.056*
	(1.87)	(1.78)
Age (years)	0.004	0.034***
	(0.47)	(7.74)
Age squared	-0.000	-0.000***
	(0.28)	(7.35)

TABLE 2. Continued

	Female	*Male*
	(1)	(2)
Demographic, Job, and Macro Indicators		
Marital status	-0.061*	-0.032*
	(1.89)	(1.86)
Working hours	-0.013***	-0.019***
	(14.73)	(23.78)
Sector (= public)	0.017	-0.004
	(1.01)	(0.44)
Type of contract (= permanent)	0.046**	0.013
	(2.17)	(1.12)
Occupation level (ISEI codes)	0.001***	0.001***
	(3.73)	(7.79)
Unemployed due to firm closings	0.035	0.012
	(1.28)	(0.94)
Unemployed due to own motivations	-0.072*	-0.002
	(1.92)	(0.09)
Constant	2.537***	2.489***
	(12.46)	(24.85)
Observations	3,995	6,892
Number	1,913	2,901
R-Squared	0.123	0.179

SOURCE: - Authors' calculations, using data from the OSA Supply Panels, 1985-2000.
NOTE: The dependent variable is the *log of hourly wages*. All the models control for human capital measures as well as demographic, job, and macro variables. – Absolute value of t statistics in parentheses. *significant at 10%; ** significant at 5%; *** significant at 1%.

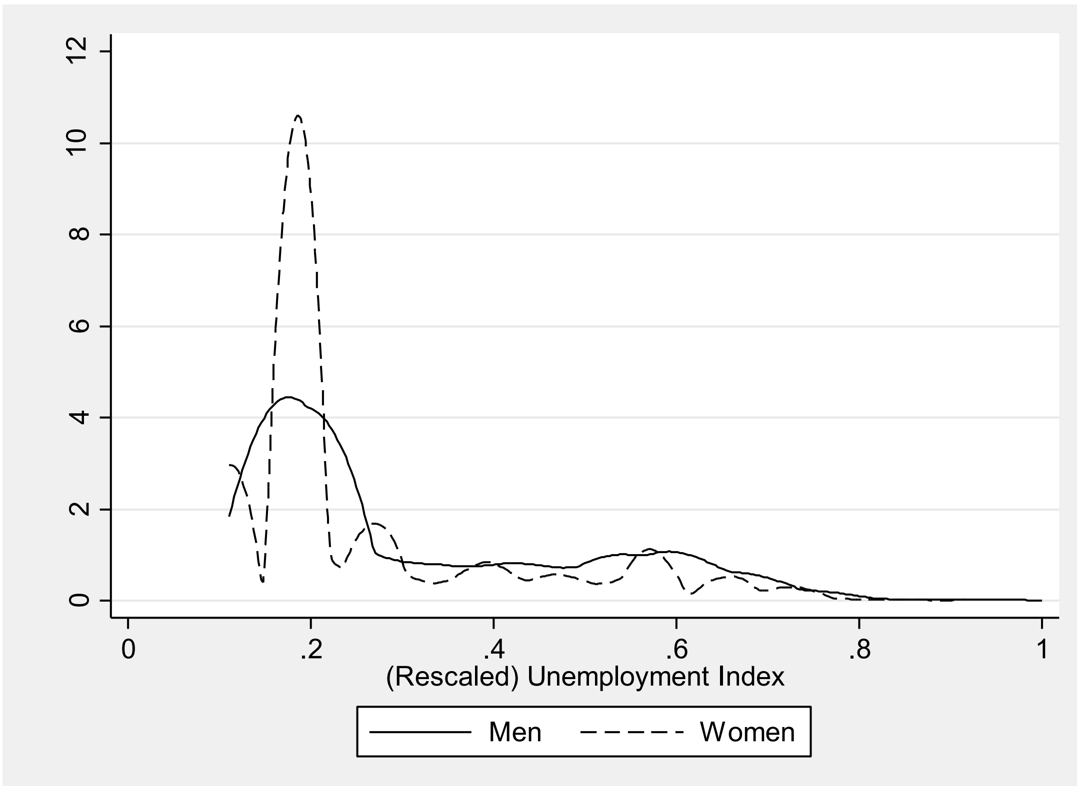
TABLE 3
Sensitivity Analyses

	Fixed Effects Regression		*Fixed Effects combined with Ordered Heckman*	
	Female	Male	Female	Male
	(1)	(2)	(3)	(4)
Unemployment occurrence (T-1)	-0.073*	0.000	0.021	-0.093
	(1.80)	(0.02)	(0.18)	(1.62)
Unemployment duration, years (T-1)	-0.002	-0.025**	0.012	-0.037***
	(0.21)	(2.47)	(0.62)	(3.09)
First unemployment	-0.156***	-0.097***	-0.173***	-0.081***
	(2.82)	(3.81)	(2.94)	(2.92)
2 previous unemployment spells	0.005	-0.047	0.014	-0.065*
	(0.07)	(1.41)	(0.21)	(1.95)
3 previous unemployment spells	-0.015	-0.035	-0.001	-0.037
	(0.15)	(0.47)	(0.02)	(0.50)
4+ previous unemployment spells	-0.114	-0.138***	-0.080	-0.161***
	(0.87)	(2.82)	(0.61)	(3.28)
Mills ratio			0.099	-0.095*
			(0.85)	(1.79)
Constant	2.473***	2.613***	2.482***	2.522***
	(11.12)	(26.28)	(12.64)	(26.16)
Observations	3,995	6,892	3,995	6,898
Number of Respondents	1,913	2,901	1,913	2,901

SOURCE: - Authors' calculations, using data from the OSA Supply Panels, 1985-2000.
NOTE: The dependent variable is the *log of hourly wages*. All the models control for human capital measures as well as demographic, job, and macro variables. – Absolute value of t statistics in parentheses. *significant at 10%; ** significant at 5%; *** significant at 1%.

APPENDIX A

Figure 1A: The Kernel Distribution of the Rescaled (0/1) Unemployment Scale



SOURCE: Data are from the OSA Supply Panels, 1985-2000, Netherlands.

TABLE 1A:
Workers' Wage Observations across the Nine Waves

The # of Wage Observation	Waves								
	1986	*1988*	*1990*	*1992*	*1994*	*1996*	*1998*	*2000*	*Total*
2	4,059	281	206	95	64	33	22	6	4,766
3	0	2,206	154	126	49	35	17	7	2,594
4	0	0	1,305	90	88	30	15	4	1,532
5	0	0	0	818	47	52	16	3	936
6	0	0	0	0	484	21	24	8	537
7	0	0	0	0	0	283	10	5	298
8	0	0	0	0	0	0	111	1	112
9	0	0	0	0	0	0	0	46	46
N	*4,059*	*2,487*	*1,665*	*1,129*	*732*	*454*	*215*	*80*	*10,821*

SOURCE: Data are from the OSA Supply Panels, 1985-2000, Netherlands.

TABLE 1B:
% Unemployed Men and Women aged between 15-54 years across the OSA panel and Bureau of Statistics Netherlands, 1985-2000

Years	**OSA %**	**CBS %**
1985	9.1	9.6
1986	7.3	8.6
1988	8.1	8.3
1990	5.6	6.9
1992	5.0	6.5
1994	6.1	8.5
1996	5.7	7.5
1998	3.7	5.1
2000	2.7	3.8

SOURCE: Data are from the OSA Supply Panels, 1985-2000, Netherlands and from the Statistics Netherlands[8].

TABLE 2A:
Means, standard deviations (SD) of Workers Aged between 21 and 54 years, by Gender

	Male Workers		Female Workers	
	Mean	*SD*	*Mean*	*SD*
Dependent measure				
Hourly wages (in Dutch guilders, absolute value)	17.65	7.15	14.72	8.08
Log of hourly wages (deflated)	2.48	0.30	2.28	0.33
Unemployment dimensions				
Unemployment occurrence (T-1)	0.01	0.10	0.02	0.14
Unemployment duration, years (T-1)	0.02	0.24	0.08	0.48
First unemployment (T-1)	0.07	0.26	0.11	0.31
2 previous unemployment spells	0.02	0.14	0.03	0.17
3 previous unemployment spells	0.01	0.07	0.01	0.10
4+ previous unemployment spells	0.01	0.08	0.01	0.07
Unemployment index	0.02	0.08	0.04	0.11
Human Capital Measures				
Elementary education	0.06	0.24	0.04	0.21
Lower intermediate education	0.34	0.47	0.33	0.47
Higher intermediate secondary education	0.36	0.48	0.40	0.49
Vocational college	0.17	0.38	0.19	0.39
University degree	0.05	0.22	0.03	0.17
Tenure with previous employer (years)	2.59	1.44	2.28	1.31
Changed occupation	0.09	0.29	0.17	0.38
Changed industries	0.01	0.10	0.01	0.12
Age (years)	37.7	8.88	36.1	9.17
Demographic, job, and macro variables				
# Co-residing children	1.20	1.21	0.99	1.11
Marital status	0.77	0.41	0.74	0.43
Non-Dutch	0.09	0.29	0.10	0.30
Working hours	37.5	4.09	25.5	11.3
Sector (= public)	0.23	0.42	0.26	0.43
Type of contract (1= permanent)	0.80	0.39	0.72	0.43
Unemployed due to firm closings	0.03	0.18	0.02	0.17
Unemployed due to own motivations	0.03	0.17	0.05	0.22
ISEI status	43.0	19.9	40.7	20.2
Macro variables				
Unemployment rate	5.38	1.16	10.03	1.99
Number of observations	6,798		4,023	
Number of workers	2,821		1,908	

[1] http://statline.cbs.nl/StatWeb/publication/?DM=SLNL&PA=70787ned&D1=a&D2=0&D3=0&D4=a&D5=0,8,18,28,l&HDR=T,G4&STB=G1,G2,G3&VW=T

[2] Income tax is levied on an individual basis in the Netherlands, meaning that wage effects are unlikely to conflate with the scar that arises through changing household circumstances.

[3] Gross wages have not been asked for consistently throughout the different waves in the OSA panel. For this reason we have chosen the net monthly wages as our primary wage variable for the analyses.

[4] The four indicator variables are highly interrelated. Constructing an index allows us to avoid multi-co linearity problems in interaction models. However, the four indicators all tap independently collected information, and averaging them also redresses random measurement error.

[5] Follow the link for more details on the classification: http://www.cbs.nl/nl-NL/menu/methoden/classificaties/overzicht/sbc/2010/default.htm

[6] http://statline.cbs.nl/StatWeb/selection/default.aspx?DM=SLNL&PA=80718NED&VW=T

[7] We thank an anonymous reviewer for this suggestion.

[8] http://statline.cbs.nl/StatWeb/publication/?DM=SLNL&PA=71761ned&D1=0,3,7&D2=0&D3=1,3,5,7,9,11,13,l&HDR=T&STB=G1,G2&VW=T